REPLICABILITY, OPEN SCIENCE, AND A PANDEMIC: THE ROLE OF TRUST IN SCIENCE

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PREFACE

Chapter II is based on the following article:

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S. Dohle generated the idea for the research project, with feedback from T. Wingen and M. Schreiber. S. Dohle and T. Wingen jointly programmed the studies and collected and analyzed the data. S. Dohle and T. Wingen jointly wrote the first draft of the manuscript and all authors critically edited it.

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T. Wingen generated the idea for the research project, with feedback from J. Berkessel and B. Englich. T. Wingen and J. Berkessel jointly programmed the studies and collected and analyzed the data. T. Wingen wrote the first draft of the manuscript and all authors critically edited it.

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T. Wingen generated the idea for the research project, with feedback from J. Berkessel and S. Dohle. T. Wingen and J. Berkessel jointly programmed the studies and collected the data. T. Wingen wrote the analysis code and analyzed the data, and J. Berkessel verified the accuracy of those analyses. T. Wingen wrote the first draft of the manuscript, and all authors critically edited it.

Please note that some changes in formatting were undertaken to fit the layout of this dissertation. No changes were made to the content of the articles and manuscripts.

ABSTRACT

Trust in science is crucial for the functioning of modern societies and is related to a wide range of desirable outcomes. People who trust science are more likely to obtain vaccinations, support climate-friendly action, and reject conspiracy theories. Moreover, studies presented in this dissertation suggest that trust in science is linked to protective behavior during the COVID-19 pandemic (Chapter 2). However, less is known on which factors influence trust in science. In particular, it is unclear whether research practices within the scientific community can alter public trust. This dissertation demonstrates that the discourse on such topics does not happen in the isolated ivory towers of academia. First, this dissertation presents a series of studies showing that information about the replication crisis can damage public trust in (psychological) science and that this damage is difficult to repair (Chapter 3). Second, this dissertation incudes a series of studies that investigate the public perception of preprints, which are an increasingly popular open science practice. These studies suggest that non-scientists trust scientific findings published as preprints less than findings published in peer-reviewed journal articles. However, this is only the case if a short explanation of preprints and the peer-review process is provided (Chapter 4). These lines of work demonstrate that research practices within academia, such as replication studies or publishing preprints, influence the perceived credibility and trustworthiness of science. Overall, this dissertation highlights the importance of trust in science, while suggesting that this trust is partly in the hands of the research community itself.

Deutsche Zusammenfassung

Vertrauen in die Wissenschaft ist von zentraler Bedeutung für moderne Gesellschaften und hat eine Vielzahl von vorteilhaften Konsequenzen. Wer der Wissenschaft vertraut, ist eher bereit sich impfen zu lassen, klimafreundlich zu handeln und Verschwörungstheorien abzulehnen. Darüber hinaus legen Studien in dieser Dissertation nahe, dass Vertrauen in die Wissenschaft eng mit Schutzverhalten während der COVID-19 Pandemie zusammenhängt (Kapitel 2). Es ist hingegen weniger darüber bekannt, welche Faktoren Vertrauen in die Wissenschaft beeinflussen können. Insbesondere ist unklar, ob Forschungspraktiken innerhalb der Forschungsgemeinschaft selbst das öffentliche Vertrauen beeinflussen. Diese Dissertation zeigt, dass wissenschaftliche Debatten zu diesem Thema tatsächlich nicht im isolierten akademischen Elfenbeinturm stattfinden. Zunächst präsentiert diese Dissertation eine Reihe von Studien, welche zeigen, dass Informationen über die Replikationskrise das öffentliche Vertrauen in die (psychologische) Wissenschaft schädigen können. Dieses geschädigte Vertrauen lässt sich nicht leicht wiederherstellen (Kapitel 3). Dann stellt diese Dissertation eine Serie von Studien vor, in denen die öffentliche Wahrnehmung von Preprints untersucht wurde. Diese Studien legen nahe, dass als Preprint publizierte Forschungsergebnisse als weniger vertrauenswürdig eingeschätzt werden als Forschungsergebnissen in Fachartikeln mit Peer-Review Verfahren. Dies ist aber nur der Fall, wenn zuvor eine kurze Erklärung von Preprints und dem Peer-Review Verfahren gegeben wird (Kapitel 4). Zusammengenommen legt diese Dissertation nahe, dass Forschungspraktiken innerhalb der Wissenschaft, wie beispielsweise Replikationsstudien oder das Veröffentlichen von Preprints, einen Einfluss auf die wahrgenommene Glaubwürdigkeit und Vertrauenswürdigkeit der Wissenschaft haben können. Insgesamt demonstriert diese Dissertation somit die Wichtigkeit von Vertrauen in die Wissenschaft und zeigt, dass dieses Vertrauen zum Teil in der Hand der Wissenschaftsgemeinschaft selbst liegt.

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CHAPTER 1: GENERAL INTRODUCTION

When testifying before the US-Congress in 2019, climate activist Greta Thunberg had one central message "I don't want you to listen to me. I want you to listen to the scientists" (Milman & Smith, 2019). Indeed, when facing complex issues such as climate change, people typically benefit from seeking expert advice (Scharrer et al., 2017). One reason for this high dependency on experts is that knowledge nowadays is highly specialized. For example, physicians are responsible for treating illnesses, engineers for building cars, teachers for educating children, and climate scientists for providing climate-related knowledge. However, an essential prerequisite for this *division of cognitive labor* is trust (Hendriks et al., 2016; Keil et al., 2008). For instance, people have to trust that physicians know how to treat illnesses, that engineers know how to build safe cars, and that teachers know how to educate their pupils. This trust is particularly important in regard to scientists, as they possess highly specialized knowledge that is virtually impossible for non-scientists to evaluate adequately (Bromme et al., 2010; Scharrer et al., 2017). This leads to a paradoxical situation: Science has historically evolved as a method to question established knowledge and facts (Hendriks et al., 2016). However, due to the highly complex nature of modern science, non-scientists often have no choice but to blindly trust scientific authority (Hendriks et al., 2016; O'Brien et al., 2021).

In general, trust is defined as a willingness to make oneself vulnerable to another in expectation of beneficial outcomes (Earle et al., 2010; Hendriks et al., 2015; Mayer et al., 1995; Rousseau et al., 1998). For trust in science, this expectation of beneficial outcomes relies on an assumed superiority of scientific knowledge compared to lay knowledge (e.g., a vaccine researcher could provide a more accurate opinion on whether the benefits of getting a vaccination outweigh the costs, compared to a non-scientists). The vulnerability follows from relying on this knowledge, even though it might after all be wrong (Hendriks et al., 2015, 2016; Sperber et al., 2010). Trust in science thus does not only refer to trust in scientists but also to trust in the knowledge produced by scientists (Hendriks et al., 2016). For example, if a research field is highly complex even very trustworthy scientists might produce unreliable knowledge (Van Bavel et al., 2016).

Trust in science can thus be conceptualized on different levels: trust in science as a whole, or as referring to (at least) two different aspects (Altenmüller, Nuding, et al., 2021). First, it can refer to trust in scientists, such as the scientific community (Nisbet et al., 2015) or individual scientists (Hendriks et al., 2015). Building on classic work on organizational trust (Mayer et al., 1995), this trust in scientists can further be differentiated in perceptions of scientists' expertise, integrity, and benevolence (Hendriks et al., 2015). Second, trust in science can be conceptualized as an assessment of the trustworthiness of scientific findings and knowledge (e.g., Anvari & Lakens, 2018). This type of trustworthiness has recently been called credibility (e.g., Altenmüller, Nuding, et al., 2021; Hendriks et al., 2020). It should however be noted that there is considerable ambiguity about the usage of the terms trust and credibility in the literature (Anderegg et al., 2010; Fiske & Dupree, 2014; Metzger & Flanagin, 2013; Renn & Levine, 1991). For example, credibility and trustworthiness are often used interchangeably, and credibility has instead also been used to describe the trustworthiness of a specific person (Anderegg et al., 2010; Renn & Levine, 1991). Moreover, trust in science has also been called epistemic trust, in particular when referring to trust in scientific knowledge (Hendriks et al., 2015, 2016) and social or institutional trust when referring to the institutionalized nature of science (Nisbet et al., 2015; Siegrist, 2000). Nevertheless, this dissertation will adopt the recent usages of the term of trust in science when referring to trust in scientists (Chapters 2 and 3), and credibility when referring to the trustworthiness of specific research findings (Chapter 4).

This dissertation focuses on the crucial role of the trustworthiness and credibility of science in light of two recent, transformative events, that largely impacted society and scholarship: First, the COVID-19 pandemic, and second, the replication crisis and the resulting open science movement (see sections 1.2, 1.4, and 1.5 for details). The following section (section 1.1) provides a brief overview of research showing the importance of trust in science as a predictor of beneficial behavior. Section 1.2 illustrates this with a particular focus on research related to protective behavior during the COVID-19 pandemic. Next, section 1.3 situates this dissertation in relation to three major theoretical perspectives on trust in science: the stereotype content model, the information deficit model, and the public engagement model. Section 1.4 then provides an overview of recent concerns regarding the trustworthiness of science, which were fueled by low replicability. Furthermore, section 1.5 presents different strategies to rebuild public trust in light of low replicability (e.g., open science) and provides an overview of empirical investigations regarding their effectiveness. Finally, section 1.6 presents concerns that too much openness can have unwanted side effects on the public understanding of science. More specifically, many scholars worry that the public might not differentiate between preprints (an increasingly popular open science practice) and the peer-reviewed literature. This section will further introduce a potential solution (i.e., adding brief explanations to preprints) for this problem.

Chapters 2 to 4 present our empirical work on the topics introduced before. Chapter 2 is based on an article that explores predictors of the acceptance and adoption of recommended behaviors during the COVID-19 pandemic, including trust in science. Chapter 3 investigates whether replicability influences trust in (psychological) science and whether damaged trust in science can be repaired. Tested trust repair strategies include increasing transparency, explaining the causes of low replicability, and restoring replicability. Chapter 4 presents evidence on whether preprints are perceived as less credible sources than peer-reviewed journal articles and if this depends on whether an explanation is provided. The General

Discussion (Chapter 5) jointly discusses the findings of Chapters 2 to 4, with a special focus on implications for theoretical perspectives on trust in science. It further examines the limitations of this dissertation, as well as promising directions for future research, and finally provides concluding remarks.

1.1 The Importance of Trust in Science

Why is it important to study trust in science? As outlined before, due to the division of cognitive labor, trust in science is of crucial importance in modern societies (Hendriks et al., 2016). Moreover, trust in science has many beneficial consequences, both on an individual as well as on a societal level. For example, people who trust science are more likely to get vaccinated (Sturgis et al., 2021) and to reject ineffective alternative treatments (Soveri et al., 2021). They also tend to perceive climate change as a serious problem (Rutjens et al., 2018), to reject conspiracy beliefs (Agley & Xiao, 2020; Tonković et al., 2021; van Mulukom, 2020), and to support genetically modified food (Siegrist, 2000; Siegrist et al., 2012), which is a beneficial technology in light of climate change and growing populations (L. U. Wingen et al., 2014).

Moreover, when people trust science, they require little cognitive effort to follow scientific recommendations. This is because even when people lack the ability or motivation to form their own opinion on a scientific issue, they can simply use source characteristics (e.g., whether the source is a scientist) as a heuristic cue to make a decision (Petty & Cacioppo, 1984, 1986; Siegrist, 2000, 2021). For that reason, as long as scientific information is typically valid, trusting science can be seen as an adaptive heuristic that allows non-scientists to make good decisions while saving time and effort (Gigerenzer, 2008; cf. O'Brien et al., 2021).

In turn, trust in science also matters for the research community. Scientists rely on public support and funding, which likely depend on public trust (Baumeister, 2016; Broomell

& Kane, 2017). Moreover, many scientists aim to have a societal impact by giving away their knowledge, often with the altruistic goal of improving people's lives (Lewis & Wai, 2021; Zimbardo, 2004). For example, scientists communicate their research findings through social media (Collins et al., 2016), in policy reports (Ruggeri, 2016), or through specialized science communication magazines (Genschow et al., 2018)[1]. This endeavor is likely to be more successful when public trust in science is high, as communication by trustworthy sources is typically more persuasive (Feng & MacGeorge, 2010; Frewer et al., 1997; Petty & Cacioppo, 1984).

1.2 TRUST IN SCIENCE AND THE COVID-19 PANDEMIC

Finally, the importance of trust in science has been prominently demonstrated during the COVID-19 pandemic, which is globally affecting health, mood, and well-being (Dörnemann et al., 2021; Gassman-Pines et al., 2020; Guterres, 2020). A variety of arguments suggest that trust in science might play a key role in tackling this pandemic.

First, when encountering complex situations, such as a pandemic, people often rely on trust to reduce this complexity (Luhmann, 1989; Siegrist, 2021). For that reason, social or institutional trust, for example in politicians, managers, or scientists, is a central predictor of the perceived risk and benefits of a variety of (potential) hazards, such as genetically modified food (Siegrist, 2000), nuclear energy (Visschers & Siegrist, 2013), or nanotechnology (Siegrist et al., 2007). This is especially the case when people have little knowledge about the hazards (Siegrist & Cvetkovich, 2000), for example when encountering a new virus. For that

[1] In 1913, statisticians from the New York City Health Department even held an infographic parade, where they mounted gigantic infographics on horse trailers and paraded them through the city (Brinton, 1914, cited in Offenhuber, 2014).

reason, it seems likely that trust in science influences people's evaluation of the COVID-19 pandemic, which consequently likely influences their behavior.

Further, trust in science might be particularly relevant during the COVID-19 pandemic, as this crisis has been characterized by a spread of mis- and disinformation, such as conspiracy theories (Imhoff & Lamberty, 2020) and fake news (Loomba et al., 2021). For that reason, the WHO even warned about a COVID-19 *infodemic* (The Lancet Infectious Diseases, 2020). To tackle this infodemic, it might be essential that people trust established scientific sources and their information, instead of less reliable information sources such as their social environment (Chambon et al., 2021) or even fake news and conspiracy theories, which have detrimental consequences for the adoption of protective behaviors (Imhoff & Lamberty, 2020).

Indeed, a lot of empirical work has demonstrated the importance of trust in science for accepting and adopting recommended measures during the COVID-19 pandemic. For example, a large survey in twelve countries (N = 7,755), showed that trust in science was a strong predictor of adherence to protective guidelines, even when controlling for conspiracy beliefs, risk perception, concerns, and knowledge (van Mulukom, 2020). Likewise, a survey across 23 countries (N = 6,948) showed that trust in science predicted individuals' behavioral intentions, whereas the actual risk of infection was a weaker predictor (Pagliaro et al., 2021). Trust in science also played a role as a mediator of the link between political ideology and the adoption of protective measures (Kossowska et al., 2021; Plohl & Musil, 2021; Sulik et al., 2021). Furthermore, trust in science moderated the relationship between political ideology and the endorsement of COVID-19 related conspiracy theories. While self-described conservatives overall showed a greater endorsement of conspiracy theories, this effect disappeared when this group at least had a moderate level of trust in science (Winter et al., 2021). Even though the correlational nature of this work makes causal claims difficult, trust in science was at least a central predictor of protective behavior during the early stages of the COVID-19 pandemic. Our own work (Dohle et al., 2020) which was among the first research projects investigating the role of trust in science during the COVID-19 pandemic, is presented in Chapter 2.

1.3. THEORETICAL PERSPECTIVES ON TRUST IN SCIENCE

Given the high importance of trust in science for modern societies, it is no surprise that trust in science has been studied from various theoretical angles and perspectives. The following section will present three major theoretical perspectives on trust in science, namely a) the stereotype content model, b) the information deficit model, and c) the public engagement model. The stereotype content perspective directly builds on social psychological literature, whereas the information deficit perspective and the public engagement perspective are more strongly rooted in the science communication literature. These different perspectives thus represent the two most relevant foundations of this dissertation.

1.3.1 THE STEREOTYPE CONTENT MODEL

One of the most influential models in the social psychological literature, the stereotype content model (Fiske et al., 2002), has also been applied to trust in science (Fiske & Dupree, 2014). In general, the stereotype content model postulates two dimensions of social perception: warmth (e.g., ratings of helpfulness, trustworthiness, and fairness) and competence, for example ratings of cleverness, creativeness, and knowledge (Cuddy et al., 2008). These two dimensions are described as the fundamental dimensions of stereotypes about groups. For example, US adults perceive some groups as warm and competent (e.g., students), whereas other groups are perceived as low on both dimensions (e.g., homeless people), and finally many stereotypes are mixed. For example, rich people are perceived as high on competence but low on warmth (Fiske et al., 2002). The model further assumes that warmth and competence are predicted by perceived competition and status, and in turn, warmth and competence judgments predict systematic patterns of cognitive, emotional, and behavioral reactions (Cuddy et al., 2008; Fiske et al., 2002).

It seems possible that trust in science is also primarily driven by stereotypic beliefs about scientists' warmth and competence (Fiske & Dupree, 2014). Indeed, people hold stereotypic beliefs about specific occupations (e.g., teachers, maids, or police officers), and this includes beliefs about scientists and even about specific groups of scientists, such as sociologists (Fiske & Dupree, 2014; Imhoff et al., 2018). Importantly, scientists, in general, are perceived as highly competent, but as lacking warmth, similar to CEOs and lawyers (Fiske & Dupree, 2014). Relevantly, warmth (related to benevolence) and competence are both important dimensions of trust in scientists (Hendriks et al., 2015). The work by Fiske and Dupree (2014) would thus suggest that while scientists are already perceived as highly competent, trust in science could be improved by gaining warmth. This is especially important because groups characterized as competent but cold invoke negative emotions, such as malicious envy and Schadenfreude (Crusius et al., 2020; Fiske et al., 2002; Fiske & Dupree, 2014), as well as negative beliefs about one's intentions (Fiske & Dupree, 2014), which could reduce the persuasiveness of scientific communication (Feng & MacGeorge, 2010; Petty & Cacioppo, 1984).

Conceptualizing trust in science as resulting from stereotypes about scientists allows for some interesting predictions on how to increase trust in science, particularly regarding the warmth dimension. For example, humans tend to trust individuals and groups that they perceive as similar to themselves (Fischer, 2009). For stereotypic group perception, that is especially the case for ideological similarity, for example regarding political beliefs (A. Koch et al., 2016). Thus, scientists are likely perceived as warmer and more trustworthy when they are perceived as ideologically similar. Given that scientists are often described as politically liberal, this could explain conservatives' growing distrust in science (Cofnas & Carl, 2018; Gauchat, 2012; see also Fiske & Dupree, 2014).

Furthermore, it is likely no coincidence that many groups (including scientists) are characterized as competent but cold (Imhoff & Koch, 2017). One reason for this is that perceptions of power (i.e., relative control over valued resources) are related to both, perceptions of competence (Chapais, 2015; Dubois et al., 2016; Fiske et al., 2002) and perceptions of immorality (Fragale et al., 2011; Hu et al., 2016; T. Wingen & Dohle, 2021). Indeed, established scientists (i.e., professors) are perceived as a very powerful group (Fragale et al., 2011), and as powerful groups are typically viewed as immoral, this may lead to expectations of immorality and coldness regarding scientists (see also Rutjens & Heine, 2016). Thus, highlighting that scientists are **not** unlimitedly powerful, for example by highlighting how scientific control mechanisms reduce their autonomy, may increase scientists' perceived warmth and trustworthiness (T. Wingen & Dohle, 2021).

Despite being an important and interesting conceptualization of trust in science, the stereotype content perspective does not directly include scientists' behavior, and particularly whether and how scientists communicate with the public. However, such a focus might be relevant, because scientists often engage and interact with the public and actively communicate their knowledge and findings (Huber et al., 2019; Weingart & Guenther, 2016). It seems likely that the public perception of science is not only influenced by stereotypic beliefs, but also by how scientists communicate their findings and knowledge, especially because trust in scientific knowledge is an integral part of trust in science (Altenmüller, Nuding, et al., 2021; Hendriks et al., 2016). This perspective is better captured in models of science communication (see sections 1.2.2 and 1.2.3).

Furthermore, a stereotype content perspective could hardly make predictions about public reactions to specific research practices such as replication studies and the open science movement (Chapter 3), or preprints (Chapter 4), as such specific behaviors are not part of the model. Even though the present dissertation does not directly test the stereotype content model, it has the potential to demonstrate boundaries of applying this model to trust in science (cf. Fiske & Dupree, 2014), by showing that trust in science is strongly influenced even by very specific research practices. This may suggest that a stereotype content perspective cannot fully explain trust in science and that the public perception of science is not merely a (special) form of stereotypic perception. Instead, science-specific aspects such as evaluations of research practices would also need to be considered to fully understand public trust in science.

1.3.2 The Information Deficit Model

During the last century, research directly focusing on science communication largely followed an information deficit model (J. D. Miller, 1983; S. Miller, 2001; Simis et al., 2016; Sturgis & Allum, 2004; Wynne, 1982, 2006), and even contemporary science communication is often well characterized by this model (Dudo & Besley, 2016; Metcalfe, 2019; Trench, 2006). The information deficit model (also called the knowledge deficit model, Simis et al., 2016) is in line with traditional communication models (e.g., Shannon & Weaver, 1949), which proposed that communication aims for a 'source' (e.g., scientists) to transmit a message to a 'receiver' (e.g., the public) without distortion (Stocklmayer, 2012). The information deficit model makes two major assumptions: First, that the public is largely scientifically 'illiterate' (Abunyewah et al., 2020; J. D. Miller, 1983; S. Miller, 2001) and second that scientists simply have to educate the public about their findings and scientific knowledge to build public trust in science (Abunyewah et al., 2020; Reincke et al., 2020; Wynne, 2006). This model assumes that, with growing scientific literacy, the public increasingly supports and trusts science (Simis et al., 2016), perhaps best captured by the quote "To know science is to love it" (Pearson, 2005).

