

The Power of Obedience:
Does coercion decrease the experience of sense of agency?

Cornelis van der Vlist (6014240)

Master Social, Health and Organizational Psychology,

Track: Social Influence

Department Social Sciences, Utrecht University

Reviewer: Hans Marien

Second Reviewer: Nil Akyüz

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**Utrecht
University**

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Abstract

In modern society, maintaining personal autonomy is challenged with the imposition of governmental laws, rules and social norms. A prime example is the use of coercion during the recent COVID-19 pandemic to enforce new measures to stop the virus from spreading. This study explored what effect coercion has on personal autonomy, as reflected in the sense of agency, by replicating parts of the experiments done by Caspar and colleagues. We expected that coercion would decrease the subjective experience of sense of agency. The experiment was conducted in an online environment where 42 participants performed a time interval estimation task. During the task, participants had the option to either financially harm another participant or to withdraw from harming. The participants were sometimes free and sometimes coerced into one of the two actions. After the action was selected the sense of agency was indexed by intentional binding. Contrasting our main hypothesis, we found no significant relationship between coercion and sense of agency. This would suggest that coercion has no effect on personal autonomy. Although no significant relationship has been found, when accounting for the limitations of this study, different results would be expected. Coercion might result in different experiences of agency when changed in setting, deliverance or degree. The current study serves as a starting point for the complex relationship between coercion and personal autonomy within a new digital environment. Future research could focus on improving the methodology of this study and explore with potential moderators, mediators and the operationalization of coercion.

Introduction

Having a feeling of responsibility and control over one's actions is an important part of life that drives people into socializing, working and achieving personal goals. This concept is also known as personal autonomy, which is the perception of being the agent of your own thoughts, feelings and actions (Legault, 2016). When personal autonomy is satisfied it makes people feel more interested, engaged, happy, powerful and is essential for their well-being (Lammers, Stoker, Rink & Galinsky, 2016; Legault, 2020; Yu, Levesque-Bristol & Maeda, 2018). Maintaining personal autonomy is challenged in modern societies with the imposition of governmental laws, rules and social norms if in any way they restrict, steer or control one's thoughts, feelings or actions. Having to conform to dining etiquettes in a formal dining setting is an example when one's actions could be restricted because of the social norm that exists in that given moment. It could therefore be questioned whether conforming to these etiquettes amount to autonomous actions or not. Another prime example of personal autonomy being challenged is during the recent COVID-19 pandemic. The world was surprised with a new virus strain which challenged society in numerous ways. People lost their jobs, couldn't see close relatives in person anymore and many countries enforced a mandatory lockdown with police officers patrolling the streets to keep citizens indoors. With the use of coercion, citizens were restricted to leave their houses to keep the virus from spreading. This brought a lot of implications into the lives of many individuals.

Recent research started to explore the relationship between coercion and personal autonomy and suggests a relationship could exist between them. In a study done by Caspar, Christensen, Cleeremans and Haggard (2016), participants were coerced into the action to harm another participant. In one group of the experiment participants were either forced into a key-press that caused physical pain to another participant (an electric shock) or to refrain from inflicting physical pain. In a second group the physical pain was changed to financial harm (taking money and increasing one's financial gain). During the experiment participants received verbally coercive instructions by the experimenter. This is of course not a new phenomenon with the first introduction of coercion and giving shocks to another individual being the classic experiments of Milgram (1963; 1974). Both experiments focused on conforming to authority and to obey coercive instructions to inflict either shocks or financial harm to a third party. Interestingly, the experiments of Milgram only focused on the participants' willingness to conform and obey, whereas the subjective experience of the participants was not explored. The experiment of Caspar and colleagues was one of the first to

explore this subjective experience, and the current study will complement this line of research by replicating parts of the experiments done by Caspar to explore what effect coercion has on personal autonomy.

In the experiment of Caspar, Christensen, Cleeremans and Haggard (2016), personal autonomy is conceptualized as and presumed to depend upon sense of agency, whereas sense of agency refers to the experience one has during a (un)intentional action. It refers to the feeling of control over one's motor actions and their external consequences (Moore, 2016; Haggard, 2017). Sense of agency tries to explore whether an action is experienced as autonomous or not. Note that it focuses on the experience of control one has during an action, rather than actually being in control as it is not an impeccable reproduction of objective reality (Moore). Synofzik, Vosgerau and Newen (2008) investigated the concept of sense of agency further, by making a distinction between a non-conceptual level of feeling of agency and a conceptual level of judgement of agency. It makes a distinction between implicit and explicit aspects of sense of agency, whereas implicit sense of agency is described as a pre-reflective state which does not involve a reflective act of consciousness. Explicit sense of agency however, is a higher-order judgement of the experience of feeling in control and agent over one's actions (Moore, Middleton, Haggard & Fletcher, 2012). Thus, there is a difference in how we experience control on an unconscious pre-reflective level compared to a conscious after reflection. This is also reflected in the implicit and explicit measures of the sense of agency which is shown to have no significant correlation between them and that they therefore might tap into different processes (Dewey & Knoblich, 2014; Moore, Middleton, Haggard & Fletcher). Caspar and colleagues used implicit measures of sense of agency to explore the subjective experience the participants have when receiving and obeying to coercive instructions. The intentional binding effect was used to measure this implicit sense of agency. Intentional binding is a phenomenon where intentional actions and their effect are temporally bound together in the perception of the agent. This causes the interval between them to decrease and experienced as compressed (Antusch, Custers, Marien & Aarts, 2021). During the experiment a Time Interval Estimation (TIE) task was used which are estimations of the perceived interval between an action and its effect, and therefore measures participants temporal binding. Next to being used in the study of Caspar, intentional binding is widely used in research as a reflection of implicit sense of agency. The results of the study done by Caspar show that coercion can reduce the implicit experience of sense of agency. This would suggest that obeying to coercive instructions would reduce the experience of having personal

