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Real consequences matters: why hypothetical biases in the valuation of time persist even in controlled lab experiments

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Real consequences matters: why hypothetical biases in the valuation of time persist even in controlled lab experiments

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Abstract

In a controlled lab experiment, we investigate hypothetical biases in the value of time by comparing stated preference (SP) and revealed preference (RP) values attached to unexpected waiting times. The SP and RP choice sets are identical in terms of design with the only difference being that the RP choices have real consequences in terms of unexpected waiting times and monetary incentives. We find a substantial hypothetical bias with the average SP value of time being only 71% of the corresponding RP value. The bias is mainly driven by participants who have scheduling constraints during the time of the unexpected wait. Scheduling constraints are taken into account to a much lesser extent in the SP setting than in the RP setting, presumably because only in the latter, the consequences of ignoring them are costly. We find evidence that this effect is stronger for persons with relatively low cognitive ability.

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1. Introduction

The value of (travel) time (VOT) is an essential input for transport models and appraisals of transport investments and policies, and therefore is one of the most important parameters in transport economics (e.g. Small, 2012). Numerous studies have been undertaken to elicit the VOT from hypothetical or actual choice situations, resulting in stated preference (SP) and revealed preference (RP) data, respectively. Insightful meta-studies on the valuation of (travel) time can for instance be found in Abrantes and Wardman (2011) (for Great Britain) and Shires and De Jong (2009). Results of recent national VOT studies are discussed in Axhausen et al. (2014) (Germany), Börjesson and Eliasson (2014) (Sweden), Kouwenhoven et al. (2014) (Netherlands), and Batley et al. (2017) (Great Britain).

Existing research on the VOT reveals systematic differences in the value of time with respect to transport modes, trip purposes, socio-economic characteristics, short-run vs. long-run decision time frame (e.g. Wardman, 2004; Peer et al., 2015). Also the underlying data source (SP vs. RP data) seems to affect the value of time significantly (e.g. Ghosh, 2001; Hensher, 2001; Brownstone and Small, 2005; Small et al., 2005; Isacsson, 2007; Peer et al., 2014; Hultkrantz and Savsin, 2018): most studies find that the value elicited from SP data is substantially lower than the value elicited from RP studies. This gap is usually referred to as hypothetical bias.

This study aims at investigating whether hypothetical biases in the value of time disappear if RP and SP choice settings are identical and only differ according to whether the choices have real-life consequences in terms of monetary incentives and unexpected waiting times (RP) or not (SP). We use this controlled environment to test existing hypotheses related to the origin of hypothetical biases. Unlike the recent study by Hultkrantz and Savsin (2018), which has a similar scope, we focus on unexpected waiting times that are long enough to interfere with scheduling constraints (15-90 minutes); in the experiments conducted by Hultkrantz and Savsin (2018), the unexpected time (to be spent on completing a task) was only 15 minutes. We will show that this is an important difference, as scheduling constraints play a crucial role in the formation of hypothetical biases in the valuation of time.

SP data generally have the advantage over RP data that their design is being fully controlled by the analyst, implying that the definition of alternatives and their attributes is unambiguous, and that collinearity between attribute levels can be avoided. This is not always the case for RP data, where the attribute levels may be correlated (affecting estimation accuracy negatively), and the definition of the choice set and attribute levels may not be unambiguous. For instance, Peer et al. (2013) show that the VOT may be structurally biased depending on the assumptions made concerning travel time expectations.[1]

While SP data are typically easier and less costly to collect, and can capture non-existent alternatives, they are at risk of yielding biased estimates, due to persons acting differently in hypothetical choice settings compared to actual, real-life settings. This divergence has been shown in a wide number of contexts, but seems to be especially persistent in the context of the valuation of public goods (often based on contingent valuation methods), where valuations elicited in hypothetical settings tend to be substantially higher than the corresponding valuations elicited in real-life settings (see for instance the meta-analyses by List and Gallet, 2001; Little et al., 2004; Murphy et al., 2005a).

[1]Several studies have also advised against the common use of reported travel times for the definition of travel time attributes in RP studies due to severe biases in the reporting of travel times (e.g. Van Exel and Rietveld, 2009; Peer et al., 2014).

Several reasons have been brought forwards as an explanation for the existence of hypothetical biases. The most obvious one is the lack of real consequences in hypothetical choice settings (Murphy and Stevens, 2004), which may lead to a gap between people's intentions (SP) and behaviors (RP) (Ajzen and Fishbein, 2000). Recent papers that investigate neural processes that can be associated with decision-making support the notion that there are differences between choices with real vs. hypothetical consequences in terms of the brain areas being active (Kang et al., 2011; Camerer and Mobbs, 2017). A possible explanation for these differences is that individuals try to minimize cognitive effort exerted on tasks (e.g. Walton et al., 2006; Kool et al., 2010; Shenhav et al., 2017), since cognitive effort is costly and has an opportunity cost (Kurzban et al., 2013; Westbrook and Braver, 2015). In hypothetical settings, the costs of not exerting any effort are (close to) zero, as there are no real-life consequences of the decisions taken.

Besides the absence of real consequences, also strategic answering has been brought forward as a potential reason for hypothetical biases, in particular when the choices concern the provision of public goods (e.g. Carson and Groves, 2007). Uncertainty regarding the choice (and hence the value of the underlying good) may play a role as well (e.g. Champ and Bishop, 2001). These latter two reasons (i.e., strategic answers and uncertainty) are, however, likely to play only a minor role in the context of VOT elicitation: first, is usually unknown to the participants of an SP survey what the time valuation will be used for (and hence there is no obvious case for strategic answers); second, the trade-off between time and money should be fairly familiar to most persons, as related decisions are present in a variety of contexts (choosing between transport modes, choosing the number of working hours, etc.).