Even though the information deficit model is commonly discussed in the science communication literature and explicitly referred to as a model, it is important to note that it has never been formally proposed as a (psychological) model. Instead, the term information deficit model was retrospectively coined to describe a very broad range of research that built on assumptions in line with this model (Abunyewah et al., 2020; Feinstein, 2011). As the information deficit model is a very broad umbrella term rather than a model in the traditional psychological sense, it contains no specific psychological process which would explain why scientific literacy increases public trust in science. Instead, this is simply assumed to be a consequence of gaining scientific knowledge (Simis et al., 2016).

The information deficit model was fueled by many survey results suggesting that most adults indeed lack basic scientific knowledge. For example, more than half of the British public did not know that the earth goes around the sun once per year or that antibiotics kill bacteria but not viruses (Durant et al., 1989) and the US public did not perform much better on these questions (Sturgis & Allum, 2004). Indeed, depending on the definition only around 10% of the British population could be classified as scientifically literate (Durant et al., 1989). This lack of scientific knowledge is not limited to factual knowledge, but most members of the public were also unaware of basic scientific methodology (Withey, 1959; for an overview see Sturgis & Allum, 2004). These findings, which contributed to a deficit view on the public understanding of science, stem from the last century, but even in more recent surveys, a large number of Americans and Europeans showed high levels of scientific illiteracy. For example, only 74% of US-Americans and 66% of Europeans knew that the earth orbits the sun (Grossman, 2014). Perhaps, this low level of scientific literacy should not be too surprising. Due to the division of cognitive labor people can function well in society without possessing basic scientific knowledge (Hendriks et al., 2016; Keil et al., 2008).

Despite evidence that the public lacks basic scientific knowledge, the information deficit model has been heavily criticized (Feinstein, 2011; Fiske & Dupree, 2014; Schrögel & Kolleck, 2019; Simis et al., 2016; Trench, 2006). First, there is no consistent evidence that communicating scientific information indeed leads to changed attitudes and behavior towards scientific issues. For example, a study with a representative sample of US-Americans found that better-informed individuals tended to show less concern for global warming (Kellstedt et al., 2008), and providing more information is routinely ineffective for increasing the acceptance of health-related scientific recommendations (Kelly & Barker, 2016), such as a healthy diet (Barker et al., 2008). Likewise, vaccination refusals showed to be relatively independent of a lack of information or education (Hornsey et al., 2018; Kitta & Goldberg, 2017). Moreover, instead of being simple receivers of information, individual values and beliefs (e.g., political orientation) seem to determine how people react to scientists (Chan, 2018; McCright et al., 2013). Finally, the fact that individuals actively search or avoid scientific information (Fridman et al., 2020; Siebenhaar et al., 2020; Sweeny et al., 2010), speaks against a passive receiver-perspective. This view of non-scientists who actively engage with science is better captured by a public engagement perspective (see section 1.3.3).

Nevertheless, the information deficit model provides an interesting perspective for this dissertation. If public trust in science is indeed simply improved by learning more about science (Abunyewah et al., 2020; Simis et al., 2016) then even information about scientific controversies such as the replication crisis (Chapter 3) or information about preprints (Chapter 4) should lead to increased trust in science, as they increase scientific literacy and reduce the public's information deficit. At least in the simple form described above, the model does not account for the possibility that non-scientists could react negatively to learning new scientific information (Simis et al., 2016).

1.3.3 The Public Engagement Model

Recently, work on science communication increasingly abandoned the deficit model and instead adopted a public engagement model (Feinstein, 2011; Llorente et al., 2019; Wu et al., 2019; Wynne, 2006), also called the conversation model (Llorente et al., 2019), or dialogue model (Reincke et al., 2020). This process has been described as a move from "deficit to dialogue" (Stilgoe et al., 2014, p. 5). The public engagement model views non-scientists not as passive receivers of scientific knowledge, but instead as active learners (Marteau et al., 1998), who bring their own knowledge, beliefs, and goals to the table when engaging with science (Llorente et al., 2019; Martin et al., 2018). This model thereby further assumes that science communication is particularly effective in building trust when it involves engagement, such as creating a bidirectional relationship, or at least a dialogue between scientists and the public, involving mutual listening and learning (Dudo & Besley, 2016; Llorente et al., 2019). Trust-building science communication under the public engagement model does not only include public communication (i.e., information transfer from scientists to the public), but also public consultation (i.e., information transfer from the public to scientists) and public participation, which is a dialogic exchange between scientists and the public (Schrögel & Kolleck, 2019).

Importantly, and like the information deficit model, the public engagement model has never been formally proposed as a psychological model in the traditional sense. Instead, it is rather an umbrella term for various research and science communication activities implicitly or explicitly building on assumptions in line with the model (Llorente et al., 2019; Stilgoe et al., 2014). These include popular science communication movements and buzzwords such as public participation, citizen science, do-it-yourself-science, and public science (Schrögel & Kolleck, 2019), which are often supported or demanded by policy makers and funders (e.g., Salmon & Priestley, 2015). However, as the model has not been formally proposed the exact psychological process on how public engagement with science leads to increased trust in science remains unspecified. It should however be noted that the model is in line with insights from the educational literature, which suggest that adult learning is more effective when it contains reflection, problem-solving, and involvement, compared to passive reception of knowledge (Kilroy, 2004; Marteau et al., 1998; Wood, 2003).

Despite the recent popularity of public engagement approaches, empirical evidence for the model remains ambiguous. On the one hand, there is convincing evidence that non-scientists are not passive receivers of information, but instead are influenced by values and beliefs (Altenmüller, Lange, et al., 2021; Bender et al., 2016; Chan, 2018; Hamilton, 2015) and actively engage with science, for example in online forums (Hine, 2014), science festivals (Jensen & Buckley, 2014), on science blogs (Bender et al., 2016), or on Twitter and Youtube (Su et al., 2017; Visbal & Crawford, 2017). On the other hand, less is known whether increasing the public engagement with science indeed increases public trust in science. Most work on this topic relies on case studies (Holmes et al., 2019), descriptive surveys (Chen et al., 2021), or qualitative interviews (Domecq et al., 2014), but rarely on experimental work, which would be required to test causal effects of public engagement. Moreover, recent studies found no significant correlation between whether and how often people actively engage with science and their trust in a specific scientist (Altenmüller, Lange, et al., 2021; Altenmüller, Nuding, et al., 2021). Thus, more systematic and experimental research is required to test whether public engagement approaches increase public trust in science, especially beyond a simple information transfer.

Nevertheless, the public engagement model is probably most consistent with the present dissertation. It seems reasonable to assume that if non-scientists actively engage with science and seek scientific information through various channels, it is much more likely that they also encounter information about scientific controversies (e.g., the replication crisis, Chapter 3), scientific reform movements (e.g., open science, Chapter 3, Study 4), or new publication formats such as open access or preprints (Chapter 4), instead of relying only on directly communicated information (cf. Chapter 1.3.2) or on simple stereotypes about scientists (cf., Chapter 1.3.1). Moreover, conceptualizing science communication as a relationship between science and the public would predict that certain scientific information can also damage public trust in science, as damaged trust is a common phenomenon in relationships between individuals and institutions (Bachmann et al., 2015; Bachmann & Inkpen, 2011; Dirks et al., 2009). Thus, under the public engagement model, it seems possible that information about low replicability (Chapter 3, Studies 1 to 3), or about deviations from established publication standards (Chapter 4) might also reduce public trust in science (compared to assuming that learning any new information about science would increase public trust).

1.4 TRUST IN SCIENCE AND LOW REPLICABILITY

The previous sections presented various perspectives on trust in science, which provide guidance and ideas on how to strengthen trust in science. However, the idea that trust in science is beneficial and should be strengthened roots in the assumption that scientists are indeed trustworthy and create credible knowledge (IJzerman et al., 2020; Ruggeri et al., 2020). If this is the case, then relying on scientific knowledge can improve decision-making and inform effective policies (Ruggeri et al., 2021). Yet, scientific knowledge is typically provisional and tentative (Bromme & Goldman, 2014), especially in science areas that deal with complex human behavior (Stroebe & Strack, 2014; Van Bavel et al., 2016).

Indeed, meta-scientific evidence suggests that many, if not most research findings across various disciplines are false (Ioannidis, 2005; Scheel et al., 2021). Moreover, many research findings face concerns about their replicability, defined as the ability of a finding to be consistently observed in new samples using similar methodologies as the original study (LeBel et al., 2018). As replicability is typically viewed as a very low bar for research findings to pass (Vazire et al., 2020), it seems particularly worrying that many research findings do not even pass this low hurdle. For example, more than 70% of surveyed researchers across various disciplines reported that they have tried and failed to replicate another scientist's study, and more than half have failed to replicate their own study (Baker, 2016). An analysis of pharmaceutical concern Bayer found that in almost two-thirds of their projects on potential drug targets, there were inconsistencies between published data in the literature and in-house data, leading Bayer to waste a considerable amount of resources (Prinz et al., 2011). A replication study of structural brain-behavior correlations revealed an even darker picture: Only one out of seventeen effects could be replicated (Boekel et al., 2015). Even though these meta-studies have various limitations (e.g., selection biases), they illustrate that replication issues can occur across various research fields, suggesting that scientific findings in many domains are not necessarily reliable.

Nevertheless, one of the fields affected most by doubts about credibility and replicability is psychology. These doubts first emerged at the beginning of the last decade, through various independent events. First, a psychological scientist admitted making up large parts of his data, which had been published in various leading journals (Der Spiegel, 2011). Second, a social psychological flagship journal published evidence for physically highly implausible PSI effects, using established methodology at this time (Bem, 2011; Ritchie et al., 2012). Third, a seminal paper showed how flexibility in data collection and analysis, without even requiring fraud, allows finding evidence for any desired effect (Simmons et al., 2011).

These cooccurring incidents caused widespread doubts over the credibility of psychology and inspired various meta-scientific projects, which mostly focused on testing the replicability of published findings. Most notably, the *Reproducibility Project: Psychology*, a project involving more than 270 scientists, tried to replicate 100 findings published in leading psychological journals (Open Science Collaboration, 2015). This study found that only one-third to one-half (depending on how replicability was assessed) of the original investigated findings were replicable. Even though this interpretation was heavily contested (Gilbert et al., 2016; Van Bavel et al., 2016), subsequent replication projects found similar issues. For example, some of the most well-known findings in the field, such as ego depletion (Baumeister et al., 1998) or the facial feedback effect (Strack et al., 1988), could not be replicated (Hagger et al., 2016; Wagenmakers et al., 2016) and even systematic replication projects of articles published in the prestigious journals *Nature* and *Science* revealed a low replication rate (Camerer et al., 2018). Moreover, some convenient explanations for replication failures, such as replicators' alleged lack of expertise or flair (Baumeister, 2016), found no empirical support in systematic investigations (Klein et al., 2019).

While this replication crisis had a huge immediate impact on the psychological science community (e.g., the open science movement, see section 1.5), it remained an open question whether the replication crisis would also affect the public perception of science, particularly public trust in psychology. Due to various media reports (Carey, 2015; Devlin, 2018), it seemed likely that (some) non-scientists would learn about the replication crisis, especially because proponents of a reform movement actively engaged in communicating these worrying findings to the public (Nosek, Cohoon, et al., 2015). Many scholars worried that this public debate over low replicability would damage public trust in (psychological) science (Białek, 2018; Fanelli, 2018), and scientists further worried for their reputation, in case of failed replications of their own work (Fetterman & Sassenberg, 2015).

However, if non-scientists simply perceive low replicability to be a form of scientific uncertainty, they might not necessarily react negatively to this information, as the effects of uncertainty on public trust in science are rather small and inconclusive (van der Bles et al., 2019, 2020). Moreover, it seems possible that non-scientists evaluate the replication crisis positively as a necessary attempt to finally getting it right (Ebersole et al., 2016), or as part of an integer self-corrective attitude towards science (Altenmüller, Nuding, et al., 2021; Mede et al., 2020). This optimistic perspective is also prominently reflected in the recently coined term credibility revolution, which highlights the revolutionary potential of the replication crisis to substantially change and improve psychological research (Vazire, 2018).

Nevertheless, initial research investigating the causal effects of replicability on trust in science provided no evidence for such a positive reaction. Results from a large experiment (N = 1,129) showed that learning about the replication crisis reduced public trust in past research and also had no positive effect on trust in future research (Anvari & Lakens, 2018). Likewise, participants perceived an individual researcher whose research failed to replicate (compared to a successful replication) as less trustworthy, reflected in three dimensions of trust: perceived expertise, integrity, and benevolence (Hendriks et al., 2020). These findings thus do not confirm hopes that non-scientists might ignore low replicability or even react positively to it. Our own work on this topic (T. Wingen, Berkessel, et al., 2020), which was conducted simultaneously with the work described above, is presented in Chapter 3 (Studies 1 to 3).

If low replicability damages trust, this puts researchers who want to conduct replication studies in a dilemma: identifying non-replicable results is crucial for scientific progress (Open Science Collaboration, 2015; Vazire, 2018), but at the same time it damages the field's reputation. This could have serious consequences for psychology, such as reduced funding and public support (Baumeister, 2016). Indeed, US-American scholars even voiced concerns that the republican party would use the replication crisis to defund and deny research in general (Yong, 2017).

1.5 Repairing Trust Damaged by Low Replicability

A solution to this dilemma could be to identify a way to investigate and communicate low replicability, without damaging the public trust in science. Shortly after the Open Science Collaboration (2015) published the results of their *Reproducibility Project: Psychology*, the

German Psychological Society (DGPs) published a press release to explain why the low replication rate in this project does not indicate that the original researchers or findings are not trustworthy (German Psychological Society (DGPs), 2015). One central argument in this press release was that low replicability is not to blame on questionable research practices by the original researchers but instead is due to the high context-sensitivity of psychological findings (see also Stroebe & Strack, 2014; Van Bavel et al., 2016). Independent of the validity of this claim, past research showed that such an external explanation can indeed help to mitigate blame (Kim et al., 2006). Chapter 3 (Study 4) presents a study that tested the effectiveness of such an explanation (context-sensitivity) compared to a different explanation (questionable research practices) and including a control group.

Another solution might be to increase the replicability of psychological findings. Previously to the replication crisis, most reported replications were in line with the original findings, which indicates that increasing replicability could be seen as returning to the status quo (Makel et al., 2012). Indeed, restoring the status quo is recommended as an effective trust repair strategy in the organizational literature (Bachmann et al., 2015; Dirks et al., 2009) and many proposed reforms within psychology, such as adopting registered reports or increasing statistical power (Lakens et al., 2018; Perugini et al., 2018; Scheel et al., 2021), would indeed likely increase replicability. However, it is heavily debated whether increasing replicability is a worthwhile goal of psychological science. Counter arguments for example include that psychology, which often studies highly abstract constructs, is necessarily context-sensitive and exact replications will remain an illusion (Stroebe & Strack, 2014), that a focus on replicability will hinder scientific progress (Baumeister, 2016), and that low replicability maximizes efficiency of knowledge gain for the scientific community under a variety of assumptions (Lewandowsky & Oberauer, 2020). Regardless of this controversy, it is an additional question if restored replicability would lead to restored public trust in psychology. Data on this question is presented in Chapter 3 (Study 5).

Finally, another promising strategy to restore public trust damaged by low replicability could be to highlight the open science movement. In fact, the open science movement, which can be described as a movement to make scientific research, data, and dissemination more accessible (Engzell & Rohrer, 2021; Frankenhuis & Nettle, 2018; Nosek, Alter, et al., 2015), has been a major response to the replication crisis. Central aspects of this movement, such as preregistering studies and sharing data, materials, and analyses code, have been viewed as central solutions to the replication crisis (Lindsay & Nosek, 2018; Nosek et al., 2018; Vazire, 2018). Thus, if non-scientists also share this positive sentiment about the open science movement, their negative reaction to low replicability might be mitigated by information about the increasing openness of science. At least in the organizational literature, increasing transparency had already been identified as a successful trust repair strategy (Bachmann et al., 2015).

However, evidence on the effects of information about open science practices on public trust is mixed. Participants who were told about the replication crisis and the open science movement reported similar levels of trust in past research, and even reduced levels of trust in future research, compared to participants who only learned about the replication crisis (Anvari & Lakens, 2018). Therefore, information about open science not only failed to restore trust in past research but even had detrimental effects on trust in future research. This research is however in contrast with more recent findings, which suggest that a researchers' commitment to implementing reforms had positive effects on trustworthiness (Altenmüller, Nuding, et al., 2021). Likewise, research articles are perceived as more trustworthy when they adhere to open science practices (Schneider et al., 2019). Our own work on this topic (T. Wingen, Berkessel, et al., 2020) conducted simultaneously with or before the work described above, is presented in Chapter 3 (Study 4).

1.6 TOO MUCH OPENNESS? THE CASE OF PREPRINTS

Despite some potential positive consequences of open science on public trust (Altenmüller, Nuding, et al., 2021; Schneider et al., 2019; cf. Anvari & Lakens, 2018) the open science movement brings further challenges to the public understanding of science. This is because the open science movement often involves publishing research products very early in the research process (e.g., preregistrations, raw data, initial findings), which are more prone to errors than final products (e.g., peer-reviewed articles). Moreover, as these research materials are by definition shared openly, they are more likely to reach non-scientists (Tennant et al., 2016), who have only a bounded understanding of (open) science (Bromme & Goldman, 2014), and might fail to correctly assess the credibility of these research products. Open science, therefore, makes it likely that preliminary research outputs do not only reach other scientists, but also members of the public who may not know how to properly evaluate them.

This problem is potentially most relevant for the publication of preprints, which are freely accessible scientific manuscripts preceding formal peer-review and publication (Elmore, 2018). Over the last years, preprints became increasingly popular (Soderberg et al., 2020), a development partly fueled by the COVID-19 pandemic (Fraser et al., 2020; Kwon, 2020). However, preprints are not peer-reviewed and did not undergo the established scientific quality control process (Carneiro et al., 2019; Godlee et al., 1998). Many scholars therefore voice concerns that preprints, which are typically publicly available, reach non-scientists (e.g., practitioners, science journalists, or policymakers), who incorrectly treat them as established evidence (Fox, 2018; Rahal & Heycke, 2020; Sheldon, 2018). Simine Vazire, a leading figure in the open science movement, even described the concern "that preprints allow bad science to get into the hands of policymakers and practitioners" as the most frequent argument against preprints (Vazire, 2020).

Indeed, many examples during the COVID-19 pandemic suggest that this concern is well-founded. For example, a study published as a preprint suggested that the Coronavirus is highly similar to human immunodeficiency viruses (HIV) implying that the virus might be a bioweapon (Forster, 2020). Even though the preprint was quickly retracted, the idea that the Coronavirus might be a bioweapon later became a leading conspiracy theory (Imhoff & Lamberty, 2020; Schultz, 2021), potentially supported by an erroneous preprint. Likewise, a widely circulated recommendation of using vitamin D as a treatment against COVID-19 was largely based on a now-retracted preprint, which was not sufficiently critically evaluated by politicians and medical decision-makers (Oransky, 2021).

An underlying assumption of concerns about the public misunderstanding of preprints is that non-scientists do not differentiate between preprints and the peer-reviewed literature. Despite the anecdotes presented above, this view could be contested, because people, in general, are very sensitive to source characteristics. This is especially the case when the content of a message is difficult to process, such as a scientific manuscript (Petty & Cacioppo, 1984, 1986). Perhaps, simply mentioning that something is a preprint (even without an explanation) could already highlight that this work precedes a formal publication process, thereby allowing differentiation to the established literature. Chapter 4 (Studies 1 and 2) presents our work (T. Wingen et al., 2021) on whether non-scientists in such a situation perceive preprints as less credible than the peer-reviewed literature.

Concerns about public misunderstanding could also be misguided if preprint authors already sufficiently explain the provisional nature of their findings, for example by adding a brief explanation on the title page of their work. In this case, non-scientists encountering preprints would immediately be informed about the difference to the peer-reviewed literature and this would enable them to differentiate the two types. Indeed, some preprint servers such as bioRxiv recently started to automatically add such a warning on top of each preprint

(Fraser et al., 2020) and similar warning labels have been proved effective in other areas, such as online news consumption (T. Koch et al., 2021; Pennycook et al., 2020). Nevertheless, many preprint servers especially in the social sciences have not yet adopted such a policy. Thus, authors can decide individually whether to add information about their preprint's lack of peer-review or their preliminary nature, and it had so far not been systematically studied whether this is commonly done. Chapter 4 (Prestudy) presents the result of a systematic coding effort, where we coded 200 recent preprints from two major preprint servers to investigate whether they contain information about their lack of peer-review or other indications of preliminary.

Providing such explanations routinely may be necessary to help non-scientists to differentiate between preprints and the peer-reviewed literature. A variety of recent research findings suggest that non-scientists can understand even complex scientific topics, such as replication studies or open science, and adjust their credibility and trustworthiness ratings accordingly (Altenmüller, Nuding, et al., 2021; Anvari & Lakens, 2018; Hendriks et al., 2020). Chapter 4 (Studies 3 to 5) presents a series of studies that tested whether an explanation also allows non-scientists to differentiate between preprints and peer-reviewed journal articles.