autonomy. Interesting to highlight from the results is that the effect of coercion was not related to specifically causing pain or harm but was rather a contextual effect. Note however that the probability of having to inflict shocks to one another might in and of itself already be a stressful situation which might influence the results. The situation might be experienced as a morally significant event to which agency is perceived differently than other events, which could explain the contextual effect (Moretto, Walsh & Haggard, 2011). Next to this, when they compared the physical pain and the financial harm group with each other, the results suggest that when coerced into inflicting financial harm, sense of agency would be less compared to when they were coerced into not inflicting financial harm. This questions if the contextual effect actually exists.

In 2018, Caspar, Cleeremans and Haggard replicated their study of 2016 and found similar results. In this study there was no experimenter who gave coercive instructions but instead another participant did. The sense of agency was not only measured for the ‘victims’ who gave the shock, but also for the ‘commanders’ who gave the instructions. For either the commanders as the victims, when giving or receiving coercive instructions the binding between action and outcome decreased compared to free choices. An interesting addition from the previous study is that commanders also experience less sense of agency. In the context of the lockdown during the pandemic this could suggest that police officers might not feel as responsible for their actions when enforcing the law. Like the previous study, the results show no difference in binding between being coerced into giving and not giving a shock, suggesting again a contextual effect of coercion. However, they did find a difference in binding between giving a shock and not giving a shock regardless of being coerced or free. Participants experienced less binding when giving a shock compared to when they did not. These findings are in line with the research of Moretto, Walsh and Haggard (2011) which found a stronger binding effect for more negative outcomes compared to less negative outcomes.

The restriction of personal autonomy comes in many degrees. The studies done by Caspar and colleagues focus mainly on using coercion to restrict and steer behavior, but there are also other ways personal autonomy could be restricted. The degree to which actions are restricted and conceptualized could play an important role in the effect on sense of agency. The first research on the relationship between personal autonomy and sense of agency began with a study of Wenke, Waszak and Haggard (2009) where the actions of participants were partly restricted. During some of the trials in the experiment, participants either had to press a left or a right key in response to a specific cue. In the other trials they could freely choose

which key to press. The onset of the keypress was also manipulated to be either fixed or free to choose. The keypress was followed by a specific tone with the participants task being to estimate or judge the timing of their actions or the timing of the tone. Results of this study suggest that actions which are derived from a single mode of selection increase the sense of agency compared to different modes of selection, i.e. when an action is partly self-determined and partly externally determined, the experience of sense of agency decreases. This would suggest that restrictions of personal autonomy have no decreasing effect on sense of agency. However, more recent research on personal autonomy and sense of agency show different and mixed results.

A study done by Barlas and Ohbi (2013) explored the idea of having more alternatives and options to give insight in the level of freedom with action choices. In the experiment participants made self-paced button presses while viewing a conventional Libet clock and reported the perceived onset time of either the button presses or consequent auditory tones. Three levels of action alternatives were used (one, three and seven). The results showed that the binding effect is strongest when participants have the maximum number of alternatives and lowest when they only had one choice. This suggests a potential link between agency and the freedom to choose one's actions. Barlas, Hockley and Obhi replicated these findings in 2018 where they found a stronger binding effect in a four-choice condition compared to lower-choice conditions. In contrast, a study from Schwarz, Weller, Klaffehn and Pfister (2019) found no binding effect in a free-action selection condition nor in an instructed action selection condition. However, when sense of agency was measured explicitly by asking to rate their experienced sense of agency via a questionnaire, the free and instructed condition differed significantly. These findings contribute to the diversion between the judgment of agency and feeling of agency and suggest that there is indeed a relationship between personal autonomy and sense of agency, although this relationship might be more subjective than objective. Research done by Antusch, Custers, Marien and Aarts (2021) show similar results. The study exists of three experiments where the role of autonomy in the sense of agency is systematically examined with use of the Libet clock task. The three experiments explored the strength of intentional binding ranging from a forced setting, a partial-autonomous setting and a full-autonomous setting. The results of the study showed a moderate to strong intentional binding effect for all cases with no evidence of autonomy restrictions influencing the effect. Like the study of Schwarz and colleagues, it suggests that autonomy restrictions have no influence on the feeling of sense of agency. When using explicit measures of control however,

the restrictions did show a significant effect on the judgement of agency. The mixed results on the relationship between personal autonomy and sense of agency show that more research is needed to investigate the complicity of autonomy and how this affects sense of agency.