Various studies have argued that hypothetical biases are (more) likely to occur if the hypothetical setting diverges strongly from real-life choice situations that are familiar to respondents (e.g. List and Gallet, 2001; Hensher, 2010; Schläpfer and Fischhoff, 2012). In a stated choice context, this holds for both the framing of the SP experiment and its design in terms of alternatives and attributes. Regarding the design of SP experiments, attribute values pivoted around the respondents' real-life reference situation have been suggested as a way to reduce hypothetical biases, and have been implemented in numerous applications (Hensher, 2010; Li et al., 2018). However, a recent study by Hultkrantz and Savsin (2018), which also concerns VOT elicitation, provides some evidence that 'referencing' to a specific situation may have little influence on the hypothetical bias, and may actually acerbate it: they find no hypothetical bias to be present if the decision between a 15-minute task and a monetary reward concerns the present moment (which hence assumes the role of a reference point), but indirectly find evidence for a hypothetical bias to exist if the trade-off concerns a later (not precisely defined) point in time.

Other strategies to reduce hypothetical biases include making respondents aware of hypothetical biases ("cheap talk") and urge them to respond truthfully (Cummings and Taylor, 1999; Loomis et al., 1996; Murphy et al., 2005b; Loomis, 2014). Also approaches for ex-post corrections of hypothetical biases have been suggested, often based on the certainty indicated by the respondent ("certainty correction") regarding his/her choice (Beck et al., 2016; Blumenschein et al., 1998).

In the context of the valuation of time, more specific factors may play a role and lead to the result that SP-based values tend to be lower than RP-based values (unlike in most studies that aim at eliciting valuations for public goods, see the above-mentioned meta-analyses by List and Gallet (2001); Little et al. (2004); Murphy et al. (2005a)). Several possible reasons for the divergence between SP and RP estimates have been brought forward. First, travel time misperceptions may be a possible cause: Brownstone and Small (2005) argue that if travel times are perceived as longer than they actually are, persons will react to travel times in an SP setting as if they were overstated, in turn leading to SP-based VOT estimates that are biased downwards. This hypothesis was tested in Peer et al. (2014) but could not be confirmed. Second, time inconsistencies may play a role (Brownstone and Small, 2005). They may, for instance, induce drivers to more often choose toll lanes in real life than implied by their SP responses, leading to comparatively lower SP-based VOTs. A further hypothesis is that in hypothetical settings, respondents do not take into account their scheduling restrictions to the same extent they would do in real life (or more generally "disregard alternative time use", see Hultkrantz and Savsin (2018)), due to the lack of real consequences. As a result the SP-based VOT is lower than the RP-based VOT, since the former does not fully account for scheduling constraints.

In our study we can exclude that travel time misperceptions or time inconsistencies play a role in explaining any hypothetical bias, as the only difference between the SP and the RP setting was that only the latter had real consequences. However, we will show that the presence of scheduling constraints plays an important role.

More than 300 Czech university students participated in our study. They were randomly assigned to either the SP or the RP treatment. Each participant had to fill multiple price lists, consisting of 80 choice situations, in which (s)he had to decide between going home directly after the end of the experimental session and no reward, or waiting after the end of the experimental session and earning a specified monetary compensation. For participants in the incentivized (RP) treatment, one of the 80 choices was randomly selected and if the participant had chosen to wait, the student had to remain in the lab for the specified time and received the stated compensation at the end. In the hypothetical (SP) treatment, students were informed that none of the choices will have a real-life consequence for them, so they will be free to leave the lab directly after the experimental session and receive no monetary compensation. We further provided a fixed compensation for participating in the experiment to a sub-group of participants in the SP treatment in order to test whether this encourages participants to invest more cognitive effort in filling in the choice tasks truthfully, potentially reducing hypothetical biases.

We find that hypothetical biases are present even in this controlled setting. In line with earlier studies comparing SP- and RP-based VOT, participants attach a higher value to (waiting) time in the RP setting than in the SP setting. The bias is larger for participants who have scheduling restrictions, presumably because thinking about rescheduling takes up mental resources, which participants are only willing to invest if the real-life consequences occur. This explanation is supported by the hypothetical bias being less strong for participants (with scheduling restrictions) who perform better in an incentivized speed coding test, and therefore are expected to have a higher cognitive ability. This implies that the hypothetical bias found in our data is mainly driven by people who would need to make important changes to their plans, if they had to wait in the lab after the experiment, but who also seem to be less able or willing to think about the consequences of these changes in a hypothetical setting. The bias does not diminish if the participants receive a fixed compensation for participating in the study.

Our study makes multiple contributions. It adds evidence on the reasons why SP- and RP-based VOT diverge. Unlike in most earlier studies that compare SP- and RP-based data , the controlled lab setting allows us to identify and test specific hypotheses related to the origin of the hypothetical bias. Our findings add also to other fields in which stated preference data (or contingent valuation data) are used to elicit willingness-to-pay values (in particular, environmental economics), and which so far have paid fairly little attention to the role of cognitive effort and ability. Multiple studies have emphasized the need for a more general theory on the formation of hypothetical biases (e.g. Murphy et al., 2005b; Mitani and Flores, 2010), to which this paper may contribute. Our paper also adds to the expanding body of literature at the intersection of lab experiments and choice modeling (e.g. Carlsson, 2010; Fayyaz et al., 2018), as well as to the limited literature that shows how constraints in mental processes may lead to biases in valuations (e.g. Koster et al., 2015).

Our results also have strong policy implications as the VOT is a commonly used parameter in transport models and appraisals. If it is afflicted by hypothetical biases, also the outcomes of these models and appraisals are likely to be biased.

The structure of the paper is as follows. Section 2 discusses the experimental design, data sources and experimental procedures. Section 3 provides an overview of the sample characteristics and descriptives on the value of time elicited from the multiple price lists. Section 4 discusses the models and results. Section 5 includes discussion and conclusions.

2. Experimental design and procedures

2.1. Main experiment

The main aim of the experiment is to compare the value of (waiting) time elicited in a setting with hypothetical consequences to a setting with real consequences. We refer to the former as *stated preference* (SP) treatment, and the latter as *revealed preference* (RP) treatment, due to the resemblance of the corresponding data to standard stated and revealed preference data. Each participant was randomly assigned to one of the treatments (between-subject design).