Overall, even though trust in science is crucial for the functioning of modern societies (Bromme & Goldman, 2014; Hendriks et al., 2016), and is associated with many beneficial outcomes (e.g., Oksanen et al., 2020; Rutjens et al., 2018; see also Chapter 2), the present dissertation has a nuanced view on this topic. On the one hand, it is often beneficial to increase or to safeguard public trust in science. For example, if conducting replication studies involves the risk of damaging trust in science, this would constitute an incentive against conducting or at least publicly communicating such studies, even though they are crucial for scientific progress (Open Science Collaboration, 2015; Vazire, 2018). Thus, finding a way how to conduct and report such studies without damaging public trust would be beneficial for science and society, and this is, therefore, a central goal of Chapter 3. On the other hand, blind and unconditional trust in science is not desirable. Some scientific outputs can be less credible than others, especially if they have not been quality controlled by the external scientific community (for example preprints without peer-review). In this case, it seems necessary to find ways to explain these differences to non-scientists, to allow them to differentiate between tentative and more established knowledge, and this is thus a central focus of Chapter 4.

CHAPTER 2: ACCEPTANCE AND ADOPTION OF PROTECTIVE MEASURES DURING THE COVID-19 PANDEMIC: THE ROLE OF TRUST IN POLITICS AND TRUST IN SCIENCE.

This chapter is based on the following article:

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*Equal contribution

Please note that some changes in formatting were undertaken to fit the layout of this dissertation. No changes were made to the content of the article.

Supplementary Materials are available at:

https://spb.psychopen.eu/index.php/spb/article/download/4315/4315.html?inline=1#sp1

ABSTRACT

The United Nations have described the outbreak of the coronavirus disease 2019 (COVID-19) as the worst global crisis since the second world war. Behavioral protective measures, such as good hand hygiene and social distancing, may strongly affect infection and fatality rates worldwide. In two studies (total $N = 962$), we aimed to identify central predictors of acceptance and adoption of protective measures, including sociodemographic variables, risk perception, and trust. We found that men and younger participants show lower acceptance and adoption of protective measures, suggesting that it is crucial to develop targeted health messages for these groups. Moreover, trust in politics and trust in science emerged as important predictors for the acceptance and adoption of protective measures. These results show that maintaining and ideally strengthening trust in politics and trust in science might be central for overcoming the COVID-19 pandemic.

Keywords: COVID-19, trust, protective measures, health communication

2.1 INTRODUCTION

The United Nations have described the outbreak of the coronavirus disease 2019 (COVID-19) as the worst global crisis since the second world war (United Nations, 2020). Behavioural protective measures, such as good hand hygiene and social distancing, may strongly affect infection and death rates worldwide. However, the adoption of protective measures is a social dilemma, as such measures are only effective if many individuals adopt these measures, even though some individuals may only have a low risk of infection or mortality. It is, therefore, of prime importance to understand predictors of acceptance and adoption of protective measures, as this knowledge may guide effective health communication.

Social determinants, such as socioeconomic status, gender, or age, have been recognized as crucial factors for predicting protective health behaviour (Bish & Michie, 2010). Besides these stable, unmodifiable social determinants of health behaviours, many social cognitive models propose that people's risk perception strongly influences protective behaviour (Conner & Norman, 2015). Risk perception is typically based upon a consideration of the perceived likelihood, severity, and susceptibility to a health threat (Brewer et al., 2007; Conner & Norman, 2015), and greater levels of perceived likelihood and perceived severity of the diseases are important predictors of protective behaviour during a pandemic (Bish & Michie, 2010). A largely overlooked factor in many social cognitive models, however, is trust (Bish & Michie, 2010).

2.2 THE ROLE OF TRUST DURING PANDEMICS

The situation during a pandemic is highly complex, as information about the severity of a new disease is constantly changing or not available. Trust can be seen as a mechanism to reduce this complexity (Bish & Michie, 2010; Luhmann, 1989; Siegrist & Zingg, 2014). For example, trust is highly important in situations in which individuals lack knowledge to make decisions (Siegrist & Cvetkovich, 2000), and trust reduces psychological reactance against public policies (Song et al., 2018).

Trust plays a crucial role for the adoption of protective measures and the adherence to governmental restrictions during pandemics (Siegrist & Zingg, 2014). During the SARS outbreak in Hong Kong, confidence in the government predicted health behaviours such as maintaining good hygiene and wearing face masks (Tang & Wong, 2003). Moreover, trust in government was associated with an intention to adopt protective measures at the beginning of the H1N1 influenza pandemic in the Netherlands and positively related to vaccination intention (van der Weerd et al., 2011). A study conducted in the UK also showed that individuals with higher trust in the responsible authorities were more likely to follow their recommended behaviours (Rubin et al., 2009). Moreover, Italians who adopted recommended behaviours—such as good hand hygiene—during the H1N1 pandemic had higher trust in media and the Italian ministry of health (Prati et al., 2011). Finally, in the COVID-19 pandemic, countries with higher institutionalized trust report lower fatalities (Oksanen et al., 2020).

Trust in governmental agencies and politicians, however, might only be one aspect of trust that may shape people's acceptance and adoption of preventive measures against novel infectious diseases. In a highly complex situation, such as a pandemic, people also need to rely on relevant scientific experts (Hendriks et al., 2016). Thus, trust in science might also play a crucial role during a pandemic. During the first wave of the COVID-19 pandemic in Germany, for example, the public debate was largely shaped by virologists and epidemiologists, who undertook large efforts to inform the public about COVID-19, such as broadcasting a popular podcast (e.g., Kupferschmidt, 2020). A representative survey conducted during the outbreak of COVID-19 in Germany confirmed that most people were more likely to trust scientists than politicians, and more than 80 % agreed that political decisions concerning COVID-19 should be evidence-based (Wissenschaft im Dialog, 2020). The relevance of trust in science has been shown for some protective behaviours such as vaccine uptake (e.g., Hamilton et al., 2015), but for many other health threats, evidence remains scarce.

2.3 The Present Research

The present research investigated three types of important predictors of the acceptance and adoption of protective measures during the COVID-19 pandemic. First, social determinants may play a crucial role in explaining protective behaviours (Bish & Michie, 2010). Identifying groups that are less likely to follow recommended behaviours allows public health authorities to design targeted messages to reach precisely the groups where interventions matter most. Second, building on the literature on health-related social-cognitive models (Conner & Norman, 2015), risk perception could explain why some individuals, but not others, accept and adhere to recommended protective measures. Finally, we examined whether trust contributes to explaining individuals' likelihood of accepting and adopting protective measures. In contrast to previous research, however, we not only focused on trust in politics, but also on trust in science. It should be noted that some theoretical models in the literature assume interrelationships between risk perception and trust and conceptualize these relationships in different ways (Mayer et al., 1995; Siegrist, 2020; Slovic, 1999). In line with Bish and Michie (2010), however, we consider both risk perception and trust as independent predictors of protective behaviours in the present research.

To reach our aims, we conducted two studies. Study 1 used an exploratory approach; Study 2 was preregistered (see study details). Materials, data, and code for both studies are available through the Open Science Framework (OSF; see https://osf.io/xmv54/?view_only=a2acd47581ef45498e78d651b2ca4b36).

2.4 STUDY 1

2.4.1 METHODS

Design and Participants. We conducted a cross-sectional study during the early phase of the COVID-19 pandemic in Germany. Data were collected between March 24, 2020, and March 31, 2020. The study started two days after the German government imposed heavy restrictions to stem the spread of COVID-19, including banning gatherings of more than two people. On the day the study started, 27,436 cases and 114 deaths due to COVID-19 had been reported in Germany (Robert Koch Institute, 2020).

Inclusion criteria for the study were (a) a minimum age of 18, and (b) permanent residency in Germany. The survey was advertised via the institute's participant pool and via social media postings on Facebook and Twitter, where it was also promoted via the university's press office. Further, participants shared the study within their personal networks (snowball-sampling-method).

Our initial sample consisted of 737 individuals. Of these, 61 individuals (8.3%) did drop out right away after providing their consent, nine individuals (1.2%) indicated that we should not use their data for analyses, and ten individuals (1.4%) resided outside of Germany. All these individuals were excluded from data analyses, leaving a final sample of 661 individuals (M_{age} = 35.22, SD_{age} = 12.56, 77.2% female; see also sample characteristics in Table S1 in the supplementary materials). Of these, 554 (83.8%) responded to the final question of the questionnaire, indicating a dropout of 16.2% throughout the survey.

A sensitivity analysis showed that the final sample had a very high chance ($\beta = .95$, $\alpha = .05$, $N = 554$) to detect correlations of $r = .15$. All statistical tests were two-sided, with $p < .05$ considered statistically significant. Given the exploratory nature of Study 1, however, *p*-values should be interpreted with caution.

Procedure. Participants were invited online to take part in a brief psychological survey on COVID-19. They were informed that they would not receive any compensation, received a digital flyer with general information, and a link to the survey hosted on Qualtrics. After participants consented, Qualtrics automatically guided them through the study and provided them with all measures. At the end of the survey, participants were provided with an open-entry textbox, where they could leave general comments to the research team.

Measures. A translated version of the measures can be found on the OSF. We first presented participants with eleven behaviour-based protective measures (e.g., "Stay home as much as possible.", α = .89, 1 = *not reasonable*, 7 = *very reasonable*). We only included measures that were promoted by the German federal government agency for disease control (Robert-Koch-Institute) and the Federal Centre for Health Education at the time when the survey was conducted. We moreover assessed acceptance of two items related to hoarding, which we do not discuss further (but see OSF). We then presented participants again with the same eleven behaviour-based protective measures and asked them how often they engage in this behaviour to assess adoption of protective measures (α = .71, 1 = *never,* 5 = *always*). These two variables—acceptance and adoption of behavioural protective measures—served as the main outcome variables in our analyses, as they seem central to tackling the spread of COVID-19. Participants also answered twelve items measuring acceptance of the shutdown and of governmental restrictions (e.g., "Closing schools and daycare.", α = .91, 1 = *not reasonable*, 7 = *very reasonable*). Because Germany has a federal structure, restrictions varied at the time of the study, but all mentioned restrictions were effective at least in one state at the time of the study.

Participants then completed nine items to measure their trust in politics during the pandemic (e.g., "Information released by German politicians concerning the coronavirus can be trusted.", α = .94, 1 = *disagree strongly*, 7 = *agree strongly*). We adapted the same nine items to measure trust in science (α = .94). We also included one additional item "Politicians rely on the recommendations of scientists in order to overcome the corona crisis.", which we do not discuss further (but see OSF). Exploratory factor analyses (with promax rotation) on the items that measured trust in politics and, separately, on the items that measured trust in science indicated a one-factor solution for both trust in politics and trust in science (as demonstrated by the eigenvalue ≥ 1 criterion and the scree plot). Factor loadings are presented in Table S2.

Risk perception was assessed as part of a larger project that investigated probability estimates and biases in risk perception; these results are reported elsewhere (Glöckner, Dorrough, Wingen, et al., 2020). Participants indicated their estimated probability that they will become infected with the coronavirus by the end of the year (from 0 to 100%) and how severe the consequences of such an infection would be (1 = *no negative consequences,* 10 = *extreme negative consequences*). We multiplied these two values to obtain an overall index of the perceived risk of infection (see also Glöckner, Dorrough, Wingen, et al., 2020). Participants moreover indicated their estimated probability that they will become infected with the coronavirus requiring hospitalization (0 to 100%) and how severe the consequences of such an infection would be (1 = *no negative consequences*, 10 = *extreme negative consequences*). We multiplied these two measures to obtain an overall index of the perceived risk of infection requiring hospitalization. The overall index of the perceived risk of infection and the overall index of the perceived risk of infection requiring hospitalization were highly correlated, $r(562) = .76$, $p < .001$, CI [.72, .79] (see also Table S3).

Moreover, we assessed susceptibility to coronavirus as measure of unrealistic optimism (Brewer et al., 2007; Weinstein, 1982); participants were asked about the risk of becoming infected with the coronavirus compared to another person of the same age and gender living in Germany (-3 = *lower risk*, 0 = about the same, +3 = *higher risk).* One additional item measured global perceived risk, but because it was strongly correlated with the estimated probability to become infected, it is not discussed further (but see OSF).

Demographics. We collected participants' gender, age, household income, job status, and education. Measures for income, job status, and education were combined to form one index of socioeconomic status, ranging from 3 to 19 (Winkler-Index, Emrich et al., 2018; Winkler & Stolzenberg, 2009). We moreover assessed participants' political orientation (1 = *left*, 10 = *right*), whether they lived in Germany, in which state they lived, and whether they had children. Regarding health, we assessed their subjective health status (1 = *very bad*, 5 = *very good*), whether participants belonged to a vulnerable risk group, whether close relatives belonged to a risk group, whether participants were or had been infected with the coronavirus, and finally whether participants were or had been suspected to be infected. The demographic characteristics of the sample are summarized in Table S1.

Careless Responses. Participants indicated on three items (taken from Meade & Craig, 2012) how much effort they invested in the study (1 = *almost no*, 5 = *very much*), how much attention they paid to the study (1 = *almost no*, 5 = *my full*), and finally, whether we should include their data in our analyses. Participants were excluded from analyses when they answered "no" to this last question.

2.4.2 RESULTS

Descriptive Results for Acceptance and Adoption of Measures. We observed a remarkable acceptance of behaviour-based protective measures ($M = 6.48$, $SD = 0.72$, on a 7-point scale; see Figure S1 for detailed results per item). Moreover, participants reported that they adopted protective measures often ($M = 4.44$, $SD = 0.41$, on a 5-point scale; see Figure S2). Shutdown and governmental restrictions were also widely accepted ($M = 6.06$, $SD = 1.00$, on a 7-point scale; see Figure S3).

Predictors of Acceptance and Adoption of Measures. Correlation analyses revealed several associations between the variables (see Table S3 and Figure S4). For acceptance of protective measures, the highest correlations were found with trust in science, $r(567) = .34$, $p < .001$, 95% CI [.27, .41], and trust in politics, $r(568) = .35$, $p < .001$, 95% CI [.27, .42]. Moreover, age, health status, and perceived risk of a serious infection requiring hospitalization were associated with acceptance, but the observed correlations are conventionally considered as small (Cohen, 1988). For adoption of protective measures, the highest correlations were found for trust in science, $r(566) = .26$, $p < .001$, 95% CI [.18, .33], and trust in politics, $r(567) = .31$, $p < .001$, 95% CI [.23, .38]. Small to medium correlations were observed between adoption of measures and age, socioeconomic status, political orientation, and perceived risk of a serious infection requiring hospitalization. Finally, the perceived risk of infection showed a very small but significant correlation with adoption of measures.

We moreover investigated group differences regarding acceptance and adoption of measures (see Table S4 for details). We compared parents with childless participants, female with male participants, and finally, participants belonging to the risk group with low-risk participants. Parents ($p = .016$) and female participants ($p = .019$) showed a significantly higher acceptance of behavioural measures, but the observed differences are conventionally considered as small (Cohen, 1988). We moreover observed slightly higher levels of adoption of measures for female participants ($p = .037$), parents ($p = .002$), and participants belonging to the risk group ($p = .034$).

Hierarchical Regression Analyses. Hierarchical regression analyses tested which variables were most relevant for predicting acceptance (Table 2.1) and adoption (Table 2.2) of protective measures. In the first step, sociodemographic variables (gender, SES, age, parental status, and being part of a risk group) were entered into the models. In the second step, risk susceptibility, perceived risk of infection, and risk of hospitalization were added. Finally, trust in politics and trust in science were added to the models in a third step. Explained variance (R^2) was highest for models including trust measures to predict acceptance (23%) and adoption of measures (21%). Results further indicated that entering trust in politics and trust in science made a significant, unique contribution to the prediction of acceptance and adoption of protective measures (acceptance: $\Delta R^2 = 0.16$, $p < .001$; adoption of measures: $\Delta R^2 = 0.11$, $p < .001$).

As depicted in Table 2.1, being female, having children, being part of a low-risk group, higher perceived risk of hospitalization, and higher trust in politics and science were related to higher acceptance of measures. For adoption of protective measures (Table 2.1), being female, having children, lower risk susceptibility, higher perceived risk of hospitalization, and higher trust in politics and science were significant predictors. Perceived risk of hospitalization, trust in politics, and trust in science were the strongest predictors for acceptance and adoption of protective measures.

Table 2.1

Hierarchical Regression Analysis to Predict Acceptance of Protective Measures from Sociodemographic Variables, Risk Perception, and Trust in Science and Politics (Study 1)

Step and predictor variable	*B*	*SE B*	β	R^2	ΔR^2
Step 1:				$.03^{**}$	
Gender (0 = *male*, 1 = *female*)	0.17	0.07	$.11^{*}$		
Socioeconomic Status	0.00	0.01	.01		
Age	0.00	0.00	.08		
Parental Status (0 = *no children*, 1 = *children*)	0.06	0.07	.05		
Risk Group (0 = *low risk*; 1 = *high risk*)	-0.19	0.07	$-.12^{**}$		
Step 2:				$.06^{***}$	$.03^{**}$
Gender (0 = *male*, 1 = *female*)	0.17	0.07	$.11^{*}$		
Socioeconomic Status	0.00	0.01	.03		
Age	0.00	0.00	.07		
Parental Status (0 = *no children*, 1 = *children*)	0.05	0.07	.04		
Risk Group (0 = *low risk*; 1 = *high risk*)	-0.31	0.08	$-.19^{***}$		
Risk Susceptibility	-0.04	0.02	-.08		
Risk of Infection	0.00	0.00	.03		
Risk of Hospitalization	0.00	0.00	$.17^{*}$		
Step 3:				$.23^{***}$	$.16^{***}$
Gender (0 = *male*, 1 = *female*)	0.17	0.06	$.11^{**}$		
Socioeconomic Status	0.00	0.01	.01		
Age	0.00	0.00	.03		
Parental Status (0 = *no children*, 1 = *children*)	0.16	0.07	$.12^{*}$		
Risk Group (0 = *low risk*; 1 = *high risk*)	-0.23	0.07	$-.14^{**}$		
Risk Susceptibility	-0.03	0.02	-.08		
Risk of Infection	0.00	0.00	.01		
Risk of Hospitalization	0.00	0.00	$.20^{**}$		
Trust in Politics	0.08	0.03	$.16^{**}$		
Trust in Science	0.17	0.03	$.28^{***}$		

Note. $^{*} p < .05$. $^{**} p < .01$. $^{***} p < .001$.

Table 2.2

Hierarchical Regression Analysis to Predict Adoption of Protective Measures from Sociodemographic Variables, Risk Perception, and Trust in Science and Politics (Study 1)

Step and predictor variable	*B*	*SE B*	β	R^2	ΔR^2
Step 1:				.05***	
Gender (0 = *male*, 1 = *female*)	0.09	0.04	.11*		
Socioeconomic Status	0.01	0.00	.07		
Age	0.00	0.00	.10		
Parental Status (0 = *no children*, 1 = *children*)	0.03	0.04	.04		
Risk Group (0 = *low risk*; 1 = *high risk*)	0.05	0.04	.05		
Step 2:				.09***	.05***
Gender (0 = *male*, 1 = *female*)	0.10	0.04	.12**		
Socioeconomic Status	0.01	0.00	.09		
Age	0.00	0.00	.09		
Parental Status (0 = *no children*, 1 = *children*)	0.03	0.04	.04		
Risk Group (0 = *low risk*; 1 = *high risk*)	0.00	0.04	.00		
Risk Susceptibility	-0.04	0.01	-.17***		
Risk of Infection	0.00	0.00	-.01		
Risk of Hospitalization	0.00	0.00	.21**		
Step 3:				.21***	.11***
Gender (0 = *male*, 1 = *female*)	0.10	0.03	.12**		
Socioeconomic Status	0.01	0.00	.07		
Age	0.00	0.00	.05		
Parental Status (0 = *no children*, 1 = *children*)	0.08	0.04	.10*		
Risk Group (0 = *low risk*; 1 = *high risk*)	0.04	0.04	.04		
Risk Susceptibility	-0.04	0.01	-.17***		
Risk of Infection	0.00	0.00	-.03		
Risk of Hospitalization	0.00	0.00	.23***		
Trust in Politics	0.06	0.02	.21***		
Trust in Science	0.06	0.02	.17**		

Note. * $p < .05$. ** $p < .01$. *** $p < .001$.

2.4.3 DISCUSSION

Study 1 revealed important predictors of acceptance and adoption of protective measures. Sociodemographic factors (being older, female, and having children), as well as an increased perceived risk of a COVID-19 infection requiring hospitalization, were associated with increased acceptance and adoption. Yet, the most important predictors were trust in politics and trust in science, which had a notable effect on acceptance and adoption, going beyond the effects of sociodemographic factors and risk perception.

Given the exploratory nature of Study 1, we aimed to replicate central findings in a preregistered second study, using a sample representative to the German adult population. Moreover, Study 2 was conducted a month later and thus tested whether our findings from Study 1 also generalize to a later stage of the pandemic.

2.5 STUDY 2

2.5.1 METHODS

Design and Participants. We conducted a cross-sectional study using a sample representative to the German adult population in terms of age and gender (N = 301, M_{age} = 50.06, SD_{age} = 16.15, 52% female, see also sample characteristics in Table S5). Data was collected as part of the COVID-19 battery of the Social Cognition Center Cologne (see https://osf.io/2w58s/?view_only=465aa85be8b54295a8a070b0af4a857e). The battery included further projects that studied conspiracy beliefs about COVID-19 (Imhoff & Lamberty, 2020), probability estimates and risk perception (Glöckner, Dorrough, Wingen, et al., 2020), the role of ambivalence (Schneider & Dorrough, 2020) and personality (Glöckner, Dorrough, Michels, et al., 2020) for adherence to measures, and the role of responsibility on prosocial behaviour (Dorrough et al., 2020). The battery of studies took participants 25 min to complete. Hypotheses for the present study were preregistered at https://aspredicted.org/blind.php?x=67u7z7, and analyses were conducted in line with the preregistration. For directed hypotheses, we apply one-sided tests and explicitly note these. For all other comparisons, we use two-sided tests (with $p < .05$). No participant was excluded from the data analyses, in line with our preregistration. A sensitivity analysis showed that this final sample had a very high chance ($\beta = .95$, $\alpha = .05$, $N = 301$) to detect correlations of $r = .21$.