The promising results by Caspar and colleagues on the relationship between coercion and sense of agency is interesting to explore further to get an even deeper understanding of the role that coercion plays. Caspar, Beyer, Cleeremans and Haggard (2021) even found a relationship between coercion and more action in the frontal lobe of the brain. Nonetheless, the amount of studies on this subject are very limited and no systematic research has been done so far. As mentioned before, this study will add to the research line of the relationship between coercion and sense of agency by replicating the studies done by Caspar and colleagues. With the current restrictions of COVID-19, we are forced to stay and work at home. This has implications for doing research in a laboratory setting but it also gives room for exploring research in the digital world. In the study of Caspar and colleagues (2016), participants were forced into a harmful action by an experimenter who stood next to them who gave verbal coercive instructions. It would be interesting to see how coercive instructions affect temporal binding between action and outcome of an individual in a digital setting. The physical appearance of someone who gives the coercive instructions could have a great impact on how coercive the instructions feel, which could also affect temporal binding. The effect of coercive instructions given through an online non-physical setting could therefore be interesting to test, as it reflects our digital world where coercive instructions are sometimes given through online platforms.

Based on the studies of Caspar and colleagues, we expect that in the current experiment temporal binding will be less when a participant is coerced into an action, compared to when a participant is free to choose an action. A few discussed studies suggest that autonomy restrictions might not influence intentional binding. Although these findings seem plausible, based on the results of studies on coercion we do expect that the coercive instructions during this experiment will have an influence on the sense of agency.

H1: The temporal binding will be less when a participant is coerced into an action than when a participant is free to choose an action.

The studies by Caspar and colleagues show mixed results on whether the action to harm or not to harm differs in temporal binding. However, one of their latest studies in 2018 suggests that participants experienced less agency when they inflicted shocks to another

participant compared to when they did not. This gives reason to expect that similar results will be found in the current research. In our experiment we focus on financial harm instead of physical pain. The change to a digital setting makes us unable to experiment with physical pain in the form of shocks, as used in Caspar's experiments. Nonetheless, we expect that financially harming will be experienced just as harmful as inflicting shocks and therefore expect similar findings. Our second expectation of the experiment will therefore be as follows.

H2: The temporal binding will be less when a participant inflicts financial harm than when a participant does not inflict financial harm.

Method

Design and participants

To test the two hypotheses, a similar research design to the study of Caspar and colleagues (2016) will be used, but altered by changing to an online setting. The study used a 2 (choice vs. no-choice) x 2 (harm vs. no harm) within-subjects design. 42 participants were recruited via Prolific, SONA, adverts in university lectures and university student social media groups. The 42 participants consisted of 27 females and 15 males with a mean age of 23,38. The inclusion criteria for the participants consisted of being fluent in Dutch, having a pair of headphones or decent speakers, a wired keyboard and a stable internet connection. For this experiment a confederate was used, therefore it was an important criteria that the participant did not know the confederate. All participants gave written informed consent before starting the experiment. The participants were linked to a random ID number to ensure confidentiality and anonymity of their data. The participants were reimbursed with a fixed amount of 5 euros at the start of the experiment for their participation. This amount could be increased to 10 euros with the choices they made during the experiment. The experiment took roughly 45 minutes to complete, including the introduction and debriefing. The experiment is part of a larger project approved by the Ethics Review Board of the Faculty of Social and Behavioral Sciences, Utrecht University (FETC approval code: 20-537).

Procedure

For recruiting the participants they had to select a timeslot via the platform Doodle. Once the timeslot was selected a meeting was set up in Microsoft Teams, together with the experimenter and the confederate. When the meeting started the experimenter gave verbal instructions consisting of a checklist of the inclusion criteria, filling in the informed consent and an introduction to the experiment. Through the introduction of the experiment it was important that the participant got the feeling that the role they were assigned to was randomized, that they were going to financially harm the other player and that what happened during the experiment depended solely on their actions. After the introduction, the participants were directed to the online testing platform Gorilla where the experiment was built in. Before starting the actual task, the participants were presented with some more information about the duration, reimbursement, consent, anonymity, contact information and a small introduction to the task to make it more meaningful. After this final information participants started with the Time Interval Estimation (TIE) task. Unlike Caspar's experiment (2016) the participants could not see the other 'participant' nor the experimenter during the task. After completing the TIE task, the participants filled in a manipulation check, the SVO questionnaire and demographic information (age, gender, nationality, level of education and employment status). After filling in the questionnaires the participants had to report if their data was usable or not and if they had sufficient concentration during the experiment. Reporting unusable data did not have an effect on their reimbursement. After closing the last screen the participants were instructed to go back to the teams meeting for verbal debriefing with the experimenter. In the debriefing it was discussed how the experiment went, what their thoughts were on the online teams setting and the use of a confederate was revealed. If the participants knew during the experiment that the other 'participant' was a confederate, their data would be excluded from the analysis.