In both the SP and the RP treatment, we elicit the value of waiting time for all our subjects using multiple price lists (MPLs), each containing choices between two options: 1) going home right after the scheduled end of the experimental session, and receiving no additional payment, and 2) waiting for a specified time and receiving a monetary compensation for the waiting time. The only difference between the two treatments is in terms of whether the choices taken in the MLP have real consequences (RP) or purely hypothetical ones (SP).

In the RP treatment, students were informed that one of the 80 questions contained in the MLPs (20 questions in 4 MPLs) will be randomly selected, and their answer to this question will be implemented. Hence, if the choice of a student in the selected question was to wait 0 minutes, she was free to leave after the end of the session. If, however, (s)he chose for the alternative, e.g., waiting for 60 minutes in return for receiving 338 CZK, (s)he had to wait for 60 minutes in the lab and only then received the waiting compensation of 338 CZK. In the SP treatment, in contrast, participants were informed that their choices would not have any consequences, yet they were asked to provide accurate answers based on their actual after-class plans[2]; they were able to leave the lab at the scheduled end of the experimental session and did not receive any additional payment.

In total, participants were required to fill in four MPLs containing 20 questions with varying compensation for waiting: two MPLs with waiting times fixed to 30 and 60 minutes, respectively, and two MPLs with uncertain waiting times with a mean of 30 and 60 minutes, respectively. In the MPLs with uncertain waiting times, the duration of waiting was randomly drawn from a list of six values equal to 15, 21, 27, 33, 39, and 45 minutes in MPLs with short waiting times, and to 30,

[2]All experimental sessions were scheduled in the evening such that none of the students had a class at the end of the experiment.

42, 52, 66, 78, or 90 minutes in the MPLs with long waiting times. The draw was implemented for the RP treatment (if for the selected choice the participant had decided to wait), whereas in the SP treatment it was a hypothetical distribution of potential waiting times. Participants saw the four MPLs in a random order and had a possibility to return to completed MPLs. Each multiple price list contained 20 questions, with the compensation for waiting ascending from 0 to almost 380 CZK per hour.[3] For each MPL, we recorded the midpoint between the values where the subject switches from not waiting to waiting for a given waiting time (or range of waiting times in case of the MPLs with uncertain waiting times), from which the monetary value of (waiting) time (VOT) in CZK per hour can be derived.

While the RP treatment had the same structure in the entire experiment, we implemented two versions of the SP treatment. We test whether students are more motivated to carefully think about their answers (i.e. in a similar way as they are expected to think about their decisions in the RP treatment) if they receive a fixed payment for their participation in the experiment. Hence, in treatment SP0, students were not paid for filling in the MPLs, whereas in treatment SP50, they received a fixed payment of 50 CZK and an additional note stressing that this payment is there to motivate them to think carefully about their choices.

The participants were not informed in advance that they might have to wait after the experiment. The waiting option hence represented an unexpected change in their schedules. Participants in both treatments were informed about the rules applicable in their (hypothetical or actual) waiting time. They knew that while waiting they were not allowed to talk to anyone, use computers in the lab, or sleep. They could, however, use whatever electronic device or reading and writing materials they happened to have with them, providing them, among others, with the possibility to listen to music, read books, do homework, use laptops and mobile phones. They also could go to the bathroom, and if they had to wait for more than 60 minutes, they were allowed to buy food or drinks from a vending machine at school. These rules should imitate an unexpected delay in a public transport trip where the passenger travels alone and waits in an environment with available seats, desks and free WiFi (e.g. when spending unplanned time at a train station due to an unexpected delay).

2.2. Coding speed test (CST)

The MPLs were complemented by data from a coding speed test (CST) filled in by the same participants. In the CST, which measures the speed of information processing (Segal, 2012), participants had 5 minutes time to match as many words as possible with four-digit numbers according to a key. The key, visible during the entire task, contained 10 words and their respective codes. In each question, participants were asked to match a word with one of the five codes offered. As shown by Segal (2012), the interpretation of the recorded test scores differs depending on whether the test is incentivized (i.e. a reward is given for each correct match) or not. If the test is incentivized, the scores tend to reflect cognitive ability. In the unincentivized version, the scores tend to not only reflect cognitive ability, but also intrinsic motivation.

In our experimental setting, both cognitive ability and intrinsic motivation may play a role: both factors may potentially influence how much effort participants put into thinking about their

[3] At the time of the experiment 380 CZK corresponded to roughly four times the hourly wage of unqualified student labor in the Czech Republic. The prices ranged from 0 to 187 CZK if the (mean) waiting time was 30 minutes, and from 0 to 378 if the (mean) waiting time was 60 minutes. We avoided any round numbers that could form focal points.

choices in the MPLs and in particular the implied scheduling implications. While participants in the RP treatment have a natural incentive to carefully think about the real-life implications of their choices due to the fact that one of their choices is going to be implemented, this incentive is absent for participants in the SP treatment. We hypothesize that the extent to which participants in the SP treatment take choices resembling those taken in the RP treatment may depend on their cognitive ability and/or intrinsic motivation. We therefore test the following two types of compensation: in half of the sessions participants received a fixed payment of 30 CZK for completing the CST, and the other half received a piece rate of 1 CZK for each correct answer. Hence, each of the above-mentioned versions of the experiment (RP, SP0, SP50) consists in fact of two treatments, one treatment in which filling in the CST is compensated by a fixed pay (FP) and another treatment in which it is compensated a piece rate (PR). All six treatments are summarized in Table 1.