Data was collected between April 29, 2020 and May 4, 2020, so roughly one month after Study 1 was completed. In the meantime, Germans had lived under severe government restrictions (e.g., closing of non-essential businesses and institutions, contact restrictions) and the COVID-19 pandemic rose to 163.175 cases and 6.692 deaths (Robert Koch Institute, 2020). This constitutes an increase of 135.739 cases (495%) and 6.578 deaths (5770%) compared to Study 1.

Procedure. Participants were invited to a survey by the online panel provider Toluna, a professional recruitment platform that provides access to population-representative samples (https://de.toluna.com/#/). Participants received a link to the survey, hosted on the online survey system Unipark. After participants consented, the platform automatically guided participants through the study and provided them with all measures. At the end of the survey, participants were provided with contact details for further questions.

Measures. We aimed to include all measures form Study 1. Due to time restrictions, some measures had to be shortened and some secondary measures had to be dropped. In the following, we briefly present all measures that we used in Study 2.

We presented participants the same eleven items from Study 1 to measure acceptance of behaviour-based protective measures, and one additional item asking about the use of face masks ($\alpha = .94$). Participants then reported their adoption of these measures by indicating how often they engage in each behaviour ($\alpha = .88$). We also measured participants' acceptance of the shutdown and of governmental restrictions and added one additional item asking about the use of tracking apps, to include a more recently discussed measure ($\alpha = .95$).

We selected two items to measure participants trust in politics during the pandemic ("Information released by German politicians concerning the coronavirus can be trusted."; "The skills of important decision makers in politics are sufficient to overcome this crisis.") and their respective adapted versions to measure trust in science. Items were selected because they all showed a very high correlation with the overall trust in politics and trust in science scores, respectively (all *r*s > .82).

Participants completed the same items from Study 1 measuring their estimated probability that they will become infected with the coronavirus by the end of the year, the severity of the consequences of such an infection (which we again multiplied with each other to indicate perceived risk of infection), their estimated probability that they will become infected with the coronavirus requiring hospitalization and how severe the consequences of such an infection would be (which we again multiplied with each other to assess risk of hospitalization) and their risk susceptibility. There was one minor deviation compared to Study 1, in that the consequences of infection and hospitalization were each assessed on an 11-point scale (0 = *no negative consequences,* 10 = *extreme negative consequences*). As in Study 1, the index of the perceived risk of infection and the index of the perceived risk of infection requiring hospitalization were highly correlated, $r(298) = .79$, $p < .001$, CI [.74, .83] (see also Table S6).

Demographics. We again collected participants' gender, age, household income, job status, and education. We moreover assessed participants' political orientation and whether they had children. Regarding health, we assessed whether participants belonged to a vulnerable risk group and whether participants were infected with the coronavirus (for a summary, see Table S5).

2.5.2 RESULTS

Descriptive Results for Acceptance and Adoption of Measures. We again observed a high acceptance of behaviour-based protective measures (*M* = 6.11, *SD* = 1.06, on a 7-point scale), and participants adopted protective measures often (*M* = 4.34, *SD* = 0.63, on a 5-point scale). Acceptance of the shutdown and governmental restrictions was also high (*M* = 5.34, *SD* = 1.37, on a 7-point scale). Detailed results per item are presented in Figures 2.1, 2.2, and S5, respectively.

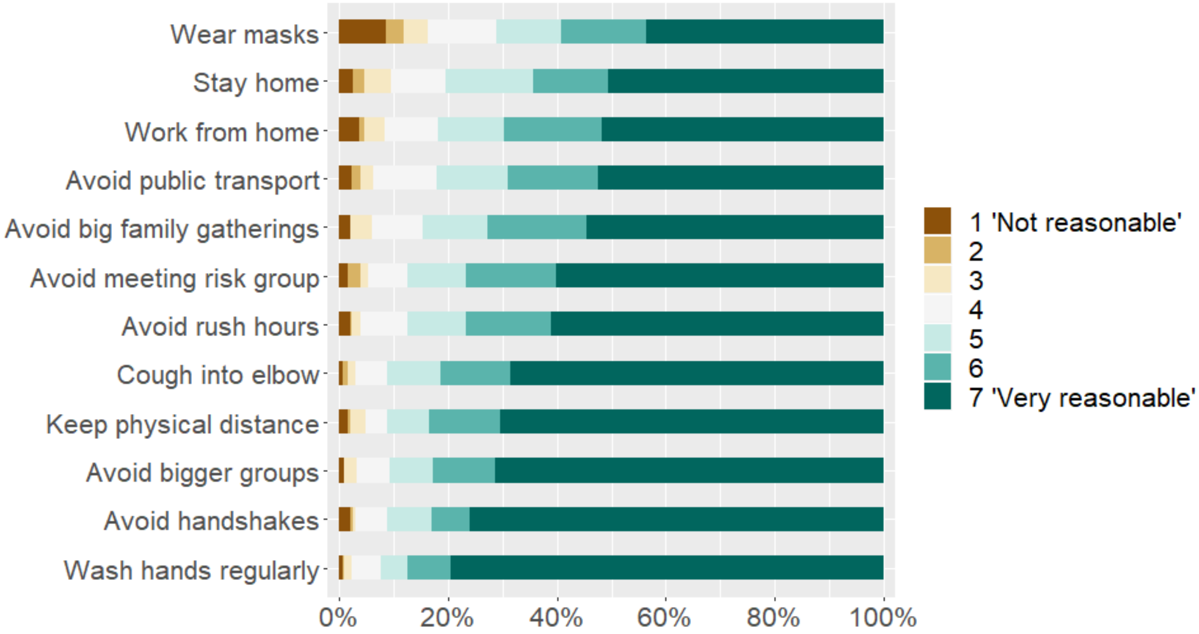

Figure 2.1. Stacked bar chart presenting acceptance of protective measures (Study 2). Note. Measures are sorted by the percentage of participants who selected "very reasonable" in ascending order.

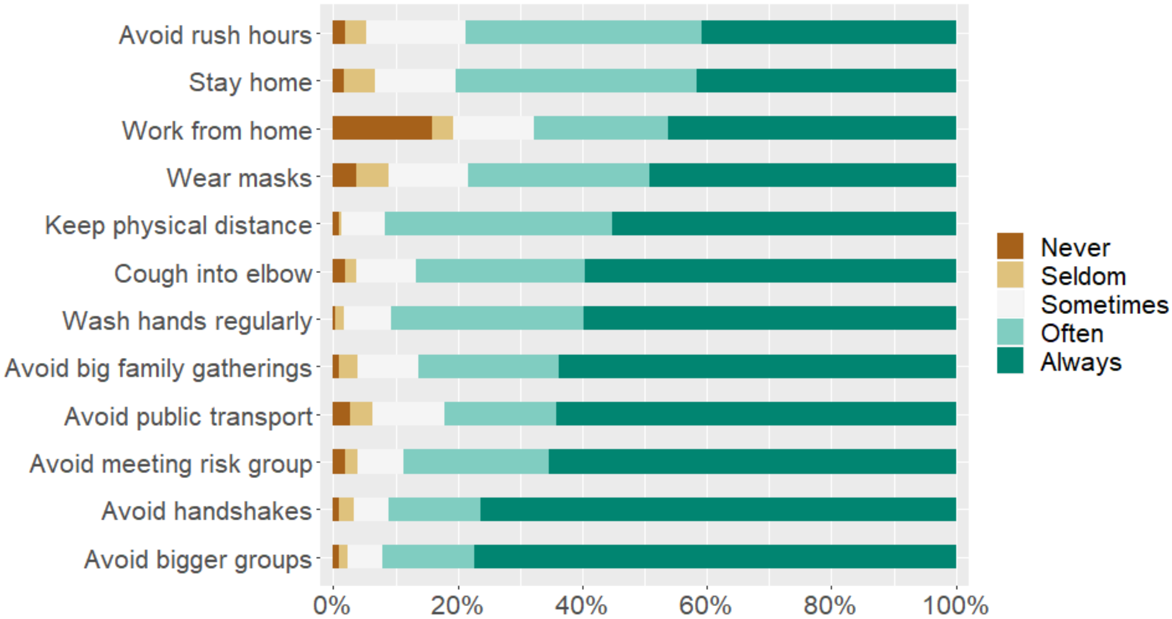

Figure 2.2. Stacked bar chart presenting adoption of protective measures (Study 2).. Note. Measures are sorted by the percentage of participants who selected "Always" in ascending order.

Development over Time – Combined Analyses. Even though acceptance and adoption were overall still high in our second study, they were reduced compared to the levels observed in our first study. Multiple linear regressions controlling for sample differences (age, gender, risk group status, parental status, and SES) and including only those items that were measured at both time points revealed that these differences were significant for all investigated dimensions: acceptance of protective measures ($p < .001$), adoption of measures ($p < .001$), and finally acceptance of the shutdown and governmental restrictions ($p < .001$). Detailed results for these analyses are presented in Table S7.

Predictors of Acceptance and Adoption of Measures. Correlation analyses revealed several associations between the variables (see Table S6 and Figure 2.3). For acceptance, the highest correlation was found for trust in science, $r(299) = .46$, $p < .001$, 95% CI [.36, .54], trust in politics, $r(299) = .35$, $p < .001$, 95% CI [.25, .45], and age, $r(299) = .35$, $p < .001$, 95% CI [.25, .45]. Moreover, perceived risk of an infection, perceived risk of a serious infection requiring hospitalization, and political orientation were also associated with acceptance, but the observed correlations are conventionally considered small to medium (Cohen, 1988). For adoption of protective measures, the highest correlation was found for trust in science, $r(299) = .37$, $p < .001$, 95% CI [.27, .47], trust in politics, $r(299) = .30$, $p < .001$, 95% CI [.20, .40], and age, $r(299) = .31$, $p < .001$, 95% CI [.21, .41]. Small to medium correlations were observed between adoption and perceived risk of an infection, perceived risk of a serious infection requiring hospitalization, risk susceptibility, and political orientation.

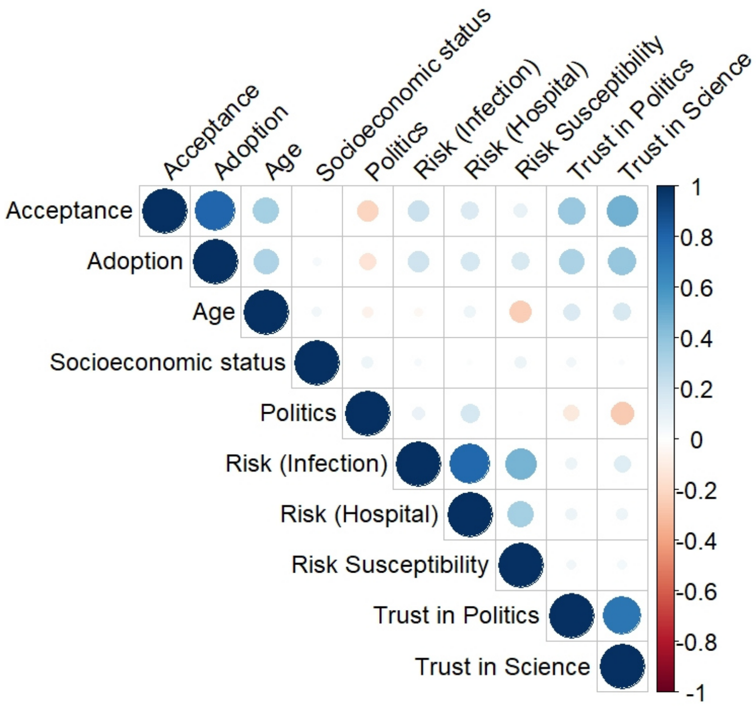

Figure 2.3. Graphical overview of zero-order Pearson correlations between measures (Study 2).

We moreover investigated potential group differences regarding acceptance and adoption of measures (see Table S8). As preregistered, we hypothesized that parents (compared to childless participants) and women (compared to men) would show a significantly higher acceptance, as in Study 1. In line with our preregistration, we found that women (p = .027, one-sided test for directed hypothesis) had a higher acceptance, but parental status had no influence. We also found that participants belonging to the risk group indicated increased acceptance (p < .001) of behavioural measures, compared to low-risk participants. For adoption of measures, we hypothesized that women, parents, and individuals belonging to the risk group would show higher values. The results showed that parents (p = .027, one-sided) and participants belonging to the risk group (p <.001, one-sided) showed higher adoption of protective measures, but the hypothesis was not confirmed for gender.

Hierarchical Regression Analyses. For the hierarchical regression analyses, we hypothesized that all steps (the same as in Study 1) will make a unique contribution to the prediction of acceptance and adoption of protective measures, as indicated by significant changes in R^2. We also hypothesized that the increase in R^2 will be largest when adding trust in politics and trust in science to the model. As predicted, all steps made a unique contribution to the prediction of acceptance and adoption of protective measures and resulted in significant changes in R^2. Entering trust in politics and trust in science lead to a stronger increase in R^2 compared to the inclusion of risk-related measures (acceptance: $\Delta R^2 = 0.14$, $p < .001$; adoption of measures: $\Delta R^2 = 0.09$, $p < .001$). As depicted in Table 2.3, being female, higher age, and trust in science (but not trust in politics) were related to higher acceptance of measures. For the adoption of protective measures (Table 2.4), being female, higher age, higher risk susceptibility, and higher trust in science (but not trust in politics) were significant positive predictors.

Table 2.3

Hierarchical Regression Analysis to Predict Acceptance of Protective Measures from Sociodemographic Variables, Risk Perception, and Trust in Science and Politics (Study 2)

Step and predictor variable	*B*	*SE B*	β	R^2	ΔR^2
Step 1:				$.17^{***}$	
Gender (0 = *male*, 1 = *female*)	0.45	0.12	$.21^{***}$		
Socioeconomic Status	0.02	0.02	.07		
Age	0.02	0.00	$.30^{***}$		
Parental Status (0 = *no children*, 1 = *children*)	-0.09	0.12	-.04		
Risk Group (0 = *low risk*; 1 = *high risk*)	0.35	0.15	$.17^{*}$		
Step 2:				$.22^{***}$	$.05^{**}$
Gender (0 = *male*, 1 = *female*)	0.42	0.12	$.20^{***}$		
Socioeconomic Status	0.01	0.02	.04		
Age	0.02	0.00	$.36^{***}$		
Parental Status (0 = *no children*, 1 = *children*)	-0.10	0.12	-.05		
Risk Group (0 = *low risk*; 1 = *high risk*)	0.27	0.15	.13		
Risk Susceptibility	0.06	0.05	.08		
Risk of Infection	0.00	0.00	$.26^{**}$		
Risk of Hospitalization	0.00	0.00	-.13		
Step 3:				$.36^{***}$	$.14^{***}$
Gender (0 = *male*, 1 = *female*)	0.46	0.11	$.22^{***}$		
Socioeconomic Status	0.01	0.01	.03		
Age	0.02	0.00	$.31^{***}$		
Parental Status (0 = *no children*, 1 = *children*)	0.00	0.11	.00		
Risk Group (0 = *low risk*; 1 = *high risk*)	0.16	0.14	.08		
Risk Susceptibility	0.05	0.04	.07		
Risk of Infection	0.00	0.00	.16		
Risk of Hospitalization	0.00	0.00	-.06		
Trust in Politics	0.01	0.04	.02		
Trust in Science	0.27	0.05	$.38^{***}$		

Note. $^{*}p < .05$. $^{**}p < .01$. $^{***}p < .001$.

Table 2.4

Hierarchical Regression Analysis to Predict Adoption of Protective Measures from Sociodemographic Variables, Risk Perception, and Trust in Science and Politics (Study 2)

Step and predictor variable	*B*	*SE B*	β	R^2	ΔR^2
Step 1:				.13***	
Gender (0 = *male*, 1 = *female*)	0.22	0.07	.18**		
Socioeconomic Status	0.01	0.01	.08		
Age	0.01	0.00	.29***		
Parental Status (0 = *no children*, 1 = *children*)	0.01	0.07	.00		
Risk Group (0 = *low risk*; 1 = *high risk*)	0.11	0.09	.09		
Step 2:				.18***	.05**
Gender (0 = *male*, 1 = *female*)	0.21	0.07	.17**		
Socioeconomic Status	0.01	0.01	.05		
Age	0.01	0.00	.37***		
Parental Status (0 = *no children*, 1 = *children*)	-0.01	0.07	-.01		
Risk Group (0 = *low risk*; 1 = *high risk*)	0.05	0.09	.04		
Risk Susceptibility	0.07	0.03	.16*		
Risk of Infection	0.00	0.00	.15		
Risk of Hospitalization	0.00	0.00	-.06		
Step 3:				.27***	.09**
Gender (0 = *male*, 1 = *female*)	0.23	0.07	.19**		
Socioeconomic Status	0.01	0.01	.04		
Age	0.01	0.00	.33***		
Parental Status (0 = *no children*, 1 = *children*)	0.04	0.07	.03		
Risk Group (0 = *low risk*; 1 = *high risk*)	0.00	0.09	.00		
Risk Susceptibility	0.06	0.03	.15*		
Risk of Infection	0.00	0.00	.08		
Risk of Hospitalization	0.00	0.00	-.01		
Trust in Politics	0.01	0.03	.03		
Trust in Science	0.12	0.03	.30***		

Note. * $p < .05$. ** $p < .01$. *** $p < .001$.

2.5.3 Discussion

Acceptance and adoption of protective measures were still high in Study 2 but significantly reduced compared to Study 1, which was conducted one month earlier. Even at this later point in time, however, trust in science strongly affected whether people accepted and adopted protective measures. Moreover, hierarchical regression analyses as well as the combined analyses across studies demonstrated that older and female individuals were more likely to accept and adopt protective measures.

2.6 General Discussion

Our research identified important predictors of acceptance and adoption of protective measures during the COVID-19 pandemic. Across studies, women and older participants showed higher acceptance and adoption. Besides these sociodemographic variables, risk perception, but especially trust in politics and trust in science emerged as important predictors of the acceptance and adoption of protective measures.

Higher fatality rates for COVID-19 have been reported for older individuals, but also for men (e.g., Livingston & Bucher, 2020). Nevertheless, men seem to adopt protective measures less frequently than women, in line with previous research showing that men usually show less health-promoting behaviours and more health-risk behaviours (Bish & Michie, 2010; Helgeson, 2017). In many countries, there are also differences in health behaviours and long-term health outcomes between people of different socioeconomic positions (e.g., Petrovic et al., 2018), but we found no indication that socioeconomic status was related to acceptance or adoption of measures. For health communication during the COVID-19 pandemic, these results suggest that it is important to develop target-group specific interventions focusing on men and younger individuals. For men, this could include highlighting their increased individual risks, whereas for younger individuals these messages could focus on the societal consequences of their behaviour.

The three indicators of risk perception (risk of infection, risk of hospitalization, and risk susceptibility) revealed a mixed picture. In Study 1, risk of hospitalization, but not risk of infection was related to higher acceptance and adoption of the measures in the hierarchical regression analysis, but this pattern was not observed in Study 2. Risk susceptibility was even negatively related to adoption of measures in Study 1, indicating that participants who felt less susceptible compared to others of the same age and gender reported more protective behaviours. This pattern was not observed in Study 2; here, participants who felt more susceptible showed more protective behaviours. These differences might be due to unobserved sample characteristics or actual changes over time, but it is also possible that participants in Study 1 considered themselves not to be vulnerable because they already showed protective behaviours.

Both trust in politics and trust in science were important predictors in the earlier study, but only trust in science was a significant predictor in the hierarchical regression in Study 2. The outbreak of COVID-19 may have undermined people's confidence that responsible politicians could control the disease, pointing to the complex interplay of trust and risk management strategies (Siegrist & Zingg, 2014). The predictive power of trust in science was also somewhat stronger in Study 2 compared to Study 1, which could be due to the fact that ceiling effects in the outcome measures were less pronounced in Study 2 (see also limitations below). Comparisons between Study 1 and 2, however, should be carried out cautiously as sample recruitment and composition differed.

Given that our studies point to the important role of trust, the question arises how trust can be fostered. Trust is difficult to create (Slovic, 1999), and many strategies to build trust can only be achieved in the long term. For example, education (Bak, 2001; Hayes & Tariq, 2000) and science knowledge (Evans & Durant, 1995) are positively associated with trust in science. Other research has stressed that media use may play an important role: Heavy TV viewing is negatively correlated with trust in science (e.g., Gerbner, 1987), whereas using traditional news and social media is positively correlated to trust in science (Huber et al., 2019). Finally, trust in science also results from replicable research findings (Anvari & Lakens, 2018; Hendriks et al., 2020; Wingen et al., 2020). Despite the need for quick answers, researchers studying the current pandemic should thus be especially careful not to publish potentially unreliable findings. In the short term, trust in science and politics might be best bolstered by effective crisis communication. Focusing on trust and crisis communication during pandemics, Siegrist and Zingg (2013) recommended (i) that uncertainties about what is known and what is unknown about a given disease are transparently addressed, (ii) that role models adopt the recommendations for fighting the pandemic, and (iii) that heterogeneous sets of experts should unanimously communicate about the effectiveness of the recommended behaviours.