Materials

Time Interval Estimation Task

A similar version of the TIE task used in the study of Caspar (2016) has been used to measure the participants implicit sense of agency. During the TIE task the participants were faced with two options: either to take money from the other participant or to take no money. In this experiment the participants did not have the option to physically harm the other participant

due to ethics and the change to an online setting. The participants only had the option to financially harm the other participant. In half of the trials, the participants were able to freely choose the option they prefer, the 'choice' condition. In the other half of the trials, the participant were coerced to select one of the two options with the use of an experimenter's voice recording, the 'no choice' condition. The trials went as follows: In the choice condition the participants were presented with the cue 'choose' for 750ms. After the cue the participants needed to choose between taking money from the other participant or taking no money. They did so by pressing the 'x' or 'm' key on their keyboard. For half of the participants 'x' was the option to take money and 'm' the option to take no money. In the other half of the participants this was reversed to prevent a left or right hand preference. The participants could only press a key after the cue had disappeared and for 10 seconds only. When the key was pressed a tone was played to give feedback to the participant that the option had been selected. The time interval between the key press and the feedback of the tone varied between 200, 500 and 800ms. After the tone the participants were faced with a blank screen for 500ms. After the blank screen, participants were asked to estimate the time interval between the moment of pressing the key and the feedback of the tone. They did so with the use of a slider going from 0 to 1000ms. The slider gave precise feedback on how many milliseconds the participant chose. The participants were informed that the delay between the pressing of the key and the tone would vary on each trial between 0 and 1000ms. After the participants gave their estimation a screen was presented with the amount of money they currently had and the amount of the other participant. In the trials of the no choice condition the participants were presented with the cue 'forced' and would simultaneously hear a voice saying either 'Take the money' or 'Don't take the money'. After the voice, the participants could only press the key corresponding with the instructions of the experimenter. In half of the no choice trials the participants were forced to take the money and in the other half of the trials the participants were forced to not take the money. The rest of the no choice condition trials were similar to the choice condition. In total there were 120 trials (60 free, 60 forced) divided into ten blocks with a 30 second break after every two blocks. Half of the participants started with all the no choice trials first, with 'Take the money' or 'Don't take the money' trials being randomized. The other half started with all the choice trials first. The reason for this was to prevent participants to make their choice based upon the previous trial. For instance, if a participant were to be forced to take money in a previous trial it could influence the decision for the next choice trial. The delays between pressing the key and hearing the sound were equally distributed in every block. If participants started with the no choice trials first, they would not

be informed that they would be free to choose after and vice versa. There was an attention check before the start of the no choice trials and before the start of the choice trials.

SVO-questionnaire

As an explorative part of this study a revisited version of the Social Value Orientation (SVO) questionnaire has been used to measure the magnitude of the concern people have for others (Murphy, Ackermann & Handgraaf, 2011). The aim of using this questionnaire was to explore if a relationship exists between someone's social value orientation and the choices they made during the TIE task for possible future research. The questionnaire consists of six items where the participant needs to divide money between themselves and another person. Before filling in the questionnaire, the participants got information about the task, that they were randomly paired with another person and about anonymity. For each item the participant had nine options to divide money. Examples of the options are 'you get 98, the other gets 54' or 'you get 81, the other gets 69'. Each item has different options to choose from. For a complete overview of the task and its six items see [Appendix 1](#).

Manipulation check

The manipulation check consisted of seven questions for both the choice and the no choice condition. The aim of the questions was to check if the choice and no choice condition differed compared to the implicit sense of agency. It could for instance give insight into the participants explicit sense of agency. The following questions were asked for both conditions: **1)** To what extent did you enjoy this situation? **2)** To what extent did you struggle with this situation? **3)** To what extent did you find this situation unpleasant? **4)** To what extent did you think this situation was natural? **5)** To what extent did you think this situation was unnatural? And **6)** To what extent did you feel that your autonomy was restricted in this situation? The first six questions could be answered using a 9 points-Likert scale (1= not at all, 9= very much). The seventh question was as follows: **7)** In this situation, did you choose to take the money as frequently as you chose to not take the money? This question was used to check if they used both options in equal amounts. This could give insight into any biases for a specific option. The participants could answer the question using the following options: 'No', 'I think so', 'Yes' and 'I don't know'.