Table 1
Overview of treatments

	Revealed preference (RP)	Stated preference (SP)	
		No pay	Fixed pay (50 CZK)
Coding speed test fix pay	RP FP	SP0 FP	SP50 FP
Coding speed test piece rate	RP PR	SP0 PR	SP50 PR

2.3. Questionnaire

Before filling in the MPLs, every participant had to answer several questions regarding his/her plans after the scheduled end of the experimental session. This was done in order to induce the participants to carefully think about their scheduling constraints before they attach a monetary value to spending additional time in the lab. There were five questions in total. We asked the following three yes/no questions related to their scheduling constraints, each with tree versions depending on the waiting time (30/60/90 minutes): (Q1) Do you plan to travel somewhere and if yes, would you miss the last connection if you waited in the lab for additional 30/60/90 minutes after the scheduled end of the experiment? (Q2) Do you have a planned activity and if yes, would you miss a part or the entire activity if you waited in the lab for additional 30/60/90 minutes? (Q3) Do you have a meeting planned, and if yes would you have to let someone know that you are late (or cannot make it) if you waited in the lab for an additional 30/60/90 minutes? In addition to this, we asked two open questions: (Q4) What would you miss or which activity would you shorten the most if you waited in the lab for additional 30/60/90 minutes? And finally, (Q5) how would you spend your time if you waited in the lab one hour after the scheduled end of the experiment?

2.4. Procedures

A total number of 16 experimental sessions were conducted at Masaryk University Experimental Economics Laboratory (MUEEL) in Brno, Czech Republic, in November and December 2017 and February and March 2018. The experimental environment was programmed in the Software *zTree* (Fischbacher, 2007). At the beginning of the experiment, an experimenter read the instructions aloud with students following slides with examples of the experimental environment on computer screens in front of them.[4] Registration was carried out through *hroot* (Bock et al., 2014) by sending

[4]Complete instructions are available upon request from the corresponding author.

an e-mail to subjects registered in the *hroot* database. The e-mail contained a link through which individuals could register for an experimental session on a web application. Invitations contained starting and ending times of the experiments. At each time, both the SP and the RP treatment took place simultaneously in two separate labs. Students could sign up for any pair of sessions by clicking on a common link. They were randomly divided into the SP or RP treatment right before the start of the experimental session.

All sessions took place in the late afternoon. The planned end of 8 of the sessions was at 18.00, and of the other 8 at 19.30. A total number of 309 students participated in one of the 16 experimental sessions, each of which had between 16 and 20 participants. 155 persons were assigned to the RP treatment, and 154 to the SP treatment. Right after the end of the experimental session or at the end of the waiting time the participants privately received a payment in cash. The payment included the applicable payoffs, including the fixed or performance-based payment for the speed coding test, a fixed payment of 50 CZK in the SP50 treatment and, in the RP treatment, the payment for waiting if in the randomly selected question the respondent had indicated that (s)he preferred waiting to going home right after the scheduled end of the session. All choices in the RP treatment were successfully implemented, i.e. no participants left before the selected waiting time.

3. Descriptives

Table 2 shows descriptive statistics divided by the RP and SP treatment. The upper part of the table presents the basic characteristics of our respondents and their coding speed test (CST) scores, while the lower part provides summary statistics on the value of (waiting) time (VOT) elicited from the MPLs. As every respondent had to fill in four MPLs, our dataset contains 1,236 VOT observations (309×4).[5] Note that the switching values from MPLs with a (mean) waiting time of 30 minutes were multiplied by two to get an hourly value of time. Moreover, in 89 out of 1236 observations (7.2%), the participants were not willing to wait even for the highest value presented in the 20th line of the MPL (among these 89 observations, 69 are from the RP treatment). These observations were assigned the value of 384 CZK in MPLs with short waiting times and of 388 CZK in MPLs with long waiting times; these values are equivalent to a situation in which the participant would be willing to wait for the value on the 21st line (corresponding to another equidistant step in the MPL). We will present a robustness check regarding this assumption in Section 4.3.

All the subject-specific variables were tested for imbalance across the two treatment groups (SP and RP). As expected due the random assignment of participants to the RP and the SP treatment, no statistically significant differences were found between the two treatment groups (all p-values > 0.05). In both groups, we find a slight prevalence of females. The subjects were mostly Czech and Slovak students with a mean age of 22.5 years. Also average CST scores are comparable across the treatments. In the further analysis, we use CST scores that are normalized separately for the fixed pay and the piece rate treatment.

We find that the mean VOT in the RP treatment equals 172.8 CZK per hour, which is almost double the normal hourly wage for unqualified student labor. The average SP value equals 71.5% of the RP value, which clearly indicates the presence of hypothetical bias in the data.

[5] We did not have to drop any observations due to inconsistent choices made in the MPL, e.g. multiple switches between choosing for waiting vs. leaving, as the experimental environment did not allow for such switches.

Table 2
Descriptive statistics

	Incentivized (RP)	Hypothetical (SP)
Subjects	155	154
Female	56.1%	55.2%
Mean age (s.d.)	22.4 (2.6)	22.6 (2.8)
Nationality		
– Czech nationality	63.2%	58.4%
– Slovak nationality	36.1%	37.7%
– Other nationalities	0.7%	3.9%
Coding speed test		
– Mean score overall (s.d.)	47.5 (11.0)	45.7 (12.2)
– Mean score fix pay (s.d.)	46.2 (11.3)	44.7 (11.8)
– Mean score piece rate (s.d.)	48.8 (10.8)	46.7 (12.6)
MPL-specific observations (four MPLs per participant)	620	616
Mean VOT in CZK (s.d.)	172.8 (117.3)	123.6 (92.4)
Travel = 1	7.1%	7.5%
Plan = 1	39.5%	43.3%
Call = 1	45.5%	41.9%
Constrained (Plan = 1 and Call = 1)	29.8%	31.2%

Figure 1 shows more detailed descriptive evidence on the VOT, taking into account the results from the MPL with a fixed waiting time of 60 minutes (certain) and the waiting times drawn randomly from a distribution with a mean waiting time of 60 minutes (uncertain).[6] The figure further distinguishes for the SP treatment between those who received a fixed pay (SP50), and those who received no pay (SP0). Using a non-parametric Mann-Whitney U test, we find that the values in the four different versions of the experiment shown in Figure 1 are significantly different at a 5% level. In the MPLs that contain uncertain waiting times, the value of time is about 15% higher compared to the corresponding MPLs with fixed waiting times.[7]