Some limitations regarding our research need to be acknowledged. First, adoption of protective measures was self-reported, and actual levels of the behaviour may be lower due to reporting bias and social-desirability bias (Bish & Michie, 2010). Second, acceptance and adoption of protective measures showed ceiling effects in both studies. Due to the restricted variance in these measures, it is likely that we underestimated correlations and regression coefficients. For future research, it is therefore important to develop measures that allow for a greater variability in responses. Third, it is important to note that many researchers consider trust as a multidimensional construct, although views differ on how many dimensions are sufficient to describe the construct (Allum, 2007; Hendriks et al., 2015; Johnson, 1999; Mayer et al., 1995; Siegrist, 2020; Terwel et al., 2009). Seminal work by Mayer and colleagues (1995) has identified expertise, integrity, and benevolence as crucial dimensions of trust, and empirical evidence supports these three dimensions (e.g., Hendriks et al., 2015). Building on this research, future studies should take a more systematic approach to capture trust in a multidimensional way. This would also allow conclusions to be drawn as to which dimensions of trust are most strongly linked to protective behaviours in pandemics. Fourth, although the predictors in the hierarchical models explained considerable variance in the outcome variables, it is likely that additional important predictors exist. However, we aimed to parsimoniously explain acceptance and adoption and adding additional predictors would have compromised this aim. The perhaps most important limitation is that the cross-sectional design of this study does not allow to test causal relations. The overserved correlational associations should thus be considered as a tentative hint for potential causal relationships. Yet, especially the link between trust and increased engagement in prosocial behaviour is well documented in general (Ferrin et al., 2008) and especially during pandemics (Siegrist & Zingg, 2014), and thus, it seems likely that this reflects a causal relationship. Nevertheless, future research should carry out longitudinal or experimental designs to investigate the causal effects of trust in science and politics and acceptance and adoption of protective measures during a pandemic. Furthermore, it has been demonstrated that trust in science varies in the context of specific topics (Hendriks et al., 2016). It is likely that in the context of COVID-19, where science may help to overcome a crisis, trust in science is much higher compared to a context where potential risks arise from science and technology (e.g. nuclear energy). As a result, the influence of trust in science (and also of trust in politics) on the acceptance of behavioural measures will vary according to the context that is studied.

2.6.1 CONCLUSION

In sum, our research shows that trust is a key factor for the acceptance and adoption of protective measures during the COVID-19 pandemic. Trust in politics and science increases the probability that people will accept and implement protective measures, which can eventually lead to a reduction in infection and fatality rates. Thus, politicians or scientists have to be careful not to propagate any ineffective or even dangerous measures (e.g., Liu et al., 2020). Because the implementation of protective measures is directly related to infection and fatality rates, strong efforts should be taken to ensure that trust in politics and science is not undermined, and that those who are trusted give valid and sound recommendations on how to protect oneself and others.

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CHAPTER 3: NO REPLICATION, NO TRUST? HOW LOW REPLICABILITY INFLUENCES TRUST IN PSYCHOLOGY

This chapter is based on the following article:

Wingen, T., Berkessel, J. B., & Englich, B. (2020). No replication, no trust? How low replicability influences trust in psychology. *Social Psychological and Personality Science*, *11*(4), 454-463. https://doi.org/10.1177/1948550619877412

Please note that some changes in formatting were undertaken to fit the layout of this dissertation. No changes were made to the content of the article.

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ABSTRACT

In the current psychological debate, low replicability of psychological findings is a central topic. While the discussion about the replication crisis has a huge impact on psychological research, we know less about how it impacts public trust in psychology. In this paper, we examine whether low replicability damages public trust and how this damage can be repaired. Studies 1, 2 and 3 provide correlational and experimental evidence that low replicability reduces public trust in psychology. Additionally, Studies 3, 4, and 5 evaluate the effectiveness of commonly used trust-repair strategies, such as information about increased transparency (Study 3), explanations for low replicability (Study 4), or recovered replicability (Study 5). We found no evidence that these strategies significantly repair trust. However, it remains possible that they have small but potentially meaningful effects, which could be detected with larger samples. Overall, our studies highlight the importance of replicability for public trust in psychology.

Keywords: replicability, public trust, replication crisis, open science

3.1 INTRODUCTION

A trustworthy reputation is crucial for psychology. Researchers in psychology aim to have societal impact and to inform practitioners and policymakers. Additionally, psychological research relies on public funding and participation. In turn, when facing complex scientific questions, decision-makers seek advice from the (psychological) science community (Bromme & Thomm, 2016). If based on robust evidence, these well-informed decisions can lead to improved individual and societal outcomes (Ruggeri et al., 2018). Thus, public trust is not only crucial for psychology itself, but also for the public. What happens to this trust when psychological findings often fail to replicate?

This is a relevant question since many prominent studies indeed suggest a low replicability of psychological findings. For example, the *Reproducibility Project: Psychology* replicated 100 psychological studies and only about one-third to one-half of the original findings were replicated (Open Science Collaboration, 2015). This low replication rate often serves as an illustration for a “replication crisis” (e.g. Anderson & Maxwell, 2017).

3.1.1 LOW REPLICABILITY MIGHT DAMAGE PUBLIC TRUST IN PSYCHOLOGY

Many researchers see crises positively since they play a central role in the advancement of sciences and show that science self-corrects (Kuhn, 1970; Vazire, 2018). Indeed, in the course of the replication crisis, psychology has gone through major changes. Currently, journals and scientific societies encourage open science practices (e.g. Schönbrodt, Gollwitzer, & Abele-Brehm, 2017) and many researchers implement open science (Lindsay & Nosek, 2018). These changes are often seen as a major advancement of psychological science: Researchers argue that the rate of scientific progress is likely to increase (Vazire, 2018), that the widespread use of preregistration will increase the interpretability and credibility of research findings (Nosek, Ebersole, DeHaven, & Mellor, 2018), and that open science will liberate researchers and foster creativity (Frankenhuis & Nettle, 2018).

Nevertheless, there are reasons to assume that information about low replicability might damage public trust in psychology. While findings regarding the effects of (scientific) uncertainty on audience reactions are rather inconclusive (for a review, see van der Bles et al., 2019) some studies suggest that non-scientists react negatively to scientific uncertainty. For example, non-scientists who perceive scientific evidence as uncertain further perceive the corresponding research field as less valuable (Broomell & Kane, 2017). Likewise, even modest amounts of scientific dissent reduce public support for government policies and lead to disagreement with the scientific consensus (Aklin & Urpelainen, 2014). Similarly, low replicability might also result in reputational damage and diminished public trust (Białek, 2018; Chopik, Bremner, Defever, & Keller, 2018; Fanelli, 2018). We test this hypothesis in the present article.

3.1.2 How can Public Trust be Repaired?

If low replicability damages public trust, an important question for the psychological science community is if and how this damage can be repaired. We tested the following three theory-based and commonly used approaches to repair public trust.

Repairing trust through increased transparency. Transparency signals that there is nothing to hide and thus repairs trust (Bachmann, Gillespie, & Priem, 2015). Indeed, one major response to the replication crisis is the open science movement (Frankenhuis & Nettle, 2018; LeBel, Campbell, & Loving, 2017). Central aspects of this movement, such as preregistrations, open data, and open materials, aim to increase the transparency of psychological research (Miguel et al., 2014; Nosek et al., 2015). Thus, building on the idea that transparency can repair trust, information about the open science movement might help to repair public trust in psychology.

Repairing trust through explanations. The causes and responsibilities of a transgression are often not evident (Bachmann et al., 2015). Explanations of the causes of a transgression can help to repair trust by establishing a shared understanding of what and why the transgression happened (Bachmann et al., 2015; Dirks, Lewicky, & Zaheer, 2009). If low replicability violates the public expectations of reliable published findings, the public may perceive low replicability as a transgression. In this case, explanations for low replicability could be an effective trust repair strategy.

Considering the replication crisis, two major explanations emerged. Some scholars argue that questionable research practices (QRPs) are the main reason for the replication crisis (e.g. Sijtsma, 2016; Simmons, Nelson, & Simonsohn, 2011). Other scholars attribute failed replications to hidden moderators and the high context-sensitivity of psychological effects (e.g. Stroebe & Strack, 2014; Van Bavel, Mende-Siedlecki, Brady, & Reinero, 2016). While this debate has not been settled, it is an additional open question whether any of those explanations – QRPs vs. hidden moderators – would repair public trust damaged by low replicability.

Repairing trust by restoring the status quo. Trust can further be repaired by restoring the status quo, as norms and expectations are also restored (Dirks et al., 2009). Before the replication crisis, the majority of replications in psychology journals reported similar findings to their original studies (Makel, Plucker, & Hegarty, 2012) and it was thus likely assumed that psychology is highly replicable. To restore this status quo, psychological science would thus need to achieve high levels of replicability. Indeed, many new methodological standards in psychology aim at increasing replicability (Cook, Lloyd, Mellor, Nosek, & Therrien, 2018; Van Bavel et al., 2016). If those standards succeed, the status quo belief that psychology is a highly replicable science might be restored. Eventually, this increase in replicability might also lead to a restoration of public trust.

3.1.3 Overview of Studies

We conducted five studies to test whether low replicability damages public trust and if this damage can be repaired. Study 1 examined whether trust in psychology correlates with expected replicability. Study 2 experimentally tested whether low replicability causes reduced trust, which we replicated in Study 3. Moreover, Studies 3 to 5 tested different commonly used trust-repair strategies: information about increased transparency (Study 3), explanations for low replicability (Study 4), and information about increased replicability (Study 5). Participants who took part in one of our studies were not allowed to participate in subsequent studies. We relied on MTurk workers in all studies, since they are significantly more socio-economically and ethnically diverse, and presumably less likely to have prior knowledge of the replication crisis, compared with a student sample (Casler, Bickel, & Hackett, 2013).

We include all studies we conducted, and report all collected variables and all conditions included in the study designs across all studies. We preregistered all analyses presented in the manuscript (except for specifically highlighted correlations presented in Table 3.1), and we report all preregistered analyses in either the manuscript or the supplemental materials. We discuss the central preregistered hypotheses when introducing each study. All analyses with a preregistered hypothesis are accompanied by one-sided *p*-values. All participants who completed our studies were included in the analyses except if they met preregistered exclusion criteria. All materials, data, analyses syntaxes, and preregistrations are shared on https://osf.io/9ba28/?view_only=7f2edfc9b5f143beb5f86dfdc657d73d.

3.2 STUDY 1

Study 1 investigated which replication rate non-scientists assume for psychological studies and whether their expected replication rate correlates with their trust in psychology and their perceived value of psychological science. We expected positive correlations.

3.2.1 METHOD

Participants and design. Participants completed a short online study on Amazon's Mechanical Turk (MTurk) website for $0.50. The sample size was set to 266, based on an a priori power analysis for 95% power (one-sided α of .05) to detect a small to moderate effect of $r = .2$, that would be typical for similar social-psychological research (Richard, Bond Jr, & Stokes-Zoota, 2003). The final sample was slightly larger as is often the case in online studies and consisted of 271 participants (54.3% male; age: $M = 33.7$ years, $SD = 8.9$). No participants were excluded from the analyses. A sensitivity analysis showed that our final sample had a high chance ($1 - \beta = 0.80$, one-sided $\alpha = 0.05$) to detect a correlation of $r = .15$ and a very high chance ($1 - \beta = 0.95$, one-sided $\alpha = 0.05$) to detect $r = .20$.

Procedure. Participants read a short, jargon-free description of the *Reproducibility Project: Psychology (*for details, see https://osf.io/9ba28/?view_only=7f2edfc9b5f143beb5f86dfdc657d73d). Participants then guessed how many of these 100 original findings were successfully replicated.

Afterward, participants indicated their trust in psychology with five items (e.g., "I trust the psychological science community to do what is right; 1 = *strongly disagree*, 7 = *strongly agree*; α = .90; adapted from Nisbet et al., 2015). Although we conveniently call this measure "trust in psychology" throughout this manuscript, it is important to note that it was designed to measure institutional trust in the (psychological) science community (Nisbet et al., 2015). Alternatively, trust in psychology could for example also be conceptualized as trust in psychological findings (Anvari & Lakens, in press) or in the scientific methods used by psychologists. However, for non-scientists, the scientific community might be the most vivid aspect of psychology. Moreover, prior research showed that the used "trust in psychology"-measure is affected by dissonant science communication (Nisbet et al., 2015), so it could be particularly suitable to capture potential effects of (expected) low replicability.

Although this measure showed acceptable to excellent reliability in Studies 1-5, it showed a poor confirmatory model fit across most indices and studies. We believed this to likely be due to the reverse-coding of items and found that accounting for this drastically improves the model fit and does not change the pattern of our results (see supplemental materials).

As an additional dependent variable, we measured participants perceived value of psychological science with four items (e.g., "Please rate the societal benefit of research produced by psychological science."; $\alpha = .80$; 1 = *very low*, 5 = *very high*; adapted from Broomell & Kane, 2017). In all studies, perceived value showed a similar result pattern to trust in psychology. All preregistered analyses regarding perceived value can be found in the supplemental material. Finally, participants indicated whether they knew the results of the *Reproducibility Project: Psychology* and completed demographics.

3.2.2 Results

Eleven participants (4.0%) said they had heard of the *Reproducibility Project: Psychology* but only one participant reported to know the results. This participant, however, indicated an incorrect replication rate of 14 out of 100 studies. On average, participants estimated that 60.9 out of 100 studies could be replicated (*SD* = 22.9). Descriptive statistics for the "trust in psychology"-measure across all studies and conditions are presented in Table 3.1. As predicted, the higher participants estimated the replication rate, the more they trusted psychology, $r(268) = .329$, one-sided $p < .001$, 95% CI [.218, .431] (see Fig. 3.1). Perceived value showed similar results to trust in psychology (see supplemental materials).

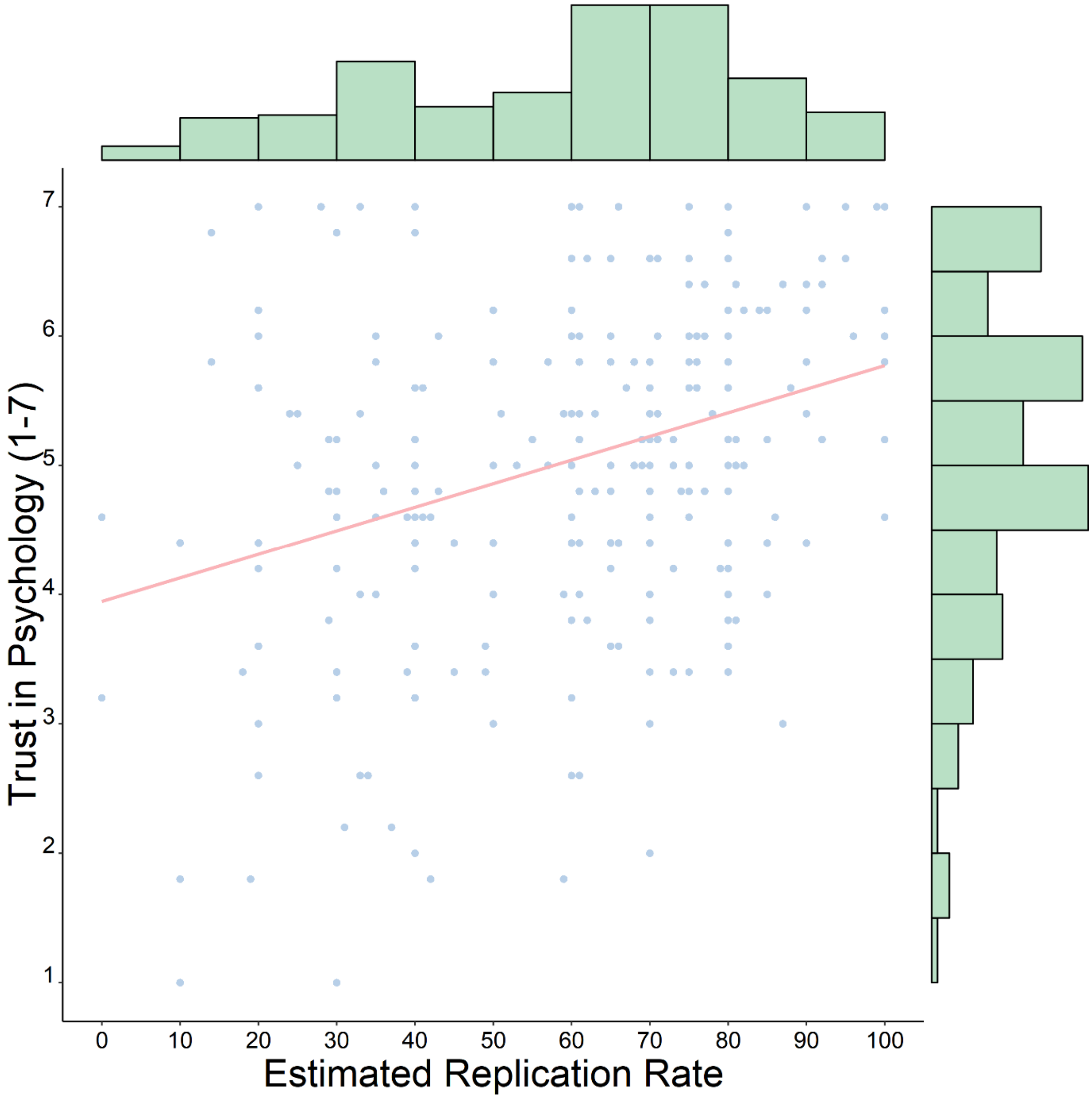

Figure 3.1. Relationship between the estimated replication rate and trust in psychology in Study 1. Histograms show the distribution of each measure.

3.3 STUDY 2

Study 1 provided correlational evidence that expected replicability is related to public trust in psychology. Building on Study 1, we employed an experimental approach to test causality. We expected low replicability (compared with high replicability) to reduce trust in psychology and to reduce the perceived value of psychological science.

3.3.1 METHOD

Participants and design. Participants completed a short online study on MTurk for \$0.50. We randomly assigned participants to three conditions (low replicability, medium replicability, high replicability). We set sample size to 264, based on an a priori power analysis for 95% power (one-sided α of .05) to find a moderate effect ($d = 0.5$), that would be typical for similar social psychological research (Richard et al., 2003), requiring 88 participants per cell (the same power analysis was applied to Studies 3, 4, and 5). The final sample consisted of 269 participants (59.9% male; age: $M = 34.59$ years, $SD = 10.74$). No participants were excluded from the analyses. A sensitivity analysis showed that our final sample had a high chance ($1 - \beta = 0.80$, one-sided $\alpha = 0.05$) to detect a difference of $d = 0.37$ between the low replicability and any of the two other conditions and a very high chance ($1 - \beta = 0.95$, one-sided $\alpha = 0.05$) to detect $d = 0.50$.

Procedure. Participants read the same description of the *Reproducibility Project: Psychology* as in Study 1. This time, however, participants received information about the results. Depending on their condition, participants were told that out of the 100 investigated studies, 39 (low replicability condition), 61 (medium replicability condition) or 83 (high replicability condition) could be successfully replicated. These values were based on the estimated replication rates found in Study 1: 61 represents the mean estimated replication rate in Study 1, and 39 and 83 represent the mean plus/minus one standard deviation in Study 1. Afterward, participants responded to three text-understanding items and a manipulation check

("Psychological research is replicable"; 1 = *strongly disagree*, 7 = *strongly agree*). Then, they filled out the five items from Study 1 to measure trust in psychology ($\alpha = .92$). We also measured participants' perceived value of psychological science and various individual differences as preregistered potential moderators (beliefs about science, error culture, error attribution style; see supplemental materials for details). Finally, participants completed a brief demographic questionnaire and were debriefed.

3.3.2 RESULTS

The manipulation check suggested that the manipulation was successful (see supplemental materials). A one-way analysis of variance revealed significantly different levels of trust in psychology between the three conditions, $F(2, 265) = 4.86$, $p = .008$, $\eta^2 = .04$, 90%-CI [.01, .07], see Figure 3.2. Participants in the low replicability condition indicated a significantly lower trust in psychology than participants in the high replicability condition, $t(176) = 3.25$, one-sided $p < .001$, $d = 0.49$, 95%-CI [0.19, 0.79].

Further analyses indicated that participants in the exploratory medium replicability condition tended to be more trustful than the participants in the low replicability condition, $t(176) = 1.48$, one-sided $p = .070$, $d = 0.22$, 95%-CI [-0.07, 0.52], and less trustful than participants in the high replicability condition, $t(176) = 1.59$, one-sided $p = .057$, $d = 0.24$, 95% CI-[-0.06, 0.53], but these differences were not significant. Perceived value showed similar results to trust in psychology (see supplemental materials).

