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Vienna, December 5 2021

Dissertation external review
Student: Michal Szczepanik
University: Nencki Institute of Experimental Biology
Title: Neural correlates of emotional contagion in humans: familiarity between participants does not enhance transmission of fear
Grade: [1] Excellent

General assessment and evaluation criteria:
This thesis consists of two distinct manuscripts: one where the applicant is the sole first author, and that is already published, in Scientific Reports, and focuses on psychophysiological and behavioural indicators of observational fear learning from friends vs. strangers; another one available as a preprint on bioRxiv, with shared first authorship, on fMRI activation and connectivity observational fear learning from friends vs. strangers. Moreover, the candidate has been prolific as a co-author of many other published papers, but from my understanding these papers do not count as the core of the thesis. Besides the papers, the thesis consists of an elaborate introduction to the topic of investigation, including an extensive yet succinct review of the background literature, and an overall discussion and outlook chapter. Generally, the thesis attains a high scholarly level and is well written.

The main objective of the thesis was to investigate the role that familiarity with an observed demonstrator obtaining painful shocks in response to visual cues plays in observational fear learning. To this end the candidate collected data from two samples, and used a variety of methods including skin conductance responses and functional MRI analyses, including foremost functional localization mass-univariate approaches and effective connectivity using the Psychophysiological Interactions (PPI) analysis framework. The results converge in that they indicate that in contrast to the main hypothesis motivating the work, whether or not a friend or a stranger is shown as the demonstrator in an observational fear learning paradigm does not influence psychophysiological or brain responses, nor behavioural (self report) outcomes. While this is a classic “null finding”, the candidate took great care both in designing the research in a way that such null findings could be explained (e.g. by designing and powering the fMRI study based on the findings of the SCR study), and in exploring alternative ways to interpret the (absence of confirmatory) findings. Moreover, the fact that the main finding is consistently observed across two different studies and samples confirms that if there is an effect, it may be rather small in terms of statistical effect size.

Overall, this thesis demonstrates the use of a number of innovative and complementary methods, and I would like to congratulate the candidate and his supervisors for the excellent scholarly level of the work performed, and the expertise both in terms of methodology and conceptual background he has achieved through this thesis. I moreover consider the empirical work of the thesis to be original, thoroughly analysed and interpreted, as well as very well written up and contextualized in the overall introductory and concluding chapters. This is why I would rank the thesis in the top 25% of comparable theses I have supervised or evaluated in the past, and grade it with the best possible grade [1, excellent].

There are also a few things that I noticed during reading the thesis which I would like to communicate to the candidate. None of them are essential or bear heavily on my overall positive impression on the thesis.

1. Citations: I am not sure which citation guidelines are followed here, but would suggest to also mention the page number for direct, verbatim citations.
2. On page 7, re: the connection between empathy and observational fear learning, a process model of which aspects of empathy contribute how and when to OFL would have been helpful.
3. On page 14, second paragraph, the reference Lindstrom needs to be spelled out.
4. On page 15, it is unclear what is meant by “another conversion” – probably should have been convention?
5. In chapter 2, the methods are very well but perhaps a bit too succinctly summarized. For instance for the behavioural measures (which should be referred to as questionnaire or self-report measures, to be more precise), the psychometric properties (e.g. reliability, validity indices) could have been refereed.
6. In general, statistical power considerations could have been mentioned more explicitly; as said above though, the step-wise approach (and the higher sample size in study 2) is very much endorsed.
7. On page 26, excessive head motion should be defined
8. In terms of the design, I would suggest to perform the friend and stranger session identically (and not have two persons participate live only for one; but maybe I misunderstood this aspect, and there may be good reasons for doing it the way it was done, but they should be mentioned upfront).
9. Page 29: Calling runs or images BOLD seems less precise than calling them EPIs or functional runs. If anything, add contrast to BOLD.
10. Page 32: explain the added/expected valued of the ROI analyses, and their limitations somewhat better and more extensively.
11. Page 37, Fig. 3.3 and several thereafter: the lines connecting the data points are misleading, since these are discrete rather than temporally or conceptually connected data points (lines are indicative of a continuum)
12. Conceptually, I wonder whether it is really just familiarity that you manipulate with friends vs. strangers – I guess a term such as connectedness would be a better, though somewhat less neutral fit.