Finally, the dummy variables *Travel*, *Plan* and *Call*, as listed in Table 2, are constructed from questions Q1–Q3 of the questionnaire. For a specific MPL, the value of these variables is 1 if the waiting time for which they answered "yes" in questions Q1–Q3 was lower or equal to at least one of the waiting times included in the MPL. For instance, if a person indicated (s)he would have to call someone if (s)he had to wait for 30 minutes, "*Call* = 1" for all four MPLs (30 mins, 60 mins, 15–45 mins, and 30–90 mins). If the call is necessary only for waiting times above 90 minutes, "*Call* = 1" only applies to the MPL with uncertain waiting times between 30 and 90 minutes. The variable *Travel* is equal to 1 (meaning that the last public transport connection would be missed) for less than 8 percent of observations, therefore we did not take it into account when evaluating individuals' scheduling constraints. In contrast, more than 40% of participants indicated that they are constrained by a planned activity (*Plan*) or would have to inform someone that they would

[6] The results for the 30-minute versions are very similar to those of the 60-minute versions, and are hence not shown.

[7] Given that the average standard deviation for the two MPLs with uncertainty is 17 minutes, the reliability ratio (i.e., the value of reliability – with reliability being expressed by the standard deviation of travel times – divided by the value of time) amounts to 0.56, which is in line with the relevant literature (though situated towards the lower end of the distribution, see the meta-analysis by Carrion and Levinson (2012)).

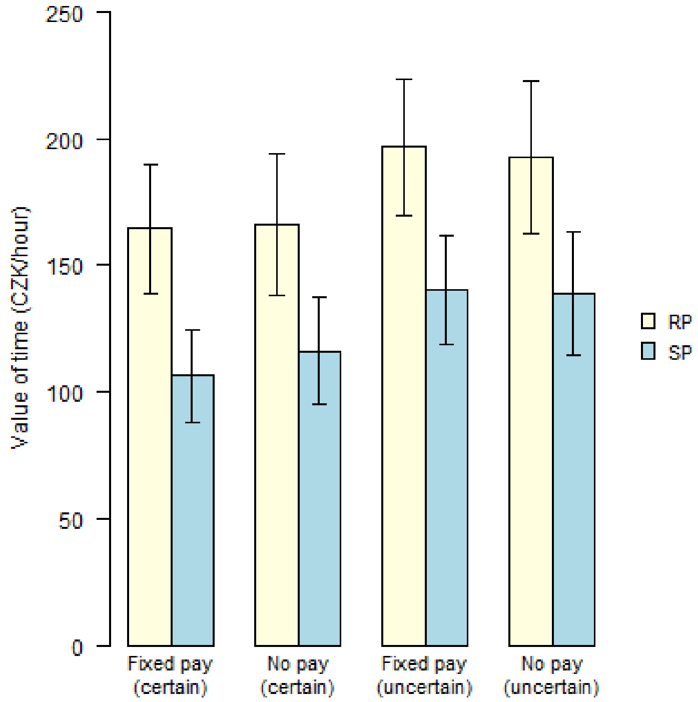

Figure 1
Hourly values of waiting time for the two MPLs with the (mean) waiting time of 60 minutes and the corresponding Mann-Whitney p-values. The bars show the range of ±2SE.

miss (or be late for) a scheduled meeting (*Call*) if they actually had to wait. Since both *Plan* and *Call* capture constraints of a slightly different nature (a planned activity vs. a scheduled meeting), we consider choices of participants as being constrained (i.e. the dummy variable "*Constrained* = 1") if both *Plan* and *Call* are equal to 1.

4. Models & results

4.1. Overview

Table 3 presents the results of four OLS regressions on the pooled data (four observations per subject) with errors clustered at the individual level. They all aim at explaining the elicited VOTs, but then with an ascending number of explanatory variables. In all models we measure the treatment effect by adding a dummy for the SP treatment (*SP*), and we further control for three dummy variables: *uncertain* corresponds to MPLs with uncertain waiting times, *long* to MPLs with a mean waiting time equal to 60 minutes (as compared to 30 minutes in the other versions), and *CST* to the normalized coding speed score. Model 1 only includes these variables, whereas Model 2 additionally controls for scheduling constraints. Models 3 and 4 extend Model 2 by shedding light on the interaction between the hypothetical bias and the CST score.

4.2. Main results

In line with the descriptive results (see Figure 1), all models show that the value of time is significantly higher if the waiting time is uncertain. The parameter estimate for *long* is negative and statistically significant in Models 2–4 (it is not statistically significant in Model 1): once controls for scheduling constraints are added to the regressions, the (on average) 60-minute wait is less than twice as costly as the (on average) 30-minute wait.

Model 1 shows that a significant difference exists between the VOT elicited from the RP and the SP treatment, with the SP-based VOT (152.2-44.7=107.5 CZK) being 71% of the RP-based VOT (152.2 CZK). We refer to this differences as hypothetical bias, as it is purely driven by whether a choice has real consequences (RP) or not (SP).

We find that the size of the bias does not depend on whether subjects in the SP treatment received a fixed fee for completing the MPLs (treatment SP0) or whether they received no pay (SP50), as shown by the interaction between *SP* and *Paid MPL* not being statistically significant. Note that *Paid MPL* is a dummy variable that is equal to 1 for all decisions that were made in SP sessions where participants received a fixed fee for filling in the MPLs as well as in the corresponding parallel sessions with the RP treatment.

Models 2–4 provide further evidence about the origin of the hypothetical bias. Model 2 controls for scheduling constraints. The VOT is significantly higher in both the SP and the RP treatment when the waiting interferes with participants' plans after the scheduled end of the experimental session (as defined at the end of Section 3). More importantly, the estimate of the interaction between the SP treatment and the presence of scheduling constraints is negative and statistically significant (see *SP*×*Constrained*). It implies that the hypothetical bias almost doubles for choices that require adjustments in the evening plans. This effect is driven by the fact that the increase in the VOT due to scheduling constraints in the SP treatment is about half as high compared to the RP treatment. It seems that in the SP setting, where consequences are purely hypothetical, participants do not fully consider possible inconveniences related to the change in schedule.