Data-analysis

First of all, the practice trials were removed from the data. Secondly, participants with missing data were excluded from the analysis ($n = 12$). The missing data consisted of in the free condition 11 participants never chose to harm and 1 participant never chose not to harm. Thirdly, one participant had a mean estimation of 1000 in the condition [choice : harm]. This outlier was also excluded from the analysis. Before running the analysis, it was checked that the data met the assumptions independence, normality and sphericity. After the preparation of the data a repeated-measures ANOVA was run ($n = 29$) with choice (choice vs. no choice) and harm (harm vs. no harm) as independent within-subject variables, and the time estimations as the dependent variable. The variable block (choice trials first vs. no choice trials first) was added to the repeated-measures ANOVA as a between-subject variable to investigate a 'block' effect. As an exploratory analysis, SVO-types were added as a covariate to the repeated-measures ANOVA to investigate the interaction between social value orientations and the variables choice and harm. Before the analysis, SVO-mean scores were derived from the 6 SVO-items. Then, the scores were converted to SVO-angles with the use of a mathematical formula ($SVO^\circ = \arctan \left(\frac{A^- o - 50}{A^- s - 50} \right)$). At last, the SVO-angles correspond with a specific SVO-type (1 = competitive, 2 = individualistic, 3 = prosocial, 4 = altruistic). For the manipulation check, paired sample t-tests were run for the six items: enjoyment, struggle, pleasantness, naturalness, unnaturalness and autonomy for the choice and no choice condition. All statistical analysis was executed in IBM SPSS Statistics 27.

Results

Time Interval Estimation (TIE)

The repeated-measures ANOVA with choice and harm as independent variables found no significant main effect of choice ($F(1,26) = 1.939, p = .176, \text{partial } \eta^2 = .069$) and no significant main effect of harm ($F(1,26) = .698, p = .411, \text{partial } \eta^2 = .026$). Also, no significant interaction effect has been found ($F(1,26) = 1.092, p = .306, \text{partial } \eta^2 = .040$). However, the repeated-measures ANOVA with only [choice : no-harm] and [no-choice : no-harm] did find a significant main effect of choice ($F(1,39) = 4.890, p = .033, \text{partial } \eta^2 = .111$). The pairwise comparisons between the [choice : no-harm] and [no-choice : no-harm] conditions showed smaller time estimations in the choice condition ($M = 324.46, SD = 19.40$)

compared to the no choice condition ($M = 359.26$, $SD = 17.88$). The means and standard deviations for the different conditions are shown in Table 1.

Table 1.

Means and standard deviations for the variables choice and harm

	No choice		Choice	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
No harm	347.95*(359.26*)	18.64(17.88)	290.14*(324.46*)	22.40(19.40)
Harm	355.32	22.26	318.21	29.67

Note. *M* and *SD* represent mean and standard deviation, respectively. * $p < .05$. The *M* and *SD* in brackets represent the mean and standard deviation for the repeated-measures ANOVA with only [choice : no-harm] and [no-choice : no-harm] as conditions.

Block

The independent variable block which was added to the repeated-measures ANOVA analysis as a covariate to investigate a block effect. There was no significant main effect found of block ($F(1,26) = .139$, $p = .713$, partial $\eta^2 = .005$). No significant interaction effect of choices and block has been found ($F(1,26) = .028$, $p = .868$, partial $\eta^2 = .001$). No significant interaction effect of harm has been found ($F(1,26) = .101$, $p = .753$, partial $\eta^2 = .004$). Lastly, also no significant interaction effect between choice, harm and block has been found ($F(1,26) = .415$, $p = .525$, partial $\eta^2 = .016$).

SVO

As an explorative part of this study, SVO-types were measured and added to the repeated-measures ANOVA analysis as a covariate to investigate any relation between SVO-type and time estimations. No significant interaction effect of choices and SVO-type has been found ($F(1,26) = 1.315$, $p = .262$, partial $\eta^2 = .048$). No significant interaction effect of harm and SVO-type has been found ($F(1,26) = .466$, $p = .501$, partial $\eta^2 = .018$). Lastly, also no significant interaction effect between choice, harm and SVO-type has been found ($F(1,26) = 1.376$, $p = .251$, partial $\eta^2 = .050$).

Manipulation check

Paired-samples t-tests have been run with six variables: enjoyment, struggle, unpleasantness, naturalness, unnaturalness and restriction of autonomy. The choice and no choice conditions have been paired up to examine the relationship between them. A paired-samples t-test indicated that the scores for restriction of autonomy were significantly higher in the no choice condition ($M = 7.41$, $SD = 1.68$) than for the choice condition ($M = 4.38$, $SD = 2.46$). None of the other paired-samples t-tests indicated a significant difference in scores. The results of the paired-samples t-tests are shown in table 2.

Table 2.

Paired-sample t-tests results for manipulation check scores.