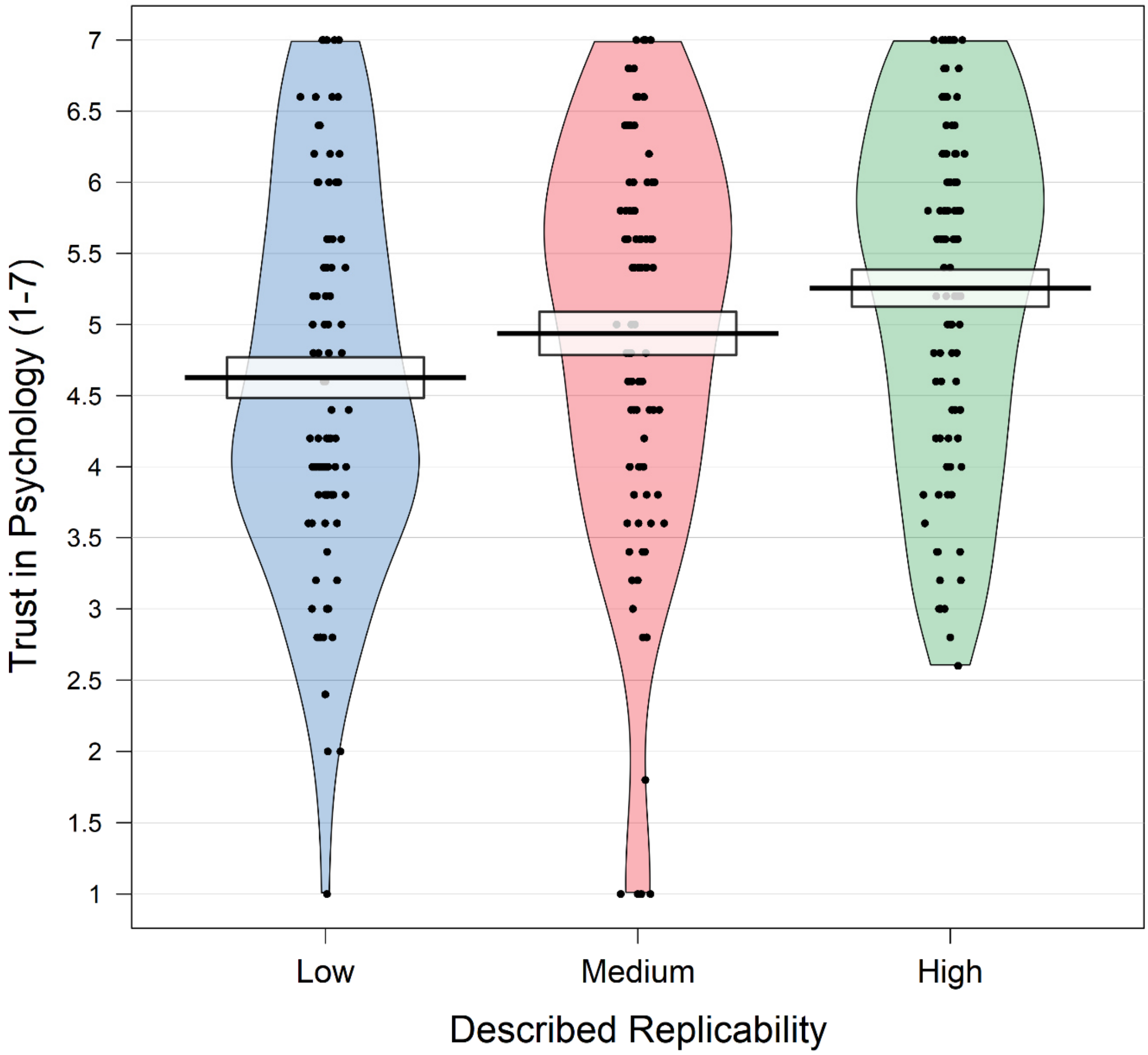

Figure 3.2. Pirate plot (Phillips, 2017) showing trust in psychology in the different replicability conditions. The black dots represent the raw data which is shown with smoothed densities indicating the distributions in each condition. The central tendency is the mean and the intervals represent two standard errors around the mean.

3.4 STUDY 3

As expected, Study 1 and 2 provided evidence that low replicability, compared with high replicability, reduces trust in psychology. In Study 3, we replicated the trust-damaging effect of low replicability and tested whether informing participants about the open science movement and increased transparency of psychological science would repair trust damaged by low replicability. We expected low replicability (compared with high replicability) to reduce trust in psychology (as in Study 2). Crucially, we expected information about increased transparency to repair public trust, compared with information about low replicability only.

3.4.1 METHOD

Participants and design. Three hundred and four participants were recruited to complete a short online study on MTurk for $0.60 each. Compared with Study 2, we increased the target sample size to 300 to compensate for potential exclusions. Indeed, seven participants were excluded for meeting the preregistered exclusion criteria (failing more than one text understanding questions). We randomly assigned participants to three conditions (low replicability, low replicability but transparency, high replicability). The final sample consisted of 297 participants (56.9% male; age: $M = 35.7$ years, $SD = 11.6$). A sensitivity analysis showed that our final sample had a high chance ($1 - \beta = 0.80$, one-sided $\alpha = 0.05$) to detect a difference of $d = 0.36$ between the low replicability and the low replicability but transparency condition and a very high chance ($1 - \beta = 0.95$, one-sided $\alpha = 0.05$) to detect $d = 0.47$.

Procedure. Participants read the same description of the *Reproducibility Project: Psychology* as in Study 2. Once again, participants were told that out of the investigated 100 studies, 39 (low replicability condition) or 83 (high replicability condition) could be successfully replicated. In a third condition (low replicability but transparency condition), participants were also told that 39 studies could be replicated, but that psychology has since then become much more open and transparent. This comprehensible information described major aspects of the open science movement, including preregistration, open data, and open materials, and highlighted that those measures contribute to increased transparency (for details see https://osf.io/9ba28/?view_only=7f2edfc9b5f143beb5f86dfdc657d73d).

Afterward, participants responded to three text-understanding items and two manipulation checks ("Psychological research is replicable."; "Psychological research is transparent."; 1 = *strongly disagree*, 7 = *strongly agree*). Then, they filled out the five items from Studies 1 and 2 to measure their trust in psychology ($\alpha = .86$). Participants also completed a brief demographic questionnaire and were debriefed.

3.4.2 RESULTS

The manipulation check suggested that the manipulations were successful (see supplemental materials).

As predicted, and replicating our prior findings, participants in the low replicability condition indicated a significantly lower trust in psychology than participants in the high replicability condition, $t(196) = 3.36$, one-sided $p < .001$, $d = 0.48$, 95%-CI [0.19, 0.76]; see Figure 3.3. However, contrary to our prediction, participants in the low replicability but transparency condition did not indicate a significantly higher trust in psychology than participants in the low replicability condition, $t(194) = 0.74$, one-sided $p = .231$, $d = 0.11$, 95%-CI [-0.18, 0.39].

Finally, participants in the low replicability but transparency condition indicated a significantly lower trust in psychology compared with participants in the high replicability condition, $t(192) = 2.60$, $p = .010$, $d = 0.37$, 95%-CI [0.09, 0.66].

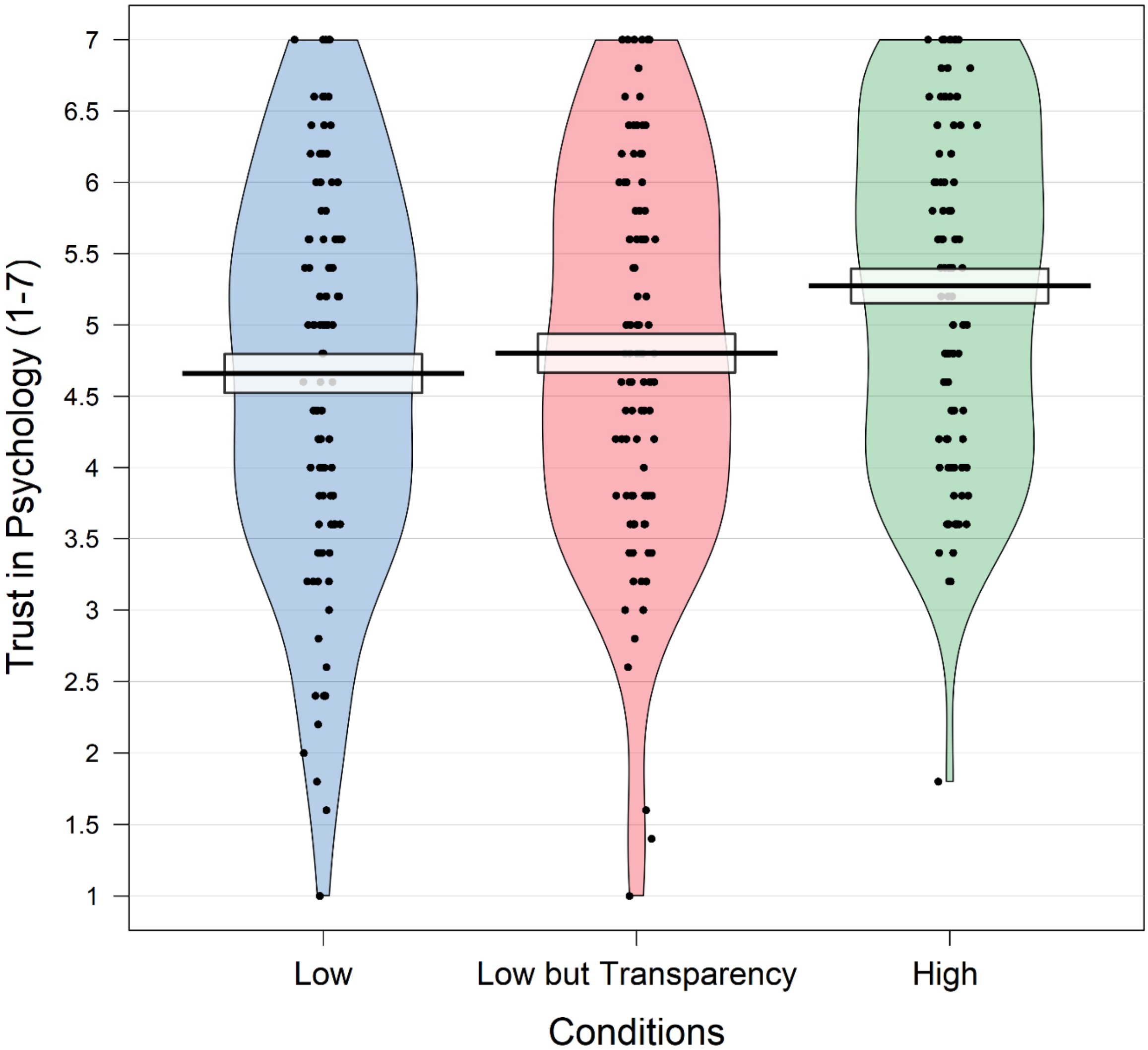

Figure 3.3. Pirate plot showing trust in psychology in the different replicability and transparency conditions.

3.5 STUDY 4

Study 3 found no evidence that increased transparency can repair trust. While this approach focused on the consequences of the replication crisis, another approach to repair public trust might be to explain the causes of low replicability. Thus, in Study 4, we tested the effectiveness of the two most common explanatory strategies: hidden moderators and QRPs. We expected the hidden moderator explanation to lead to a higher trust in psychology than the

QRPs explanation. However, given the little effectiveness of our trust repair strategy in Study 3, we had no clear hypotheses on whether any of the explanations would be able to repair trust compared with providing no explanation, so we preregistered these analyses as exploratory.

3.5.1 METHOD

Participants and design. Three hundred and three participants were recruited to complete a short online study on MTurk for $0.60. 20 participants were excluded for meeting the preregistered exclusion criteria (failing more than one text understanding questions). We randomly assigned participants to three conditions (low replicability condition, hidden moderator condition, QRPs condition). The final sample consisted of 283 participants (55.5% male; age: $M = 36.5$ years, $SD = 12.0$). Sensitivity analyses showed that our final sample had a high chance ($1 - \beta = 0.80$, $\alpha = 0.05$) to detect a difference of $d = 0.41$ between the low replicability and any of the two explanation conditions and a very high chance ($1 - \beta = 0.95$, $\alpha = 0.05$) to detect $d = 0.53$.

Procedure. Participants read the same description of the *Reproducibility Project: Psychology* as in Studies 2 and 3. All participants were told that out of the 100 investigated studies, 39 were successfully replicated. Depending on their condition, participants received no explanation (low replicability condition), an explanation stating that QRPs caused the low replication rate (QRPs condition) or an explanation stating that hidden moderators caused the low replication rate (hidden moderator condition). In the QRPs condition, participants read that researchers "primarily look for new and spectacular results which can lead to bad research practices, for example repeating an experiment until a surprising effect emerges. Often researchers only publish the spectacular results, while less spectacular – but potentially more reliable – results end up in a drawer somewhere." In contrast to that, participants in the hidden moderator condition learned that: "When studying humans, unknown or hidden factors such as individual differences between participants, participants' current state, or minimal differences in the experimental procedure can affect the results. It is very difficult to always have absolute control over these conditions and keep possible influencing factors constant.” (for details see https://osf.io/9ba28/?view_only=7f2edfc9b5f143beb5f86dfdc657d73d). Afterward, participants responded to three text-understanding items and two manipulation checks (1. “Unknown or hidden factors explain the low replication rate”, 2. “Questionable research practices explain the low replication rate”; 1 = *strongly disagree*, 7 = *strongly agree*). Participants did not answer the manipulation checks in the low condition, to avoid highlighting these explanations to control participants. Then, participants filled out the same five items used in Studies 1 - 3 to measure their trust in psychology ($\alpha = .80$). Participants also completed a brief demographic questionnaire and were debriefed.

3.5.2 RESULTS

Manipulation checks indicated that the manipulation was successful (see supplemental materials).

Participants in the QRPs condition showed a significantly lower trust in psychology than participants in the hidden moderator condition, $t(185) = 2.11$, one-sided $p = .018$, $d = 0.31$, 95%-CI [0.02, 0.60]; see Figure 3.4. However, the low replicability condition, which served as a control condition, did not differ significantly from the hidden moderator condition, $t(188) = 0.20$, $p = .839$, $d = 0.03$, 95%-CI[-0.27, 0.32] nor from the QRPs condition, $t(183) = -1.68$, $p = .094$, $d = 0.25$, 95%-CI [0.04, 0.54], which showed an even lower trust than the low replicability condition.

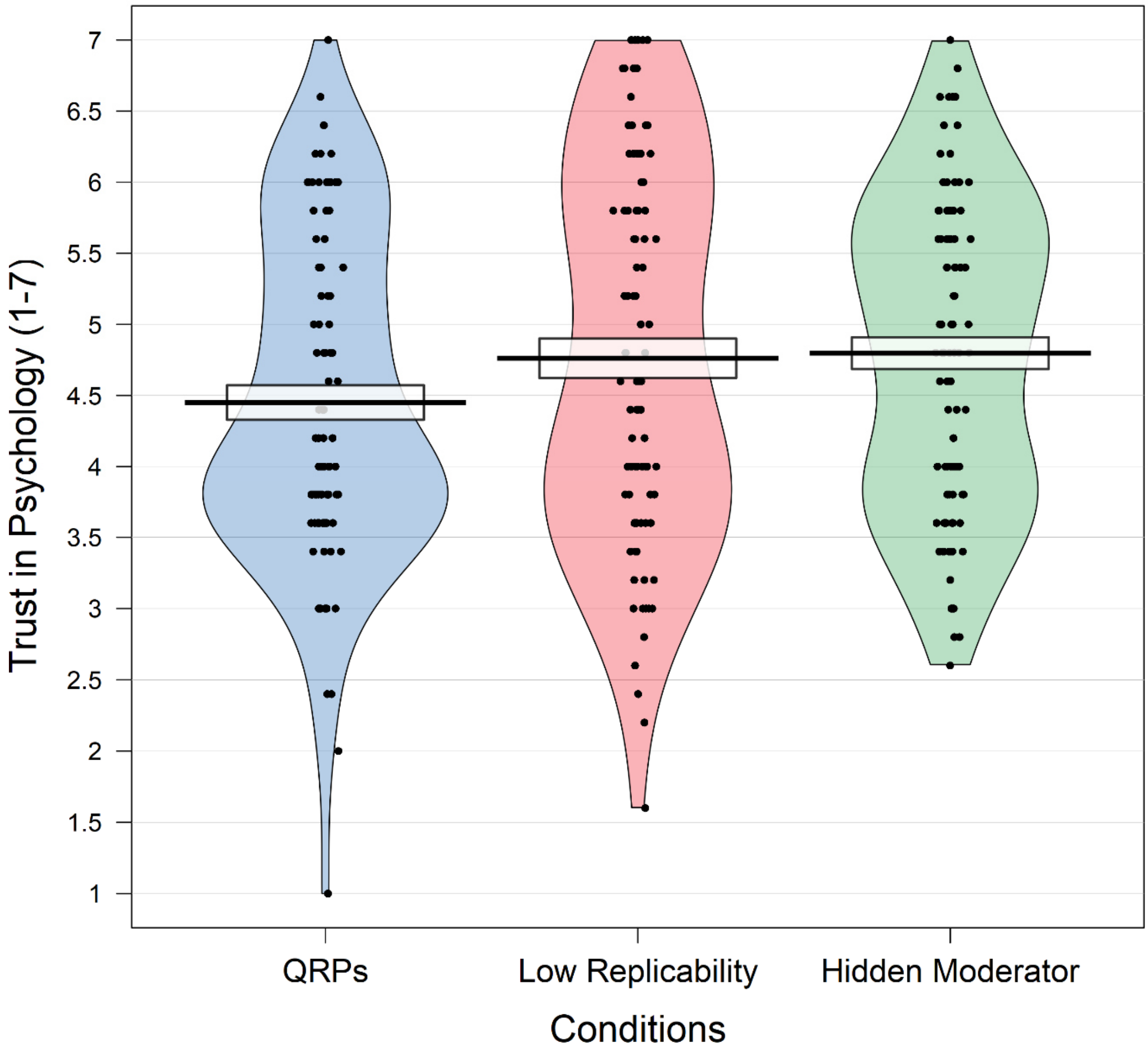

Figure 3.4. Pirate plot showing trust in psychology in the different explanation conditions.

3.6 STUDY 5

Neither increased transparency (Study 3), nor explanations (Study 4) significantly repaired trust. One reason for this might be that we did not provide information about both, the causes and adequate solutions to the crisis, in one study. If non-scientists intuitively do not believe that nontransparent practices (e.g., QRPs) cause low replicability, increasing transparency would not be a sensible response to low replicability. Thus, it might be necessary to inform non-scientists about both: QRPs as a cause of low replicability and transparency as an adequate solution. Whereas a QRP explanation on its own might even damage public trust (see Study 4), such an explanation could be especially effective when combined with information about increased transparency.

Moreover, we did not provide information about whether increased transparency was indeed effective in increasing replicability. Thus, we conducted Study 5 to address these concerns. In this final study, we tested whether public trust can be repaired by providing participants with both, information about the causes of, and adequate solutions for low replicability, and by informing them that these solutions successfully restored high replicability. We expected successfully restored replicability to lead to increased trust in psychology.

3.6.1 METHOD

Participants and design. Three hundred and four participants were recruited to complete a short online study on MTurk for \$0.50 each. We again used an increased target sample size of 300 to compensate for potential exclusions. 26 participants were excluded for meeting the preregistered exclusion criteria (failing more than one text understanding questions). We randomly assigned participants to three conditions (low replicability condition, “now high” replicability condition, “still low” replicability condition). The final sample consisted of 278 participants (64.7% male; age: $M = 33.8$ years, $SD = 11.0$). Sensitivity analyses showed that our final sample had a high chance ($1 - \beta = 0.80$, $\alpha = 0.05$) to detect a difference of $d = 0.36$ between the low replicability and any of the two explanation conditions and a very high chance ($1 - \beta = 0.95$, $\alpha = 0.05$) to detect $d = 0.48$.

Procedure. Participants read the same description of the *Reproducibility Project: Psychology* as in Studies 2, 3, and 4, and additionally learned that the *Reproducibility Project: Psychology* was published in 2015. All participants read that out of the 100 investigated studies, 39 were successfully replicated. In the low replicability condition, participants received no further information. In the “still low” replicability and “now high” replicability conditions, participants received an explanation that QRPs caused the low replication rate, but that this issue was now addressed through the open science movement and the increased transparency of psychological science. In the “still low” condition, which served as an additional control group, participants learned that these measures were not successful. Concretely, they were informed that an (alleged) new systematic replication project in 2018 revealed that out of 100 studies conducted under the new transparency guidelines, still only 41 could be successfully replicated. In contrast, in the “now high” replicability condition, participants learned that those measures were very successful since the alleged new replication project in 2018 revealed that now 83 out of 100 recent studies could be successfully replicated (for details see https://osf.io/9ba28/?view_only=7f2edfc9b5f143beb5f86dfdc657d73d). Afterward, participants responded to three text-understanding items and to the manipulation check (“Psychological research is now more replicable”). Participants did not fill out the manipulation check in the low replicability condition, which received no information about the change in replicability. Then, participants answered the five items from Studies 1 - 4 to measure their trust in psychology (α = .73). Participants also completed a brief demographic questionnaire and were debriefed.

3.6.2 RESULTS

According to our manipulation checks, the manipulation was successful (see supplemental materials).

Participants in the “now high” replicability condition did not show significantly higher trust in psychology than participants in the “still low” replicability condition, $t(178) = 1.29$, one-sided $p = .099$, $d = 0.19$, 95%-CI [-0.10, 0.49], or participants in the low replicability condition ($M = 4.35$, $SD = 1.30$), $t(186) = 1.04$, one-sided $p = .149$, $d = 0.15$, 95% CI-[-0.14, 0.44]; see Figure 3.5.