Model 3 investigates the effect of the CST score on the hypothetical bias. It shows that the CST score does not affect the VOT in the SP treatment in choices that are not constrained by scheduling restrictions (see *SP*×*CST*); however, the impact is positive and statistically significant when participants face scheduling constraints (see *SP*×*CST*×*Constrained*). The estimate shows that an increase in the CST score by one standard deviation increases the VOT measured by the SP questionnaire by almost 60 CZK, hence reducing the hypothetical bias. Participants in the SP treatment with a higher CST score hence seem to take into account the scheduling constraints in a similar way as participants in the RP treatment. In contrast, the VOT of participants with scheduling constraints in the RP treatment is not affected by the CST score *see Constrained*×*CST*). When facing real-life consequences of their choices, our subjects in the RP treatment seem to value the additional inconvenience due to the unplanned schedule change the same regardless of their CST score.

Model 4 splits the CST scores by whether the test was incentivized by a piece rate (*see CST PR*) as compared to a fixed fee in order to be able to draw some conclusions on whether the hypothetical bias is driven by lower cognitive ability or lower intrinsic motivation levels. Our results indicate that the findings of Model 3 are driven by CST scores that are incentivized by piece-rate payments (see *SP*×*Constrained*×*CST*×*CST PR*).[8] Following the study results of Segal (2012), this suggests

[8]This interaction term is equal to 1 for 88 choices made by 31 participants.

Table 3
OLS regression with robust standard errors clustered at the individual level

	Dependent variable: VOT			
	(1)	(2)	(3)	(4)
Constant	152.203***	129.572***	129.615***	125.320***
	(13.281)	(12.230)	(12.216)	(14.117)
Uncertain	28.892***	26.272***	26.498***	26.518***
	(2.408)	(2.544)	(2.548)	(2.563)
Long	8.807***	−10.807***	−10.566***	−10.886***
	(2.426)	(3.515)	(3.481)	(3.469)
CST	4.522	2.864	7.476	−8.721
	(5.077)	(4.788)	(9.210)	(11.379)
SP	−44.718***	−30.171*	−31.955**	−16.776
	(16.931)	(15.756)	(15.731)	(18.900)
Paid MPL	2.753	4.636	3.712	2.130
	(18.012)	(16.064)	(16.147)	(16.740)
SP×Paid MPL	−7.416	−8.892	−6.812	−4.229
	(22.760)	(20.708)	(20.552)	(20.822)
Constrained		110.274***	112.340***	97.858***
		(17.246)	(17.305)	(21.021)
SP×Constrained		−49.760**	−50.397**	−70.849**
		(22.694)	(22.395)	(27.964)
SP×CST			−13.444	0.461
			(10.878)	(14.766)
Constrained×CST			−18.254	0.133
			(14.225)	(17.295)
SP×Constrained×CST			58.375***	19.392
			(19.406)	(22.338)
CST PR				9.087
				(17.145)
Constrained×CST PR				29.749
				(32.486)
CST×CST PR				35.218*
				(18.600)
SP×CST PR				−31.609
				(21.212)
SP×Constrained×CST PR				41.576
				(42.718)
SP×CST×CST PR				−31.896
				(21.886)
Constrained×CST×CST PR				−31.617
				(21.841)
SP×Constrained×CST×CST PR				82.845**
				(38.774)
Observations	1,236	1,236	1,236	1,236
R^2	0.073	0.207	0.222	0.254
Adjusted R^2	0.069	0.201	0.215	0.243
Note:				*p<0.1; **p<0.05; ***p<0.01

that for the group of participants who are in the SP treatment and who have scheduling constraints, the size of the hypothetical bias is lower for those with a high cognitive ability, whereas intrinsic motivation (which tends to be associated with performing well in unincentivized CST) seems to matter less.[9] However, it should be noted that a possible reason for not finding a significant effect for those in the fixed pay treatment is that the corresponding measure of the CST might be more noisy because it may confound the influence of cognitive mobility and intrinsic motivation.

4.3. Robustness checks

We conduct several robustness checks. First, to address the problem that for 7.2% of the MPLs, the participants were not willing to wait even for the highest reward presented, we estimate a Tobit regression that is able to take into account the resulting censoring. The Tobit model with standard errors clustered at the individual level is shown in Table A.4. It provides almost identical results as the OLS regression in Table 3, for which we assumed that participants would have been willing to accept the waiting time at an additional (equidistant) monetary amount added to the MPL (384 CZK per hour in the MPLs with short waiting times, and 388 CZK per hour at MPLs with long waiting times).

Second, we added controls for socio-economic variables (nationality, gender, age), yielding very similar results to those presented above, presumably because of the homogeneity of our sample. Third, our main results also hold if a random-effects model is used instead of the OLS with clustered errors (see Tabel B.5). And finally, the results are also similar if scheduling constraints are defined as "$Plan = 1$" instead of "$Plan = 1$ & $Call = 1$" (see Tabel C.6). Marginally significant interactions between the SP treatment and scheduling constraints ($SP \times Plan$) in Models 2 and 3 are consistent with a weaker definition of constrained choices (they include situations where "$Plan = 1$" but "$Call = 0$").

5. Discussion and conclusion

We conducted a controlled lab experiment, in which participants were randomly assigned either to a stated preference (SP) or a revealed preference (RP) treatment. In both treatments participants, had to fill in multiple price lists (MPLs) in which they faced a trade-off between unexpectedly having to wait after the scheduled end of the experimental session and obtaining a monetary reward. The only difference between the two treatments was that choices made in the SP treatment had no real-life consequences, whereas choices made in the RP treatment had real-life consequences in terms of monetary incentives and unexpected waiting time.