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Łódź, December 6th 2021 r.

Monika Wróbel, PhD, Assoc. Prof.

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Review of the Doctoral dissertation of Michał Szczepanik

entitled "Neural Correlates of Emotional Contagion in Humans: Familiarity Between Participants Does Not Enhance Transmission of Fear" and written

under the supervision of Ewelina Knapska PhD, Assoc. Prof., and Marek Wypych, PhD

The reviewed PhD thesis addresses a fascinating problem by focusing on the social transfer of fear in friends vs strangers. The author conducted two experiments using a modified observational fear conditioning protocol by Haaker et al. (2017), in which one participant (a demonstrator) underwent a differential conditioning task (i.e., a neutral stimulus was paired with an aversive stimulation), while the other participant (an observer) observed the demonstrator and then performed a similar task albeit no aversive stimulation was applied. For the sake of readability, I will refer to "psychophysiological experiment" as Study 1 and to "neuroimaging experiment" as Study 2.

Overall, I enjoyed reading the thesis and found the findings highly interesting. I agree with the author that the transfer of fear and its moderators is a problem of great theoretical and empirical importance, and I like that both studies, to the extent possible, were conducted in ecologically valid settings. Below, I present my evaluation of both theoretical and empirical parts of the thesis and comment on its formal side.

Review of the theoretical part of the thesis

The first part of the thesis provides a good theoretical background for the conducted research. The author starts by introducing key concepts and measures and the experimental paradigm that served as a basis for his own research. He also presents valid arguments for studying observational

fear conditioning in more ecologically valid settings and discusses neural correlates of this process. The last section of the Introduction lists research goals and questions that are logically grounded in the reviewed literature.

Overall, I find the theoretical introduction accurate and relevant. My concern, however, is that the rationale behind the predicted familiarity (or friendship) effect on fear contagion (which is central to the thesis) is somewhat unconvincing. The author refers here to de Waal and Preston's (2017) model of empathy, which assumes that higher familiarity and similarity should promote emotional contagion because the observer relies on their own representations of emotional states, which are much richer when interacting with familiar, as compared to unfamiliar, others. Importantly, a similar idea can be found in many theoretical approaches to emotional contagion and mimicry popular in experimental social psychology, where stress is put on moderating role of factors such as affiliation (Hess, 2019; Fischer & Hess, 2017), a shared vantage point (Elfenbein, 2014) or simply liking (Stel et al., 2009). In general, regardless of the name used, all of these models, similar to de Waal and Preston's theoretical approach, assume that an initial emotional or social connection between the interacting partners is a crucial factor enhancing (or even triggering) emotional transfer. In that sense, the author's hypothesis that the transmission of fear should be more pronounced for pairs of friends than for pairs of strangers seems well-grounded in theory. One crucial element, however, is missing here. Specifically, emotional transfer in humans is modulated not only by the relationship between the interacting partners but also by the social meaning of the expressed emotion. Therefore, familiarity and similarity are well-known moderators of happiness contagion because happiness is an affiliative emotion that is readily mimicked and shared (for a review, see Hess & Fisher, 2007). Put differently, social moderators such as similarity of familiarity "work" the way de Waal and Preston's model assumes when an emotion being transferred is of affiliative nature (because affiliative characteristics of the expresser enhance the overall affiliative meaning of the expressed emotion and hence facilitate emotion contagion). This, however, is not true in the case of non-affiliative emotions such as anger, disgust, or fear, which are often not imitated at all, or – when they are imitated – they are not modulated by familiarity in a consistent way, as the social meaning of these emotions may override the role of the expresser's characteristics if these characteristics are not "strong" or explicit enough. Please note that the "strength" of the information about the expresser's characteristics may explain why the effect of familiarity was observed in Golkar and Olsson's (2017) study, which relied on an explicit ingroup-

outgroup comparison, a social factor that is "strong" enough to moderate emotional contagion (see also Wróbel & Imbir, 2019, for a comment on how important the explicitness of social information is).