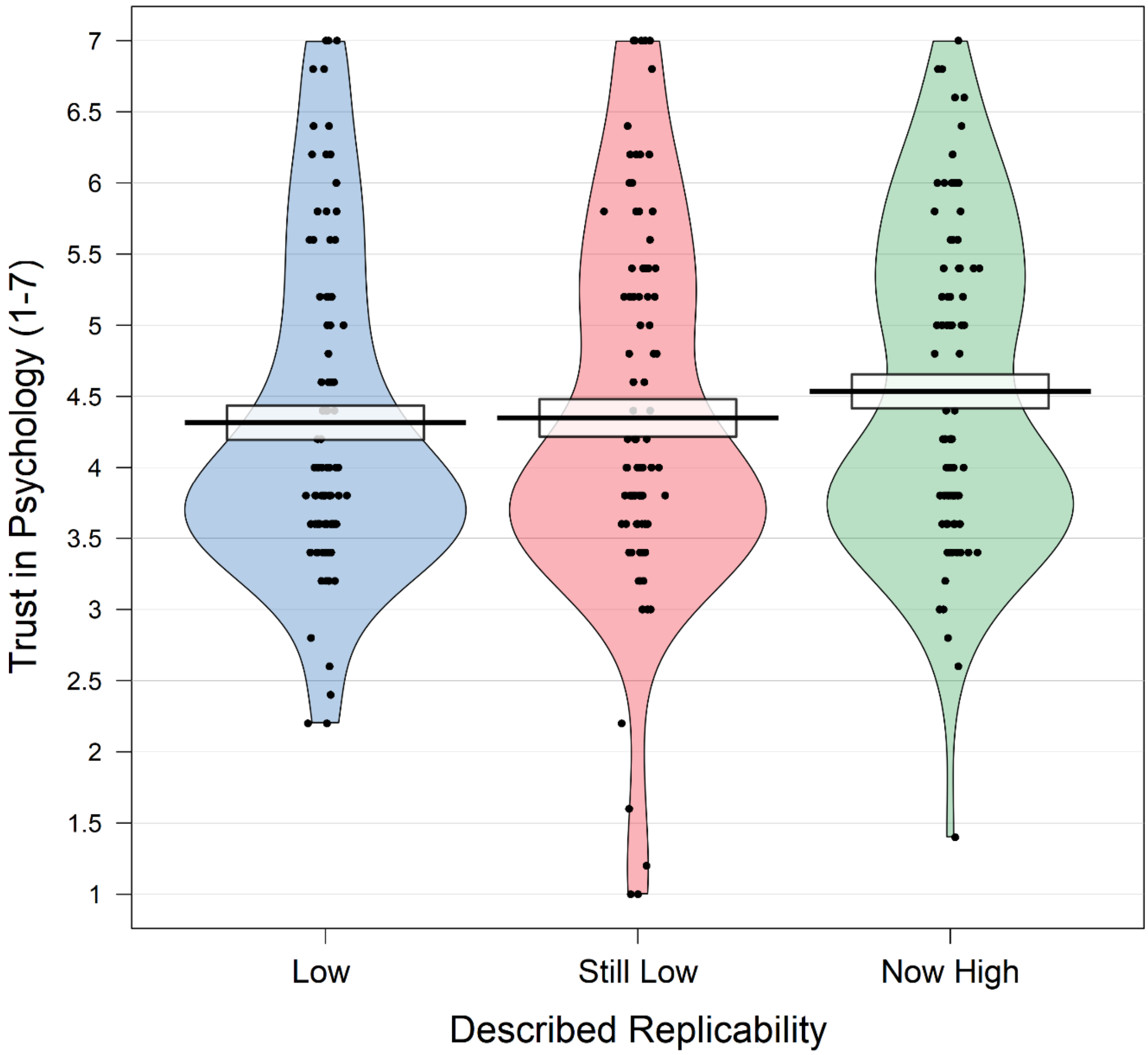

Figure 3.5. Pirate plot showing trust in psychology in the different replicability conditions.

Table 3.1

Descriptive Statistics for the "Trust in Psychology"-Measure Across Studies

Study	Sample Size[a]	Mean	Standard Deviation	Correlation with Replicability[b]
Study 1	270	5.06	1.27	*r*(268) = .329***
Study 2	268	4.94	1.36	
1. Low Replicability	88	4.63	1.34	*r*(86) = .205
2. Medium Replicability	90	4.94	1.44	*r*(88) = .424***
3. High Replicability	90	5.26	1.24	*r*(88) = .239*
Study 3	294	4.91	1.32	
1. Low Replicability	100	4.66	1.36	*r*(98) = .218*
2. Transparency	96	4.80	1.33	*r*(94) = .295**
3. High Replicability	98	5.27	1.20	*r*(96) = .373***
Study 4	281	4.67	1.21	
1. Low Replicability	94	4.76	1.34	-
2. Hidden Moderators	96	4.80	1.09	-
3. QRPs	91	4.45	1.16	-
Study 5	278	4.40	1.20	
1. Low Replicability	98	4.35	1.30	-
2. Still Low Replicability	90	4.32	1.14	-
3. Now High Replicability	90	4.54	1.14	-

Note. * $p < .05$, ** $p < .01$, *** $p < .001$.

[a]Number of participants who completed the "trust in psychology"-measure

[b]In Study 1, this correlation refers to the preregistered correlation of the "trust in psychology"-measure (ranging from 1 to 7) with the estimated replication rate. In Studies 2 and 3 this refers to the not preregistered correlation with the manipulation check "Psychological research is replicable". This manipulation check was not administered in Studies 4 and 5.

3.7 GENERAL DISCUSSION

Our results show that concerns about reduced public trust in light of the replication crisis are justified. Across three studies (Studies 1 - 3), we find correlational and experimental evidence that low replicability reduces trust in psychology. Studies 1 and 2 suggest that not only public trust but also the perceived value of psychological science is damaged by low replicability. Moreover, Studies 3 to 5 found no evidence that commonly used trust repair strategies significantly repair this damaged trust in psychology.

So does low replicability damage public trust beyond repair? Although sensitivity analyses showed that it is unlikely that the tested strategies have large trust-repairing effects, they also suggest that we had no sufficient power to rule out small, but potentially meaningful effects, which could only be detected with larger samples (equivalence tests and Bayes factors in line with this argumentation are presented in the supplemental materials). Our findings thus do not allow us to conclude that the tested strategies are certainly ineffective. However, given the non-significant observed effects of trust repair strategies, our findings also do not provide evidence for the effectiveness of the tested strategies on trust in psychology.

Hence, the critical question is: What should psychological researchers do if they encounter low replicability? Considering that replication studies have limitations and that there is often no consensus about their interpretation (Gilbert, King, Pettigrew, & Wilson, 2016), one could potentially argue that psychologists should avoid informing the public about low replicability. However, this non-transparent approach would be ethically problematic and violates, for example, the APA Ethics Code (see APA, 2017, *pp*. 3- 4). Moreover, failed attempts to cover up problematic research findings might reduce public trust even more (Leiserowitz, Maibach, Roser-Renouf, Smith, & Dawson, 2013). Therefore, covering up low replicability is neither an ethical nor an effective way to handle the problem.

A more promising approach to maintaining the public trust might be to substantially improve the replicability of psychological research findings. Although Study 5 remains inconclusive about whether this is an effective strategy to repair the public trust directly after a replication crisis, Studies 1 to 3 provide evidence that high replicability in the first-place results in increased trust in psychology. Thus, if replicability is constantly high, public trust in psychology might rise. Currently, there is considerable debate about whether constantly high replicability is a worthwhile goal for psychological science. For example, Baumeister (2016) discussed whether a strong focus on replicability could potentially reduce the likelihood of discoveries and the progress and influence of the field. Moreover, scholars debate whether conducting direct replications is after all meaningful (Stroebe & Strack, 2014; cf. Simons, 2014). Although we do not directly speak to these arguments, our work suggests that the debate should also consider the reputational benefits associated with high replicability.

However, it is important to note that we communicated information about low replicability in the form of very short texts, inspired by brief news reports. Potentially, an in-depth explanation of the replication crisis and the open science movement might lead to less negative, or even positive, audience reactions. This is especially likely for highly science-interested audiences which would be willing to engage with such a detailed explanation. Indeed, recent research suggests less negative consequences in such a situation: After a 1-hr lecture on the replication crisis, psychology students' attitudes toward psychology remained relatively stable (Chopik et al., 2018).

Moreover, we conceptualized trust in psychology as trust in the psychological science community. Trust in psychology could however also refer to trust in psychological findings. Since low replicability typically refers to past findings, it seems possible that low replicability of past findings does not necessarily damage trust in future findings (Anvari & Lakens, in press). Likewise, it is possible that the damaged trust in the psychological science community does not generalize to future generations of psychological researchers educated under new, more rigorous methodological guidelines.

Overall, our studies highlight the crucial importance of replicability for public trust in psychology. Thus, the immense effort of the psychological science community to increase replicability is not only scientifically important but also highly relevant to psychology's public reputation. This is especially important in the current political climate, where the credibility of scientific evidence is questioned and science is threatened by defunding (Fanelli, 2018; Yong, 2017).

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CHAPTER 4: CAUTION, PREPRINT! BRIEF EXPLANATIONS ALLOW NON-SCIENTISTS TO DIFFERENTIATE BETWEEN PREPRINTS AND PEER-REVIEWED JOURNAL ARTICLES

This chapter is based on the following preprint:

Wingen, T., Berkessel, J. B., & Dohle, S. (2021). Caution, preprint! Brief explanations allow non-scientists to differentiate between preprints and peer-reviewed journal articles. *Manuscript submitted for publication.* https://doi.org/10.31219/osf.io/7n3mj

Please note that some changes in formatting were undertaken to fit the layout of this dissertation. No changes were made to the content of the manuscript.

ABSTRACT

A growing number of psychological research findings are initially published as preprints, a development fueled by the COVID-19 crisis. Preprints are not peer-reviewed and thus did not undergo the established scientific quality control process. Many researchers hence worry that these preprints reach non-scientists, such as practitioners, journalists, and policymakers, who might be unable to differentiate them from the peer-reviewed literature. Across 5 studies in Germany and the US, we investigated whether this concern is warranted and whether this problem can be solved by providing non-scientists with a brief explanation of preprints and the peer-review process. Studies 1 and 2 showed that without an explanation, non-scientists perceive research findings published as preprints as equally credible as findings published as peer-reviewed articles. However, an explanation of the peer-review process reduces the credibility of preprints (Studies 3 and 4). In Study 5, we developed and tested a shortened version of this explanation which we recommend adding to preprints. This explanation again allowed non-scientists to differentiate between preprints and the peer-reviewed literature. This effect was mediated by the perception of preprints' quality control and their perceived adherence to publication standards. In sum, our research demonstrates that even a short explanation of the concept of preprints and their lack of peer-review allows non-scientists who evaluate scientific findings to adjust their credibility perception accordingly. This would allow harvesting the benefits of preprints, such as faster and more accessible science communication while reducing concerns about public overconfidence in the presented findings.

Keywords: preprints; peer-review; credibility, science communication, publishing

4.1 INTRODUCTION

Scientific findings, in psychology and beyond, are rapidly becoming more open and accessible. As part of this *open science movement*, preprints—that is, scientific manuscripts preceding formal peer review and publication—have gained popularity and their number grows exponentially (see Figure 4.1). This development has been fueled by the COVID-19 crisis, where researchers aim to rapidly disseminate their findings instead of going through the traditional peer-review process (Kwon, 2020; Polka et al., 2021; Rahal & Heycke, 2020).

Peer-review is typically considered as a hallmark of science and the established standard quality control process for scientific publications (Elson et al., 2020; Nosek & Bar-Anan, 2012). Despite its drawbacks, such as reviewer bias and delayed publication (Bornmann & Daniel, 2010; Heesen & Bright, 2019; Huisman & Smits, 2017; Jukola, 2017) peer-review usually ensures that (some) serious errors in manuscripts are detected (Godlee et al., 1998; Schroter et al., 2004). Peer-reviewed manuscripts also have a higher quality of reporting, compared to their non-peer-reviewed version (Carneiro et al., 2019; Goodman et al., 1994). Hence, researchers across disciplines use peer-review as a guiding principle on which work to read and cite (Tenopir et al., 2016), and often perceive preprints as less credible than peer-reviewed publications (Soderberg et al., 2020).

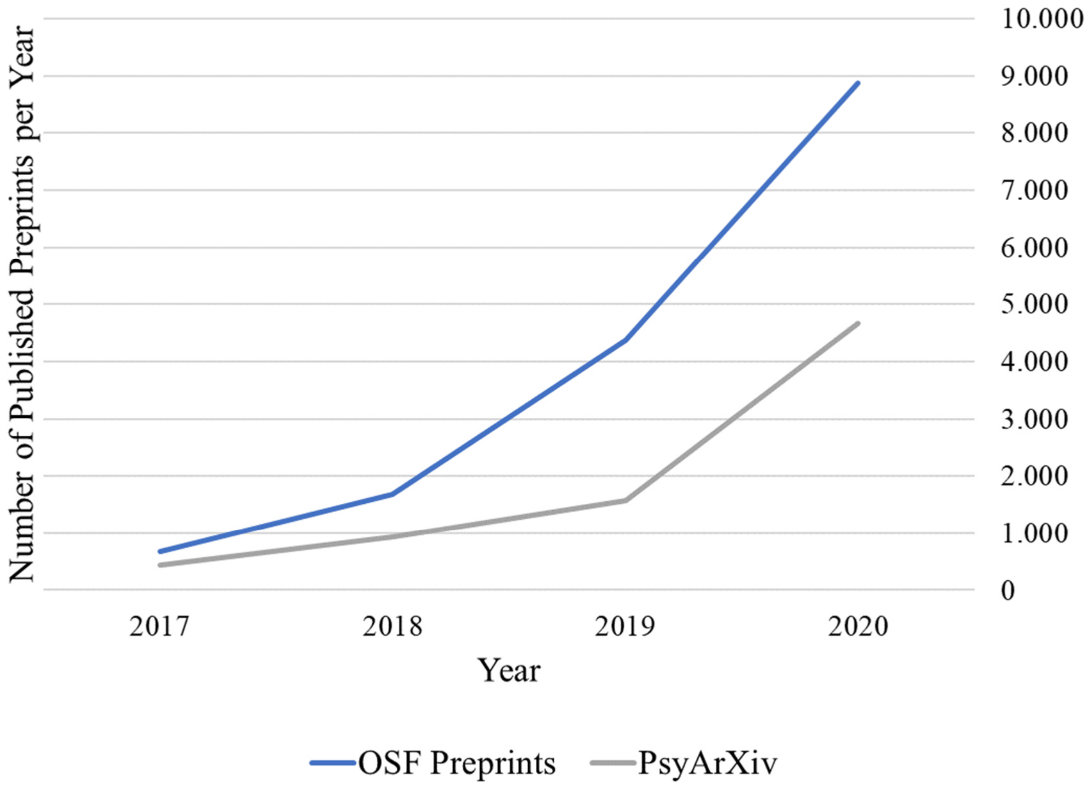

Figure 4.1. Development of the number of preprints published on two major preprint servers for psychology and the social sciences since 2017. Numbers were derived by searching for available preprints on Google Scholar, filtering for each year, and server.

Yet, as preprints are typically publicly available online, they might not only reach researchers, but also the general public. In fact, especially during the COVID-19 crisis, many preprints became part of the public discourse through traditional and social media (Fraser et al., 2020). For example, a now-retracted preprint describing an “uncanny similarity” between SARS-CoV-2 and HIV spurred discussion on social media on whether SARS-CoV-2 is a genetically engineered bioweapon (Forster, 2020), which later on became one of the leading Coronavirus-related conspiracy theories (Imhoff & Lamberty, 2020). Therefore, the responsible preprint server *bioRxiv* added a warning to their website, stating that preprints are preliminary, non-peer-reviewed reports (Forster, 2020). In another example, a preprint on the SARS-CoV-2 viral load in children was disparaged on the title page of the largest German newspaper (Niggemeier, 2020). The newspaper, however, ignored that the work was a preprint and heavily criticized some preliminary analyses. This public debate over a preprint might have damaged trust in science in Germany (Lindner, 2020), which could have had serious consequences for the adherence and adoption of recommended protective behaviors (Dohle et al., 2020).

These examples illustrate what many researchers fear: Members of the general public treating non-peer-reviewed preprints as established evidence, leading to ill-advised decisions and potentially damaging public trust in science (Fox, 2018; Heimstädt, 2020; Rahal & Heycke, 2020; Sheldon, 2018). This concern—which has been described as the most frequent argument against preprints (Vazire, 2020)—goes beyond COVID-19 related research and is highly relevant for all research findings of public interest. Preprints in psychology might be especially likely to catch the public eye, as they deal with questions directly related to human behavior and society. Indeed, media outlets and public science-communication blogs also cover preprints on psychological topics such as voting-behavior (Garisto, 2019), personality (Adam, 2019), or even the trustworthiness of psychological research as a whole (Chivers, 2020).

The central assumption underlying concerns about the public availability of preprints is that non-scientists fail to differentiate between preprints and peer-reviewed literature and thus treat them as equally credible sources. However, this assumption currently lacks empirical evidence. As preprints are often presented with no or very little accompanying information (e.g., simply stating that the results stem from a preprint), we believe that in such a situation, non-scientists will indeed fail to incorporate this information in their credibility judgment. This is because they lack the necessary background knowledge that preprints are not peer-reviewed. We hypothesize that without an additional explanation of preprints and their lack of peer-review, people will perceive research findings from preprints as equally credible compared to research findings from the peer-reviewed literature (Hypothesis 1).

However, recent research suggests that even very brief explanations (e.g., warning labels) allow non-scientists to adjust their credibility ratings (Koch et al., 2021), even for complex scientific topics (Anvari & Lakens, 2019; Hendriks et al., 2020; Wingen et al., 2020). If such a brief explanation of preprints includes that they are not peer-reviewed and thus did not undergo the established standard quality control process for psychological publications, non-scientists might perceive preprints as less credible. Emphasizing increased quality control, for example through consumer reviews or quality management systems (Adena et al., 2019; Boiral, 2012; Resnick et al., 2006; Silva & Topolinski, 2018), as well as highlighting adherence to community norms and standards (Bachmann & Inkpen, 2011; Blanchard et al., 2011; Wenegrat et al., 1996), are linked to increased credibility and trustworthiness. We thus hypothesize that, after receiving an explanation of preprints and their lack of peer-review, non-scientist would perceive preprints as less credible than peer-reviewed articles (Hypothesis 2).

4.1.1 OVERVIEW OF STUDIES

We conducted five experimental studies to test whether non-scientists perceive preprints as less credible than peer-reviewed literature and if this depends on whether they receive an explanation of the peer-review process. We focused on preprints covering research findings from psychology and the social sciences, as they seem particularly likely to be comprehensible and interesting to the general public. In the pilot study, we explored whether preprints in psychology and the social sciences typically provide an explanation of preprints and the peer-review process. We coded 200 recent preprints and examined whether they sufficiently explain their lack of peer-review. Study 1 (German sample) and Study 2 (US sample) tested whether, without an explanation of preprints and the peer-review process, non-scientist would be able to differentiate between peer-reviewed literature and preprints. Study 3 (within-subjects design) and Study 4 (between-subjects design) tested whether after receiving an explanation of preprints and their lack of peer-review, non-scientist would perceive preprints as less credible than peer-reviewed articles. Finally, in Study 5, we developed a shortened version of this explanation and tested whether this very brief explanation allowed non-scientists to differentiate between preprints and peer-reviewed literature. We moreover explored how this explanation works (mediation) and whether the effect of this explanation depends on education and familiarity with the publication process (moderation).

Preregistration. Studies 1 to 5 and the Supplemental Study 1 have been preregistered. All preregistration forms are shared on the Open Science Framework (https://osf.io/egkpb/?view_only=c4410b55f6ca4c6dae4e421272a4a158). The pilot study, which focused on coding existing data, was not preregistered.

Data, materials, and online resources. All materials, anonymized datasets, and analyses code are likewise shared on the OSF. Details regarding the Supplemental Study S1 are provided in the supplemental materials.

Reporting. For each study, we report how we determined our sample size, all data exclusions, all manipulations, and all measures in the study.

The studies are numbered 1–5 for narrative style. Chronologically, the studies were run in the following order: 3, 4, 1, 5, 2 (one study with presumably low data quality is only presented in the supplemental materials). Coding for the pilot study was completed shortly after Study 3. All analyses with a preregistered hypothesis are tested with one-sided *p*-values. In all studies where we predicted the absence of an effect, we rely on equivalence tests with preregistered equivalence bounds. This is a commonly recommended frequentist method to provide evidence for the absence of a meaningful effect (Lakens, 2017; Lakens et al., 2018). All participants who completed our studies were included in the analyses except if they met preregistered exclusion criteria or if they did not provide responses to our central dependent variable (i.e., perceived credibility, not explicitly preregistered). Participants were blocked from participating in more than one study to avoid nonnaïveté (Chandler et al., 2015).

Ethical approval. All studies were conducted consistent with the Declaration of Helsinki, and all are exempt from Institutional Review Board approval by guidelines of the German Psychological Society DGPs (German Society for Psychology, 2018)

4.2 PILOT STUDY

4.2.1 METHOD

For the pilot study, we collected the information presented in the 303 most recent manuscripts on two popular social science preprint servers, commonly used by psychological scientists: *PsyArXiv* (https://psyarxiv.com) and the social and behavioral sciences section at *OSF preprint*s (https://osf.io/preprints). We first collected general bibliographic information (authors, publication date, language, doi, whether the manuscript was a postprint). We excluded 63 manuscripts from our analyses, because they appeared to be accepted versions of articles (postprints) and thus peer-reviewed, thereby not meeting our definition of preprints. We furthermore excluded 33 non-English preprints, and finally, 7 documents that were no preprints (e.g., supplemental materials, book chapter scans).

Finally, two-hundred manuscripts (100 from each server) were included. We coded whether the authors of the preprint: a) mentioned that it is a preprint b) mentioned that it is thus not peer-reviewed, c) explained that peer-review serves as a quality control process, d) explained that peer-review is the standard procedure for scientific publication, e) and/or added another indication that the findings might be preliminary or less credible.

4.2.2 RESULTS

The results showed that only 27.50 % of the preprints explicitly stated that they were preprints. Even fewer preprints (15.50 %) contained information that they had not undergone peer-review yet. Finally, not a single preprint provided information explaining that peer-review serves as a quality control measure. Detailed results for each preprint server are presented in Table 4.1.

Table 4.1

Information About Peer-Review in Recent Preprints on Two Major Preprint Servers.