Comparing the values of (unexpected waiting) time (VOT) elicited from both treatments, we find a significant hypothetical bias (i.e. divergence of the SP- from the RP-based VOT): as in most studies comparing SP- and RP-based estimates of the VOT (e.g. Ghosh, 2001; Hensher, 2001; Brownstone and Small, 2005; Small et al., 2005; Isacsson, 2007), we find that the SP-based value is lower than the RP-based one (with the former being around 71% of the latter).

Given that the design of the MPLs was identical between the SP and the RP setting, we can exclude that other explanations such as travel time misperceptions or time inconsistencies (as suggested by Brownstone and Small, 2005) play a role in explaining the hypothetical bias.

[9]This is related to the result of Staněk and Krčál (2018) who find that time preferences correlate with the incentivized, but not with the non-incentivized CST scores.

Moreover, we find that the hypothetical bias does not diminish if participants assigned to the SP treatment are granted a fixed payment in order to make them think more carefully about their choices.

Our results instead seem to indicate that the presence of scheduling constraints plays a crucial role in explaining why the VOT elicited in the SP setting differs from the VOT elicited in the RP setting. We find that the hypothetical bias identified in our experiment can to a large extent be attributed to participants who have scheduling constraints during the time when the unexpected waiting would take place. This may also explain why Hultkrantz and Savsin (2018) in a very similar experimental setup found no hypothetical bias: they only considered an unexpected waiting time of 15 minutes, in which scheduling constraints supposedly play a much more minor role compared to the range of waiting times considered in this experiment (15-90 minutes).

For individuals with scheduling constraints, the trade-off between (unexpected waiting) time and a monetary reward is likely to be more difficult to answer than for those who are unconstrained. In an SP-setting without real consequences, participants might thus tend to ignore their constraints, leading to a downward bias on the VOT. The avoidance of mental effort is consistent with evidence originating from neurosciences and neuroeconomics. Several studies shows that humans are usually inclined to keep the cognitive effort exerted on tasks at a minimum, since cognitive effort has an opportunity cost (Walton et al., 2006; Kool et al., 2010; Kurzban et al., 2013; Westbrook and Braver, 2015; Shenhav et al., 2017). In hypothetical settings, we expect the costs of not exerting any effort approach zero, due to a lack of real-life consequences (unless there are internal, self-generated rewards (see Westbrook and Braver, 2015), which, however, we do not believe to play a major role in our application[10]). Consistent with this explanation, we find that the hypothetical bias is larger for persons with a relatively low cognitive ability (as measured by an incentivized speed coding test), i.e. persons for whom the cognitive effort required to consider the scheduling constraints is likely to be higher.

Our results add to the evidence that SP data may yield results that diverge substantially from those based on RP data (see List and Gallet, 2001; Little et al., 2004; Murphy et al., 2005a, for overviews), potentially leading to wrong decisions regarding transport policies and investments (a large share of benefits in CBA is usually due to travel time gains). We show that hypothetical biases are evident even in a controlled setting, in which participants face a supposedly simple and realistic trade-off between time and money with the hypothetical consequences being very clear and near-term. Furthermore, participants were explicitly told to make considerate choices, they were not put under time pressure, and (a part of participants) received a fixed payment in return for filling in the SP. We must hence conclude that, even if designed and conducted well, SP data are subject to substantial hypothetical biases.

Further research is required on the nature of the underlying psychological processes that seem to induce the hypothetical bias. While our results point at cognitive ability playing an important role, our indicator (coding speed test scores) might also partially capture intrinsic motivation (Segal, 2012), implying that our results are not fully conclusive on the whether only cognitive ability, or also intrinsic motivation plays a role. Measuring mental processes in a more detailed way (by means of alternative standardized tests or even magnetic resonance imaging, see for instance Kang et al. (2011)), may provide interesting insights that add to the results and implications of this paper.

[10] Internal rewards might for instance have played a bigger role in the experiment conducted by Hultkrantz and Savsin (2018), in which the unexpected waiting time was spent on filling in a survey for a Master thesis, possibly evoking pro-social behavior.

Our sample consisted exclusively of students, which raises the question of generalizability of the presented results. Given our finding that (under the presence of scheduling constraints) low cognitive ability is associated with higher hypothetical biases, we believe that the hypothetical bias might be even higher in the general population, as university students can be expected to have above-average cognitive ability. Another reason why we probably observe a conservative estimate of the hypothetical bias is that in 89 out of 1236 (7.2%) MPLs the participant was not willing to accept even the highest monetary incentive offered in return for waiting a given amount of time. In 69 cases (78%) this occurs in the RP treatment. As a result, our RP-based VOT is likely to be somewhat downward biased, and hence closer to the SP-based VOT than it would have been if we had included a wider range of monetary incentives in the MPLs (now the highest amount equals roughly four times the hourly wage rate for unqualified student labor in the Czech Republic).

We have argued that the lab experiment resembles an unexpected delay in public transportation, as participants have to unexpectedly spend time waiting at a place at which they only have access to whatever they have been carrying with them on that day (books, laptop, smartphone, etc.) as well as to WiFi. But clearly not all facets of public transport delays were represented in the experiment. For instance, persons have no uncertainty about waiting time once they have started their wait and they do not need to keep themselves informed where and when the next connection is available. Both factors may contribute to a higher VOT in field experiments (compared to the presented lab experiment), as they render the waiting time more uncomfortable.