I think that in the case of the current work, the meaning of the emotion expressed by the demonstrator (fear) was more relevant to the observer's situation (and thus "stronger") than the demonstrator's social characteristics (that is, being a friend or a stranger). Put differently, given that observers were waiting for the same task, it would be simply maladaptive to prioritize information coming from friends over information coming from strangers. Actually, this was my first thought when I read the examples given by the author on p. 3. Specifically, I was wondering why the author expected that people learning about threats by observing others would value the clues coming from friends as more valuable than those coming from strangers. For instance, would exposure to a stranger get burnt when cooking make an observer less cautious when touching a hot pot lid than exposure to a friend? Or, in a broader sense, would it be more evolutionary adaptive to learn here from a friend than a stranger? I can see that the author came to a similar conclusion when interpreting his findings (pp. 58-59), but I suppose that if the author took the literature on the social meaning of the expressed emotion into consideration, this hypothesis would not be formulated the way it was. Put another way, I think that this prediction, although backed-up by de Waal and Preston's (2017) model, was somewhat oversimplified, given the vast literature on the moderating role of the expressed emotion's social meaning on emotional transfer in humans, which was omitted in the provided literature review.

I should stress, though, that despite this omission, I think that the first part of the thesis provides an excellent introduction to the conducted studies. I find the review very coherent and informative, and the rationale behind modifying the observational fear conditioning protocol by Haaker et al. (2017) compelling. Overall, the Introduction showcases the scientific maturity of the author in understanding and integrating the literature, identifying gaps in the knowledge, and formulating important research questions addressing these gaps.

Review of the empirical part of the thesis

The empirical part of the thesis presents two interconnected studies. Study 1 was aimed to test the effectiveness of a modified fear conditioning protocol, whereas Study 2 was focused on the moderating role of familiarity (or friendship) on observational fear conditioning. Both studies were carefully designed and relied on a variety of self-report measures, psychosociological measures and neuroimaging, which is commendable and confirms that the author is highly qualified in collecting data

via different means. Sample sizes, however, were not motivated in any of the two studies, which poses a risk that, given the study designs and a rather low number of participants, both studies might have been be underpowered. The author comments on this possibility with regard to Study 1 (which was initially planned as a within-participants experiment but eventually between-participants analyses were performed, which obviously requires a larger sample), but no formal power analysis is presented. Please note that even when no a priori power analysis was conducted (which, I suppose, was the case with the current studies), it is still advisable to conduct a sensitivity power analysis to check what effect the collected sample sizes were sensitive to detect. I realize that in research that relies on challenging methods such as fMRI, collecting large samples may be difficult. Nevertheless, in the light of the ongoing discussion about limited reproducibility in neuroscience that may result from underpowered studies (e.g., Button et al., 2013; Nord et al., 2017), careful sample size planning (and reporting the results of power analyses) is essential for getting robust findings. This also applies to the research reported in the reviewed PhD thesis, especially given the null effect of friendship on observational fear conditioning.

I also find the modification of an already established fear conditioning protocol a great addition to the literature, and I appreciate the author's insights related to what changes in the protocol were necessary due to the modification and careful explanation of each of those changes. That said, I have some doubts regarding using pre-recorded videos of the demonstrators in Study 2. This concern is related to a more general observation regarding the procedure. As far as I understand, friends met before the experimental session began, which was not the case for strangers. This possibly limits the ecological validity of Study 2. Specifically, the strangers could have doubted that they were witnessing another person going through a differential conditioning task "here and now" (and the use of videos recorded during the "friends pairs" sessions and then used in "stranger pairs" sessions confirms that these doubts were reasonable). In order to make the procedures applied in both conditions as similar as possible, a very brief meeting of strangers should have been organized right before the session began (they could simply see each other entering the rooms in which the session took place). Of course, I realize that this would double the size of the strangers sample (which is a challenge), but such a meeting would probably not only increase the ecological validity of the procedure but also stress the aforementioned explicitness of the friends vs stranger manipulation.

The analyses were reported in a very clear way, and they employ multiple methods, thereby confirming that the author is fluent in advanced statistics. However, as I am an experimental social

psychologist with very limited knowledge of analyses of physiological and neuroimaging data, I find it impossible to asses whether these analyses were accurate. At the same time, I should stress, they the are reported in a clear and coherent way, which facilitates understanding.