Variable	Choice		No choice		t (28)	p	Cohen's d	95% CI	
	M	SD	M	SD				LL	UL
Enjoyment	4.38	1.82	4.48	2.08	-.20	.842	-.04	-.401	.327
Struggle	4.48	2.12	4.17	2.17	.68	.501	.13	-.240	.491
Unpleasantness	4.79	2.08	4.90	2.01	-.24	.813	-.05	-.408	.320
Naturalness	3.21	1.78	3.21	1.88	.00	1.000	.00	-.364	.364
Unnaturalness	6.38	1.66	6.07	1.89	.85	.402	.16	-.210	.523
Restriction of autonomy	4.38	2.46	7.41	1.68	-6.07	<.001**	-1.13	-1.589	-.653

Note. 1 = not at all, 7 = very much, *M* and *SD* represent mean and standard deviation, respectively, * $p < .05$, ** $p < .01$

Discussion

The aim of this study was to investigate the potential relationship between coercion and personal autonomy. More specifically, we examined whether coercive instructions affect sense of agency in a digitalized setting. We did so by having participants perform in a time interval estimation task which measures intentional binding. Participants were sometimes free to choose and sometimes coerced into an action to take money or not from another participant. We compared conditions of choice (free to choose an action and coerced into an action) and harm (delivering financial harm and delivering no financial harm). First, we expected that temporal binding would be less when a participant is coerced into an action than when a participant is free to choose an action. Contrasting our first hypothesis, findings indicate no significant decrease in temporal binding when being coerced compared to being able to freely choose an action. This suggests that coercion is not related to sense of agency. Interestingly, when only taking the no harm conditions into account, the data does show significant smaller time estimations when having a choice to not harm another individual compared to when having no choice to not harm. Second, we expected that temporal binding would be less when a participant delivers harm than when a participant does not deliver harm. The data shows again no supporting findings, indicating no significant decrease in temporal binding when harming another individual compared to not harming. This also suggests that harming does not relate to sense of agency.

The findings relating to the first hypothesis seem to contrast the studies done by Caspar and colleagues. As this is a replication study of the research line by Caspar, similar results would have been expected. The study of Caspar, Christensen, Cleeremans and Haggard (2016) suggested that coercion can reduce implicit measures of sense of agency. Our results seem to contradict these findings by showing no significant relationship between coercion and sense of agency. In the study of Caspar the experimenter was physically present, giving verbal coercive instructions to the participants. Our research was conducted in an online setting where participants received the coercive instructions via previously recorded audio fragments. It could be argued that the way coercive instructions are given, might have different or even non-existent relationships with sense of agency. The physical appearance of someone giving coercive instructions, as present in studies about coercion (Caspar, Christensen, Cleeremans & Haggard; Caspar, Cleeremans & Haggard, 2018; Caspar, Beyer, Cleeremans & Haggard, 2021), might be of great influence on the degree of how coerced participants feel and therefore be of effect on the sense of agency. The operationalization of coercion could

therefore explain why in this present study, no effect of coercion has been found on the sense of agency. The manipulation check of this study did show that participants felt indeed a restriction of their autonomy in the no choice condition, but it does not reveal how coerced they felt. Our operationalization of coercion might therefore be more in line with the study of Antusch, Custers, Marien and Aarts (2021), where in the forced condition, participants have restricted option choices and are forced to pick one of two options with the use of a written instruction. Adding audio fragments of these same instructions might not enhance the feeling of coercion and therefore show similar results to their study, finding no significant relationship. It could therefore be argued that ‘restricting option choices’ and ‘coercion’ tap into different aspects of personal autonomy.

Previous research also suggests that intentional binding is stronger in maximum alternatives conditions compared to lower-choice conditions (Barlas & Ohbi, 2013; Barlas, Hockley & Ohbi, 2018). In our research, only two option-alternative conditions existed. In the no choice condition, the participant could only pick one option and in the choice condition participants could choose from two options. Compared to these previous studies, one could argue that both the ‘no choice’ and ‘choice’ condition would fall into the lower-alternatives category. This could explain that no significant difference in binding has been found between the two conditions. Participants could have still felt limited in options in the choice condition, suggesting that the choice and no choice condition were experienced somewhat similar. Even though participants were free to choose between two options, they were still forced to choose. However, the manipulation check did reveal that participants felt explicitly more autonomous in the choice condition, suggesting that the choice condition differed significantly from the no choice condition. Before the choice trials, the participants were told they were free to choose an option during the upcoming trials which might have enhanced their explicit feeling of being autonomous but not their implicit feeling. This finding might give an interesting view in the differentiation between the feeling and judgement of agency.

To the contrary, when leaving the harm conditions out of the analysis, the results did suggest less sense of agency when having a choice to not harm another individual compared to having no choice to not harm another individual. Over one quarter of the participants never chose to harm the other individual when they were free to choose. This missing data resulted in a decrease in overall power of the study and could explain why no effect of coercion has been found. These results give room to speculate that a relationship between coercion and sense of agency might exist.