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Appendix A. Tobit model

Table A.4
Tobit model with robust standard errors clustered at the individual level

	Dependent variable: VOT			
	(1)	(2)	(3)	(4)
Constant	156.256***	131.400***	131.473***	126.153***
	(12.195)	(12.260)	(12.056)	(15.416)
Uncertain	30.284***	27.463***	27.726***	27.720***
	(2.741)	(2.641)	(2.667)	(2.669)
Long	9.357***	−11.974***	−11.736***	−12.203***
	(2.635)	(3.644)	(3.717)	(3.839)
CST	4.759	2.875	7.905	−8.948
	(7.072)	(6.167)	(9.465)	(13.912)
SP	−48.985***	−32.284*	−34.228*	−17.942
	(17.388)	(18.887)	(18.618)	(22.890)
Paid MPL	1.156	3.196	2.090	1.442
	(15.230)	(14.089)	(13.824)	(13.819)
SP×Paid MPL	−5.254	−6.899	−4.476	−2.594
	(24.946)	(22.421)	(22.248)	(22.048)
Constrained		121.876***	124.439***	103.566***
		(14.069)	(13.900)	(19.983)
SP×Constrained		−58.131***	−59.053***	−74.704**
		(21.159)	(21.054)	(29.602)
SP×CST			−13.892	0.618
			(14.789)	(20.384)
Constrained×CST			−21.974	−0.125
			(15.060)	(20.997)
SP×Constrained×CST			64.335***	20.005
			(23.026)	(34.480)
CST PR				10.355
				(17.407)
Constrained×CST PR				42.594*
				(25.388)
CST×CST PR				36.581**
				(18.269)
SP×CST PR				−32.978
				(28.907)
SP×Constrained×CST PR				32.382
				(41.301)
SP×CST×CST PR				−32.902
				(28.887)
Constrained×CST×CST PR				−43.266
				(29.405)
SP×Constrained×CST×CST PR				94.655**
				(46.573)
logSigma	4.706***	4.627***	4.617***	4.595***
	(0.046)	(0.042)	(0.041)	(0.040)
Observations	1,236	1,236	1,236	1,236
	Left-censored obs.: 0; right-censored obs.: 89			
Note:				*p<0.1; **p<0.05; ***p<0.01

Appendix B. Random effects

Table B.5
Random effects model

	Dependent variable: VOT			
	(1)	(2)	(3)	(4)
Constant	152.203***	141.314***	141.268***	131.748***
	(11.613)	(10.795)	(10.711)	(13.056)
Uncertain	28.892***	28.007***	28.039***	28.057***
	(2.172)	(2.168)	(2.175)	(2.184)
Long	8.807***	1.267	1.216	1.107
	(2.172)	(2.396)	(2.404)	(2.412)
CST	4.522	3.897	3.780	−7.670
	(5.717)	(5.259)	(7.946)	(10.638)
SP	−44.718***	−34.174**	−34.834**	−22.450
	(16.331)	(15.248)	(15.131)	(18.358)
Paid MPL	2.753	3.587	3.590	−0.243
	(16.024)	(14.737)	(14.619)	(14.707)
SP×Paid MPL	−7.416	−8.193	−7.871	−3.573
	(22.734)	(20.907)	(20.736)	(20.634)
Constrained		49.326***	49.579***	56.153***
		(6.096)	(6.135)	(8.826)
SP×Constrained		−34.960***	−33.841***	−49.506***
		(8.429)	(8.496)	(12.100)
SP×CST			−2.214	5.156
			(10.700)	(14.709)
Constrained×CST			−0.279	2.397
			(5.371)	(8.128)
SP×Constrained×CST			10.981	−3.971
			(8.382)	(11.767)
CST PR				22.613
				(14.813)
Constrained×CST PR				−11.647
				(11.631)
CST×CST PR				24.741
				(16.002)
SP×CST PR				−28.858
				(21.057)
SP×Constrained×CST PR				29.772*
				(17.133)
SP×CST×CST PR				−17.129
				(21.319)
Constrained×CST×CST PR				−5.478
				(10.883)
SP×Constrained×CST×CST PR				34.078**
				(17.046)
Observations	1,236	1,236	1,236	1,236
R^2	0.148	0.189	0.191	0.199
Adjusted R^2	0.144	0.184	0.183	0.186
Note:				*p<0.1; **p<0.05; ***p<0.01

Appendix C. Alternative definition of scheduling constraints

Table C.6
OLS regression with robust standard errors clustered at the individual level – Plan instead of Constrained

	Dependent variable: VOT			
	(1)	(2)	(3)	(4)
Constant	152.203***	127.069***	126.989***	119.016***
	(13.281)	(12.687)	(12.681)	(14.896)
Uncertain	28.892***	24.970***	24.912***	24.787***
	(2.408)	(2.584)	(2.586)	(2.595)
Long	8.807***	−15.628***	−15.547***	−15.502***
	(2.426)	(3.898)	(3.894)	(3.888)
CST	4.522	5.735	7.686	−9.947
	(5.077)	(4.673)	(9.073)	(10.618)
SP	−44.718***	−32.633**	−32.077**	−14.763
	(16.931)	(16.343)	(16.292)	(19.931)
Paid MPL	2.753	4.822	4.701	2.827
	(18.012)	(16.347)	(16.402)	(16.861)
SP×Paid MPL	−7.416	−8.202	−9.027	−6.782
	(22.760)	(20.842)	(20.764)	(21.091)
Plan		96.545***	97.221***	89.848***
		(15.743)	(15.713)	(16.886)
SP×Plan		−35.051*	−33.875*	−52.121**
		(19.604)	(19.688)	(23.190)
SP×CST			−11.466	10.599
			(11.398)	(17.362)
Plan×CST			−8.705	8.736
			(11.692)	(11.880)
SP×Plan×CST			28.849*	−8.298
			(15.781)	(19.793)
CST PR				16.159
				(17.803)
Plan×CST PR				18.840
				(29.551)
CST×CST PR				35.317*
				(18.168)
SP×CST PR				−35.737
				(22.218)
SP×Plan×CST PR				31.617
				(38.634)
SP×CST×CST PR				−44.561*
				(24.082)
Plan×CST×CST PR				−32.519
				(24.095)
SP×Plan×CST×CST PR				73.769**
				(32.006)
Observations	1,236	1,236	1,236	1,236
R^2	0.073	0.194	0.200	0.226
Adjusted R^2	0.069	0.189	0.193	0.214
Note:				*p<0.1; **p<0.05; ***p<0.01

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