The author interpreted the results well in the Discussion against the existing literature. I appreciate the author's insightful comments about the limitations of his research, the novelty and possible applications of the developed procedure, and possible alternative explanations of the observed null effect of familiarity (or friendship) on observational fear conditioning. At the same time, given that this effect was tested in one study only (as Study 1 was limited to friends), and the analyses were based on a rather small sample (n = 68, 34 per participants condition), the author should interpret this null effect with some caution. Drawing upon the title of the thesis, I expected to see very robust evidence for the notion that "familiarity between participants does not enhance transmission of fear". Having read the whole thesis, I find this evidence very promising (and in line with the literature on the social meaning of fear, as elaborated on above) but still preliminary and think that a replication – or ideally a pre-registered replication – is worthwhile and should be considered a further direction.

Review of the formal side of the thesis

Overall, I have no major concerns regarding the formal side of the thesis. It is very well-written and the writing style is understandable, concise, and to the point. The thesis is also very neatly edited (no typos, a convenient list of abbreviations, readable figures with self-explanatory captions, and clear references to the figures in the text).

The thesis has a classic structure and consists of four parts: (1) theoretical introduction, (2) methods, (3) results, and (4) discussion. The organization of these parts is clear, and most of the time, I found the author's reasoning very easy to follow. That said, I think the Results and Discussion chapters would benefit from a slight re-organization. More specifically, given that Study 2 builds upon Study 1 findings, these findings should be discussed prior to presenting the procedure of Study 2. This would help the reader better understand methodological decisions regarding the procedure of Study 2 (or, more precisely, the changes in the procedure that resulted directly from Study 1 findings, which are now described in chapter 2, but became clear to me only after reading chapters 3 and 4). Put another way, the two studies are interconnected, and therefore presenting them in a step-by-step manner (similar to how lines of studies are presented in a scientific paper) would improve the flow.

The same rule applies to some statements regarding methodological decisions. For instance, the sample was limited to male participants, which is a reasonable decision, given the novelty of the procedure. Yet, the detailed rationale behind this decision is not given until Discussion (except for a short comment on reducing sample variability given on p. 22). Another example is dividing participants into two groups in Study 1 based on their contingency knowledge (p. 34), and providing a detailed explanation of why this was done in the Discussion (pp. 46-47).

Conclusion

Despite some remarks mentioned above, I evaluate the PhD thesis of Mr Michał Szczepanik very positively. The reviewed literature provided an accurate introduction to the posed research questions. The research was based on a state-of-the-art methodology and the analysis and discussion of the results are excellent. I have no doubts that Mr Michał Szczepanik addressed a scientific problem in an original way and has the research skills that are expected of a PhD candidate.

Therefore, I confirm that the reviewed PhD thesis meets the conditions specified in the *The Law on Higher Education and Science* issued on July 20th 2018 (art. 187, Dz. U. z 2021 r. poz. 478, 619, 1630) and the Scientific Council regulations from April 13th 2018 (attachment no 1).

Monika Wróbel

Kraków, 01/15/2022

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REVIEW OF MICHAŁ SZCZEPANIK'S DOCTORAL THESIS "NEURAL CORRELATES OF EMOTIONAL CONTAGION IN HUMANS: FAMILIARITY BETWEEN PARTICIPANTS DOES NOT ENHANCE TRANSMISSION OF FEAR"

SUPERVISED BY

PROF. EWELINA KNAPSKA, PH.D., D.SC.

AND

CO-SUPERVISED BY MAREK WYPYCH, PH.D.

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Michał Szczepanik's doctoral thesis "Neural Correlates of Emotional Contagion in Humans: Familiarity Between Participants Does Not Enhance Transmission of Fear" aimed to study the effects of familiarity between the model and the observer on the magnitude of fear induced by observational learning. As observational learning is one of the essential mechanisms of fear induction, the search for factors moderating its effectiveness is very relevant for both psychological theory and clinical practice.

The thesis reports the results of two experimental studies. The first experiment tested a modified protocol to study observational learning of fear with the intention of increasing the ecological validity of previous protocols; thus, it was a proof-of-concept study. The results show that this

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protocol is effective and reliable. The second experiment verified the hypothesis that familiarity between the model and the observer would increase the magnitude of fear induced by observational learning. The results do not provide support for this hypothesis.