The findings relating to the second hypothesis seem to be in line with the first study of Caspar and colleagues (2016), suggesting no difference in sense of agency between financially harming and not financially harming another individual. However, a follow-up study by Caspar and colleagues (2018) did find a significant relationship between harming and sense of agency, suggesting a stronger sense of agency when no painful outcome was delivered to another individual. Interesting to note from their follow-up study, they only experimented with physical harm in the form of shocks and did not include financial harm which could explain the different findings. It could be argued that financially harming might not be experienced as ‘negative’ enough to have an effect on the sense of agency. For example, the study of Moretto, Walsh and Haggard (2011) revealed a stronger sense of agency for more negative outcomes compared to less negative outcomes. In our experiment, the manipulation check showed no difference in unpleasantness between the choice and no choice condition. However, it did not reveal how unpleasant the action of financially harming is. As we discussed before, it could be that the situation that the participants are in might in and of itself already be experienced as stressful, which could explain that no difference between harming and not harming has been found. But, it could also be argued that we might experience the act of taking someone else’s money as not that negative, which could give interesting insights into what we experience as harmful or not and how this affects our sense of agency.

If the methodological limitations of this study were accounted for and a significant relationship was still not found between coercion and sense of agency, this would have interesting implications on when coercion affects agency. It would seem to disprove the theory set by the studies of Caspar that coercion decreased temporal binding. Coercion would not decrease temporal binding when changed in setting which lowers the generalizability of those results. It could also be problematic in that other variables or moderators might have been in play in the experiments by Caspar and milligram which explained the found relationship and therefore the validity of those results. One such concept is proximity. In an article by Bocchiaro and Zamperini (2012) the influence of a situation on conformity, obedience and disobedience is examined. After Milgram completed his baseline study, he conducted several more variations on this experiment where the setting was altered. The most important change of these variations included the closeness of the ‘inflictor’ and ‘victim’. The variations that Milgram implemented ranged from the inflictor not being able to see or hear the victim to the inflictor having to touch the victim to inflict a shock. In the most remote

condition, the percentage of participants who gave the most dangerous shock were 65%. In the touch proximity condition, the percentage decreased to 30. Bocchiaro and Zamperini conclude that the proximity of the victim raises personal responsibility. Every element that reduces this proximity makes it easier to execute a violent task. Proximity could therefore moderate obedience to coercive instructions and have different effects on the sense of agency when altered. Making it easier to execute a violent task could decrease the negative experience and outcome of the action itself. The setting of the experiments by Caspar might therefore be experienced as more negative when compared to the proximity of the participant and victim in the current study. This negative experience could moderate or mediate the effect of coercion on sense of agency. This is supported by claims that negative emotional outcomes decrease the sense of agency (Moretto, Walsh and Haggard, 2011; Yoshie and Haggard, 2013).

The change to an online laboratory setting and finding no significant relationship between coercion and sense of agency is an interesting observation. Our generation has changed rapidly from an analog society to a digitalized world. The period of COVID-19 can be seen as a big factor in the progression from analog to digital as seen in the change to remote workspaces and digital learning environments. The cyberspace is getting bigger and more realistic, with China even speaking of an online civilization (<https://chinamediaproject.org/2021/11/19/civilizing-cyberspace/>). Whilst this is an exciting transition, it also raises questions about privacy issues and anonymity. Our current study was held online via Microsoft Teams where the participants would leave the meeting after the introduction and proceed with the initial task without observation or monitoring. They were also instructed that they would not see the other participant after the task anymore. It could be argued that this would create an enhanced feeling of anonymity which might be of influence on the feeling of agency. We cannot conclude if an actual relationship exists, but our results can suggest that the change to a digital setting could be of influence on the results of this study. Future research on this relationship could be interesting to explore. For example, Zaghoul, Li and Ren (2020) did research on electronic voting, exploring how this digital form of voting could be coercion-resistant and reassure anonymity. Not much research has been done investigating the relationship between coercion and anonymity in this 'new' digital world and our study could be a small start in the exploration of this concept.

The exploration of the degree and operationalization of coercion could be an interesting focus for future research. Telling someone to perform a specific action in a neutral manner or by shouting could be of great influence in how coercive the message is and how