The introduction nicely justifies the aims of the thesis as well as the main methodological issues in observational fear conditioning in humans. It also deals with selected and relevant theoretical issues, including the role of empathy. However, this thesis pays relatively little attention to a more general discussion of theoretical accounts of mechanisms of fear induction and a more in-depth discussion of the social learning account. Instead, the introduction is limited to the classical conditioning and social learning accounts of the mechanisms of fear induction. Moreover, it is surprising that Albert Bandura's seminal theory of social learning is not even mentioned in the thesis, although the hypothesis that was tested within this doctoral project could be derived directly from Bandura's theory. Thus, in my opinion, the introduction is the weakest part of the thesis.

The methods section is well written, and the efforts to design a procedure of higher ecological validity should be appreciated. Also, it should be highlighted that the many variables that may have influenced the results have been controlled for, including the evaluation of the model. However, I have two concerns related to the methods. First, this study uses the State and Trait Anxiety Inventory, which is a measure of

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anxiety related to threat to the self rather than to physical treat, but the latter was examined in this study. Second, the thesis lacks details concerning the sample size calculations, so it is not clear whether the studies were adequately powered to answer the research questions. This is crucial, especially in the case of null results, which are difficult if not impossible to interpret when a study is underpowered.

The studies' results are appropriately analyzed. However, although many variables were controlled for, their relationships with the primary outcomes have not been analyzed. For example, it would be important to see whether the empathy, anxiety, and model evaluation scores were correlated with physiological results. Also, and more importantly, as 22 participants from the experiment 2 who did not know the contingency between the stimuli were excluded from the analyses, their data should be compared with the data of those who were not excluded, or all the analyses should be repeated for the whole sample to see whether the results change. Any differences should then be discussed.

The discussion is strong even though it shares some limitations with the introduction (lack of more general theoretical reflection or a discussion of social learning theory). In fact, the discussion makes an attempt to interpret the null results in detail. However, there is a problem with the main conclusion of the thesis. The author has concluded that the results of his studies show that familiarity between the model and the observer does not influence the effects of observational learning of fear. Based on

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the null results, one cannot conclude that there is no effect. Instead, it may be concluded that the results of these studies do not support the expected effect. This is not just a semantic problem; instead, this is a problem of scientific inference, which is slightly surprising in a doctoral thesis.

I am not completely surprised by the null results concerning the effects of the familiarity between the model and the observer on the magnitude of fear induced by observational learning. In fact, in several studies conducted in my lab on placebo effects in pain induced by observational learning, we found no differences in the magnitude of these effects when we manipulated the features of the models, including their social status (Bieniek & Bąbel, 2022), self-confidence (Brączyk & Bąbel, 2021), or how they were introduced to the study participants (as another participant in the same study or as the experimenter's coworker) (Bajcar et al., 2020). The only model feature that we found to influence the magnitude of placebo effects induced by observational learning was sex (Świder & Bąbel, 2013). Thus, I would conclude – also based on the results presented in the doctoral thesis under review – that observational learning is such a powerful method of learning that it may be effective regardless of the model's features, at least those which by nature are more social or psychological.

Although this is a basic science thesis, its results may have some applications as they highlight the power of observational learning in fear

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induction, which is important for understanding the mechanisms of fear induction and its treatment.

The thesis is very concise, but it includes all the required information. It has a standard and proper layout, including the introduction, the methods, the results, the discussion, the bibliography, and supplementary materials. The references are relevant to the thesis topic and include most of the essential publications in the field. The thesis has been carefully and beautifully designed and edited. I have not noticed any language or editorial problems, which is quite rare in the case of doctoral theses prepared as manuscripts.

To conclude, despite some critical issues I have raised above, the thesis under review is strong. It provides an original solution to a scientific problem and demonstrates the author's general theoretical knowledge in psychology and his ability to conduct research work independently. Thus, I have no hesitation in concluding that Michał Szczepanik's doctoral thesis "Neural Correlates of Emotional Contagion in Humans: Familiarity Between Participants Does Not Enhance Transmission of Fear" meets the criteria for doctoral theses specified in article 187 of the Act of 20 July 2018: The Law on Higher Education and Science and the Appendix 1 to the Regulations of the Scientific Council of the Nencki Institute of Experimental Biology of the Polish Academy of Sciences.

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