coerced an individual truly feels. As seen in the experiments done by Milgram and Caspar, the verbal presence could be of great influence in the decrease of the sense of agency compared to the absence of a physical presence of current study. When do individuals really feel coerced? Does it differ between individuals and what degree of coercion might be necessary to be of influence on the sense of agency? Research on this could benefit our understanding of the role coercion takes in our (online) society. What the outcome of a coerced action is could also play a part in the relationship between coercion and sense of agency. In the current study, taking money from another individual and doing this anonymously might not be experienced as negative enough and thus the coercive instruction might lose its effect and purpose. Experimenting with different actions and outcomes might therefore be interesting to explore in future research and could lead to more insights of the possible relationship between coercion and personal autonomy. Lastly, research on the relationship between responsibility, stressful situations and negative outcomes with sense of agency have shown promising findings (Antusch, Custers, Marien and Aarts, 2021; Moretto, Walsh and Haggard, 2011). This suggests that there might be more variables in play which are of influence in the experience of personal autonomy. As an explorative part of this study, we used the SVO-questionnaire to get more insight in morality and social preferences in the potential relationship between coercion and sense of agency. Although no significant relationships with the SVO has been found, it did give us an interesting glance into the variance of social preference from the participants. From the total 42 participants, 39 scored prosocial on the SVO-scale and from the 11 participants who never harmed the other participant when they had free choice, all of them scored prosocial. First of all, this gives an interesting perspective on our sample. Having mostly prosocial orientated participants, will not let us compare the different SVO-types with each other and makes it more difficult to draw definite conclusions from the results. Having a more diverse sample in future research could give more insight into the role of social value orientations in coercion and sense of agency. Secondly, given that the 11 participants who never harmed the other when they had free choice were considered pro social, it gives insight into the behavioral choices of prosocial individuals. When expanded upon this, it could also explain the behavioral choices of other SVO-types in the context of coercion, agency and harmful actions.

To conclude, although no significant evidence has been found of the potential relationship between coercion and personal autonomy in the present study, it does give reason to suggest one could exist. Having found significantly stronger binding when being free to

choose not to harm compared to being coerced into not harming, complements the possibility of an existing relationship. The consistent significant results from the studies done by Caspar and Colleagues that give evidence to this complex relationship, and finding no supporting evidence in the current study shows the difference in the experience of agency when slightly changing the operationalisation of coercion. Coercion might result in different experiences of agency when changed in setting, deliverance or degree. Our change to a digital society is unavoidable. Factors such as responsibility, anonymity and privacy will take up a big part in this new cyberspace and the current research serves as a starting point to explore the complex relationship of coercion and personal autonomy within this new environment.

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Appendices

Appendix 1.

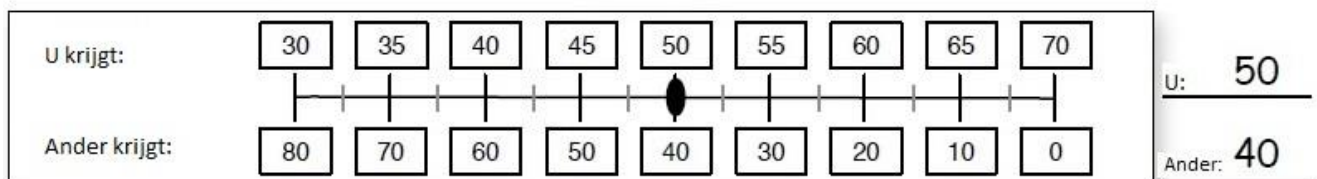
SVO Questionnaire

In deze taak wordt u willekeurig gekoppeld aan een andere persoon, aan wie we refereren als de ander. Deze andere persoon is iemand die u niet kent en u zult wederzijds anoniem blijven. U zult een opeenvolging aan keuzes maken over de verdeling van middelen tussen u en de andere persoon. Geef voor elke van de volgende vragen aan welke verdeling u prefereert door de positie te markeren op de middenlijn. U kunt maar één positie markeren bij elke vraag.

Uw keuzes betreffen geldbedragen voor zowel u als de andere persoon. In het onderstaande voorbeeld heeft een persoon ervoor gekozen het geld zo te verdelen dat hij/zij 50 euro verdient, terwijl de anonieme andere persoon 40 euro verdient.

Er zijn geen goede of foute antwoorden, het gaat alleen om persoonlijke voorkeuren. Schrijf nadat u uw keuze gemaakt heeft de uiteindelijke verdeling van het geld op in de ruimte rechts van de verdeling. Zoals u kunt zien beïnvloeden uw keuzes zowel de hoeveelheid geld die u verdient als de hoeveelheid geld die de ander verdient.

Voorbeeld:



Zelf invullen:

1

U krijgt:	100	98	96	94	93	91	89	87	85
Ander krijgt:	50	54	59	63	68	72	76	81	85

U: _____
Ander: _____

2

U krijgt:	100	94	88	81	75	69	63	56	50
Ander krijgt:	50	56	63	69	75	81	88	94	100

U: _____
Ander: _____

3

U krijgt:	50	54	59	63	68	72	76	81	85
Ander krijgt:	100	89	79	68	58	47	36	26	15

U: _____
Ander: _____

4

U krijgt:	50	54	59	63	68	72	76	81	85
Ander krijgt:	100	98	96	94	93	91	89	87	85

U: _____
Ander: _____

5

U krijgt:	85	87	89	91	93	94	96	98	100
Ander krijgt:	15	19	24	28	33	37	41	46	50

U: _____
Ander: _____

6

U krijgt:	85	85	85	85	85	85	85	85	85
Ander krijgt:	85	76	68	59	50	41	33	24	15

U: _____
Ander: _____

Dit is het einde van de vragenlijst. Bedankt voor uw deelname!