Number of preprints informing their readers that:	OSF Preprints	PsyArXiv	Overall
They are a preprint (or similar).	30.00%	25.00%	27.50%
Are not peer-reviewed (or similar).	13.00%	18.00%	15.50%[a]
Peer-review is typically part of the scientific publication process.	1.00%	0.00%	0.50%
Peer-review serves as a quality control measure.	0.00%	0.00%	0.00%
Their findings might be preliminary (or similar).	6.00%	0.00%	3.00%

[a]The overall number includes 9 publications mentioning that they are “under review”, but not 11 publications mentioning that they have been “submitted for publication”, as we believe the latter does not clearly indicate to non-scientists that the work has not yet been peer-reviewed.

4.3 STUDY 1

In Study 1, we tested whether participants would evaluate psychological research findings that were published as peer-reviewed articles as equally credible as research findings published as preprints.

4.3.1 METHOD

Participants and design. Participants were German university students recruited online in exchange for course credits, as well as individuals recruited through postings in public German social media groups for voluntary research participation. The study employed a between-subjects experimental design. We randomly assigned participants to one of two between-subject conditions (preprint condition, peer-review condition). Sample size considerations were made in relation to Study 4, which chronologically took place before Study 1; compared to Study 4, we aimed to double our sample size. The recruited sample was slightly larger and consisted of 277 participants (after excluding 35 participants who already took part in Study 4, as preregistered), out of which 204 provided responses to all credibility ratings and were therefore included in the main analysis (74.5 % female; age: M = 25.41, SD = 7.09 y). Power analyses revealed that the sample size of 204 had a 99.87 % power to detect the effect observed in Study 4 (d = .70, α of .05) and a 95% power to demonstrate in an equivalence test that an observed effect is considerably smaller than the effect observed in Study 4 (preregistered equivalence bound of d < .5, compared to d = .70 in Study 4).

Procedure. Participants were presented with five different research findings (for an overview, see Table 4.2). The findings were described as either being published as a peer-reviewed journal article, or as a preprint, depending on condition. For each research finding, participants indicated their perceived credibility ("How credible is this study result?") on a 7-point Likert-scale (ranging from 1 = "not at all credible" to 7 = "very credible"). Participants received no further information (e.g., explanation of the peer-review process). In fact, all five findings (Gervais & Norenzayan, 2012; Hauser et al., 2014; Nishi et al., 2015; Shah et al., 2012; Wilson et al., 2014) were published in the peer-reviewed journals *Nature* or *Science*. Descriptions of these findings were adapted from prior work, where they proved to be comprehensible to non-scientists (Hoogeveen et al., 2020). Findings covered various psychological and economic behavioral science topics, and participants judged the credibility of these five research findings. An average credibility score across all five ratings was computed and served as the dependent variable.

Table 4.2

Overview of Research Findings Used in Studies 1, 2, 4, and 5.

Authors	Short Description
Gervais et al. (2012)	Analytical thinking promotes religious disbelief.
Hauser et al. (2014)	When making collective decisions people share more common resources for future generations.
Nishi et al. (2015)	Financial inequality between group members remains when people are informed about each member's wealth.
Shah et al. (2012)	Poverty drains people's attention.
Wilson et al. (2014)	People dislike doing nothing and prefer an engaged mind.

4.3.2 RESULTS

In line with our preregistration, we computed an average credibility score across all five credibility ratings. As predicted, without a brief explanation, participants considered research findings published as preprints ($M = 4.09$, $SD = 0.80$) to be equally credible compared to findings published as peer-reviewed journal articles ($M = 4.24$, $SD = 0.88$), $t(202) = 1.25$, $p = .211$, $d = 0.18$, 95% CI[-0.10, 0.46]. A preregistered equivalence test, a test that provides support for the absence of a meaningful effect, showed that the observed effect size, which is conventionally considered very small, was equivalent with an interval containing only small to medium effects ($d < .5$), $t(202) = 2.29$, $p = .012$. Descriptive statistics for the perceived credibility across studies and conditions are presented in Table 4.3.

Table 4.3

Perceived Credibility of Research Findings Depending on Source and Explanation Across Studies.

Study	Mean	Standard Deviation	*t*-value[a]	Degrees of freedom	*p*-value[b]	Cohen's *d* [95%-CI]
Study 1 (Without Explanation)						
Peer-Reviewed Article	4.24	0.88				
Preprint	4.09	0.80	1.25	202	.211	0.18 [-0.10;0.46].
Study 2 (Without Explanation)						
Peer-Reviewed Article	4.73	1.11				
Preprint	4.58	1.00	1.50	464	. 136	0.14 [-0.04;0.32]
Study 3 (With Explanation)						
Peer-Reviewed Article	5.63	1.34				
Preprint	4.00	0.93	10.06	51	<.001 (one-sided)	*dz* = 1.39.
Study 4 (With Explanation)						
Peer-Reviewed Article	4.15	0.65				
Preprint	3.67	0.72	3.74	111	<.001(one-sided)	0.70 [0.32;1.09]
Study 5 (With Explanation)						
Peer-Reviewed Article	4.65	1.00				
Limited Information	4.42	1.15	2.02	379	.044	0.21 [0.01;0.41]
Authors' Explanation	4.39	1.07	2.31	359	.010 (one-sided)	0.25 [0.04;0.45]
External Explanation	4.31	1.02	3.25	379	<.001 (one-sided)	0.33 [0.13;0.54]

[a]The *t*-tests results refer to the comparison of the respective condition with the peer-review-condition in each study. These are *t*-tests for dependent samples in Study 3, and for independent samples in the other studies.

[b]One-sided *p*-values are reported for directional hypotheses

4.4 STUDY 2

Study 2 aimed to replicate the findings from Study 1 in a different population using an even larger sample size ($n = 466$; US sample) and a stricter preregistered criterion of what constitutes a negligible difference ($d < .3$). The design was identical to Study 1, with the only difference that Study 2 also included a basic text comprehension check which had to be answered correctly to ensure that participants were aware that the five research findings were published as preprints or peer-reviewed journal articles, respectively.

4.4.1 METHOD

Participants and design. Participants were US-based individuals recruited on the Amazon MTurk platform in exchange for $0.50. The target sample size was set to 578, which allowed us to detect group differences of $d = .30$ ($1- \beta = .95$, α of .05) and moreover provided sufficient power for an equivalence tests ($1- \beta = .95$, equivalence bounds of $d = .3$). To increase data quality, we opted to exclude participants who failed a basic text comprehension check (see below). This decision was based on previous research raising concerns about MTurk workers not reading study materials or even being bots (Chmielewski & Kucker, 2020). To compensate for potential exclusions, we recruited 753 participants, of which 476 passed the preregistered comprehension check. Finally, 466 participants answered all credibility items and were therefore included in the main analysis (42.15% female; age: $M = 37.00$, $SD = 11.97$ y).

Procedure. For the text comprehension check, participants had to answer how the research findings were published and were presented with eight options (e.g., “as textbooks”, “as preprints”). If participants answered the text understanding question incorrectly, they were asked to carefully read the text again. If they failed the text understanding question again, they were excluded from our analyses. We also added a few exploratory questions about whether participants perceived the research findings as strictly quality controlled, whether they believed that the researchers adhered to the standard publication procedure, and participants' education and familiarity with the publication process (to ensure comparability with Study 5). Apart from this, the procedure and design were identical to Study 1.

4.4.2 RESULTS

We computed an average credibility score across all five credibility ratings. As predicted and in line with Study 1, participants rated research findings from preprints (M = 4.58, SD = 1.00) as equally credible as research results from peer-reviewed journal articles (M = 4.73, SD =1.11), $t(464) = 1.50$, $p = .136$, $d = 0.14$, 95% CI[-0.04, 0.32]. An equivalence test showed that this observed effect size, which is conventionally considered very small, was equivalent with an interval containing only small effects ($d < .3$), $t(464) = 1.741$, $p = .041$.

4.5 STUDY 3

In Study 3, we tested whether participants would rate peer-reviewed articles as more credible than preprints if they were informed about the difference between peer-reviewed articles and preprints.

4.5.1 METHOD

Participants and design. Participants were recruited through postings in public German social media groups for voluntary research participation. The targeted sample size was set to $N = 45$, based on an a priori power analysis for 95% power (one-sided α of .05) to detect a moderate effect of $dz = .5$ that would be typical for similar social-psychological research. The recruited sample was slightly larger as is often the case in online studies and consisted of 65 participants. Of these, 52 responded to all credibility items and were therefore included in the main analysis (73.08% female; age: $M = 30.83$, $SD = 9.71$ y).

Procedure. This study employed a within-subjects design. Participants read a short, jargon-free description of the peer-review process, which highlighted that peer-review serves as a quality control process, and that peer-review currently is the standard procedure for scientific publication. They were also informed that some research findings are initially published before the peer-review process as preprints, in order to achieve rapid dissemination of results. The full description reads as follows (translation by authors):

> Usually, scientific articles are subject to an extensive peer-review process. This means that other scientists anonymously review articles submitted to a scientific journal. They then speak out for or against a publication and provide important suggestions for article improvement. This procedure is considered the gold standard of scientific journals. Only articles that receive positive reviews have a chance of being published. This procedure is intended to ensure that the articles are of particularly high quality. However, some articles are now published online as preprints without having been peer-reviewed. This allows scientists to make their results available to the public very rapidly, whereas the time-consuming peer-review process can take several months. Normally, peer-review is then carried out after the article has been submitted to a scientific journal.

Afterward, participants reported the perceived credibility of research findings published as peer-reviewed articles and as preprints on a 7-point Likert-scale (ranging from 1 = “not at all credible” to 7 = “very credible”). Finally, participants indicated whether they had heard about preprints and peer-reviewed articles prior to the study, completed demographics, and were debriefed.

4.5.2 RESULTS

As predicted, participants rated research findings from preprints (M = 4.00, SD = 0.93) as less credible than research results from peer-reviewed journal articles (M = 5.63, SD =1.34), $t(51) = 10.06$, one-sided $p < .001$, $dz = 1.39$.

4.6 STUDY 4

Study 4 tested whether the finding of Study 3 generalizes to a more realistic situation where participants do not directly compare preprints and peer-reviewed articles with each other. Instead, participants judged specific research findings, identical to Studies 1 and 2.

4.6.1 METHOD

Participants and design. Participants were German university students recruited online in exchange for course credits. The study employed a between-subjects experimental design. We randomly assigned participants to one of two between-subject conditions (preprint condition, peer-review condition). The target sample size was set to 102, based on an a priori power analysis for 80% power (one-sided α of .05) to detect a moderate effect of d = .5. The recruited sample consisted of n = 140 participants, of which 113 responded to all credibility items and were therefore included in the main analysis (76.11% female; age: M = 23.75, SD = 5.10 y).

Procedure. Participants read the same short descriptions of the peer-review process and preprints as in Study 3 and answered two exploratory text comprehension questions. Participants judged the credibility of five research findings (see Studies 1 and 2, and Table 2). The findings were described as either being published as peer-reviewed journal articles or as preprints. Ratings were given on a 7-point Likert-scale (ranging from 1 = “not at all credible” to 7 = “very credible”). Participants also indicated whether they had heard about preprints and peer-reviewed articles prior to the study, received an exploratory open-entry question on how they made their credibility judgments, completed demographics, and were debriefed.

4.6.2 RESULTS

In line with our preregistration, we computed an average credibility score across all five credibility ratings. As predicted, participants rated research findings from preprints (M = 3.67, SD = 0.72) as less credible than research findings from peer-reviewed journal articles

($M = 4.15$, $SD = 0.65$), $t(111) = 3.74$, one-sided $p < .001$, $d = 0.70$, 95% CI[0.32,1.09]. This pattern is depicted in Figure 4.2.

Figure 4.2. Pirate plot showing perceived credibility as a function of publication mode in Study 4, which provided participants with an explanation of preprints and the peer-review process. The black dots represent the jittered raw data which is shown with smoothed densities indicating the distributions in each condition. The central tendency is the mean, and the intervals represent two standard errors around the mean.

4.7 STUDY 5

In Study 5, we developed a shortened version of the explanation utilized in Studies 3 and 4 which could be easily added to preprints. We tested whether this explanation allows non-scientists to differentiate between preprints and the peer-reviewed literature. We further tested whether it matters if this brief explanation is provided by the authors, or by an external source, but expected the explanation to be effective in both cases. As most preprints are published in English, we tested this in an English-speaking population ($n = 727$; US sample).

We also aimed to explore the underlying mechanism of our explanation and tested preregistered mediators (perceived quality control and perceived adherence to publication standards), and moderators (education and familiarity with the publication process).

4.7.1 METHOD

Participants and design. Participants were US-based individuals recruited on the Amazon MTurk platform in exchange for $0.50. We randomly assigned participants to one of four between-subject conditions (peer-review condition, preprint: limited information condition, preprint: authors' explanation condition, preprint: external explanation condition). The target sample size was set to 1,000, which allowed us to detect group differences of d = .29 (1- β = .95, one-sided α of .05) and moreover provided sufficient power for an equivalence tests (1- β = .91, equivalence bounds of d = .3). We recruited 1,051 participants, of which 739 passed the preregistered text comprehension check. For the text comprehension check, participants had to answer how the research findings were published (see Study 2). If an additional explanation of peer-review and preprints was given, they also indicated for three additional text comprehension questions whether they were true or false ("Scientific articles are usually peer-reviewed.", "As part of the peer-review process, independent researchers evaluate the quality of the work.", and "Preprints have been peer-reviewed"). If participants answered any of the questions incorrectly, they were asked to read the text carefully again. If they again failed any of the text comprehension questions, they were excluded from our analyses. Finally, 727 participants responded to all credibility items and were thus included in the main analyses (43.39 % female; age: M = 39.24, SD = 12.68 y). One participant provided no responses to the remaining items, reducing the sample size for secondary analyses to 726.

Procedure. Participants learned that they would judge the credibility of five research findings and were randomly assigned to one of four conditions. In the *peer-review condition*, participants were informed that the findings went through a peer-review process and were published in a scientific journal. The *preprint: limited information condition* stated that the findings were preprints but in contrast to Studies 1 and 2, it was also added that preprints are not peer-reviewed (without further information, however, what is meant by peer-review). In the other two conditions, the research findings were presented as non-peer-reviewed preprints, but participants received an additional explanation of preprints and the peer-review process. This additional explanation was allegedly either provided by the authors of the preprint (*preprint: authors' explanation condition*) or without any reference to the source in the introduction of the study (*preprint: external explanation condition*).

The explanation was drafted building on the information provided in Studies 3 and 4 but incorporated further feedback from colleagues from various disciplines (anthropology, biology, psychology, and sociology), as well as from non-scientist to ensure an interdisciplinary perspective and comprehensibility. The explanation highlighted two important aspects, namely that peer-review serves as a quality control process, and that peer-review currently is the standard procedure for scientific publication. Compared to Studies 3 and 4, we aimed to keep this explanation as comprehensive as possible. This explanation read:

> Scientific articles usually go through a peer-review process. This means that independent researchers evaluate the quality of the work, provide suggestions, and speak for or against the publication. Please note that the present article has not (yet) undergone this standard procedure for scientific publications.

After judging the credibility of the research findings, participants were also asked about the perceived quality control of the research findings, the perceived adherence to scientific publication standards, their education, and their familiarity with the publication process. Credibility ratings were given on a 7-point Likert-scale (ranging from 1 = "not at all credible" to 7 = "very credible"). Familiarity with the publication process ("I am familiar with the scientific publication process"), perceived quality control of the research findings ("The quality of the research findings has been strictly controlled”), and perceived adherence to scientific publication standards (“When publishing their findings, the researchers followed the standard procedure of the research community”) were measured on a 7-point Likert-scale (1 = “strongly disagree” to 7 = “strongly agree”)

4.7.2 RESULTS

Main analyses. We again computed an average credibility score across all five credibility ratings. As predicted, across both preprint explanation conditions, participants reported lower credibility of research findings compared to the peer-review-condition (M = 4.65, SD = 1.00). This was the case when participants received the explanation by the authors (M = 4.39, SD = 1.07), $t(359) = 2.32$, one-sided $p = .010$, $d = 0.25$, 95% CI[0.04,0.45], as well as by an external source (M = 4.31, SD = 1.02), $t(379) = 3.25$, one-sided $p < .001$, $d =$ 0.33, 95% CI[0.13,0.54], see Figure 4.3. Interestingly and unexpectedly, this was also the case when participants only received very limited information (M = 4.42, SD = 1.15), $t(379) =$ 2.02, $p = .044$, $d = 0.21$, 95% CI[0.01,0.41]. The three preprint conditions did not significantly differ from each other (all $ps > .317$, all $ds < .10$), and the observed differences between these conditions were all equivalent with an interval containing only small effects ($d < .3$), all $ps < .031$ (see OSF analyses for details).

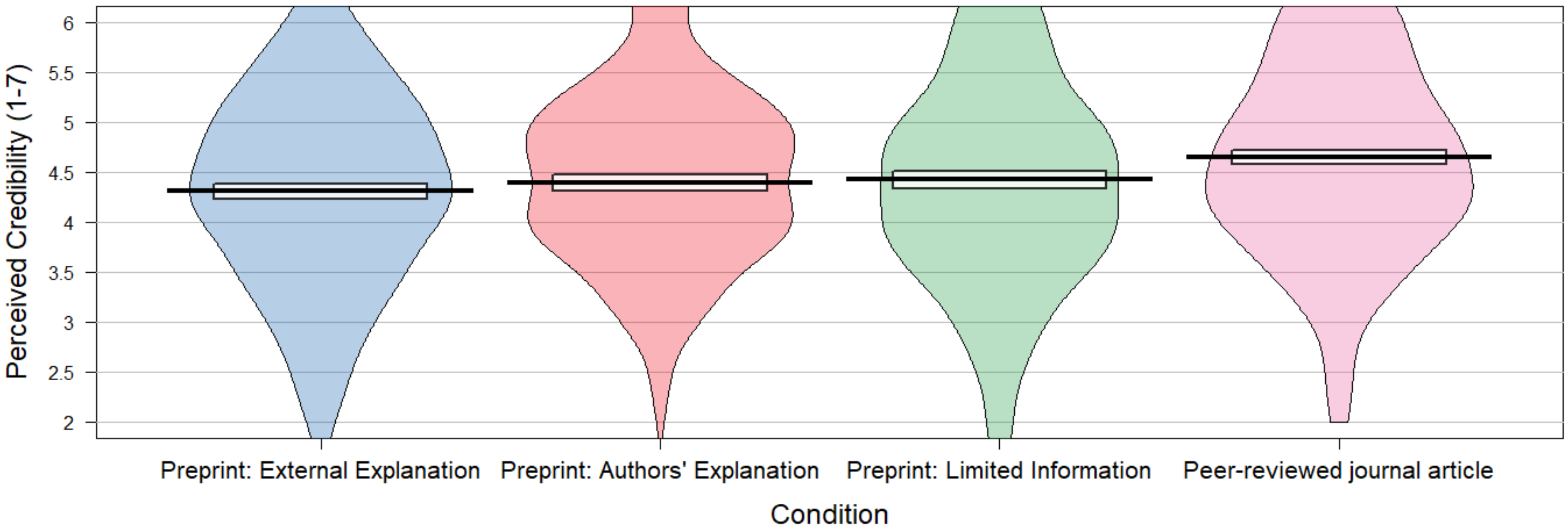

Figure 4.3. Pirate plot showing perceived credibility as a function of publication mode and explanation in Study 5.

Quality Control and Adherence to Scientific Publication Standards. However, the three preprint explanations differed regarding the perceived quality control of the research findings and the perceived adherence to scientific publication standards. Participants who received an explanation reported lower perceived quality control of preprints compared to the limited information condition ($M = 4.27$, $SD = 1.79$). This was the case no matter whether participants received this explanation by the authors ($M = 3.72$, $SD = 1.63$), $t(344) = 2.97$, one-sided $p = .002$, $d = 0.32$, 95% CI [0.11,0.53], or by an external source ($M = 3.\ 81$, $SD = 1.71$), $t(363) = 2.51$,one-sided $p = .006$, $d = 0.26$, 95% CI [0.06, 0.47]. Likewise, after receiving an explanation, participants reported lower perceived adherence to scientific publication standards compared to the limited information condition. This was again the case no matter whether participants received this explanation by the authors ($M = 3.83$, $SD = 1.72$), $t(344) = 3.15$, one-sided $p < .001$, $d = 0.34$, 95% CI [0.13,0.55], or by an external source ($M = 3.95$, $SD = 1.79$), $t(363) = 2.50$, one-sided $p = .007$, $d = 0.26$, 95%-CI [0.05, 0.47].

Moderation Analyses. In line with our preregistration, we also tested whether education or familiarity with the scientific publication process moderated the effect of our explanation on the perceived credibility of research findings (compared to the peer-review condition). For these analyses, we merged the preprint: authors' explanation-condition and the preprint: external explanation-condition, as they did not differ on any of the relevant variables. We conducted multiple linear regression analyses to test whether any of our potential moderator variables moderated the relationship between explanation (detailed explanation vs. peer-review) and credibility. Indeed, whereas centered education did not significantly interact with our explanation ($b = -0.22$, SE = 0.14, $t(539) = 1.58$, $p = .115$), centered familiarity with the publication process was a significant moderator, indicating that our explanation was more effective for people who indicated a higher familiarity with the scientific publication process (*b* = -0.12, SE = 0.05, *t*(539) = 2.49, *p* = .013; see Table 4).

Table 4.4

Multiple Linear Regression Predicting Perceived Credibility From Condition, Centered Familiarity With the Publication Process, and Their Interaction Term.

Predictor	*B*	*SE B*	*t(539)*	*p*
Condition (0 = *peer-review*,1 = *explanation*)	-0.22	0.09	2.46	.014
Familiarity (Centered)	0.23	0.04	5. 85	<.001
Condition * Familiarity (Centered)	-0.12	0.05	2.49	.013

Mediation Analyses. Finally, we explored preregistered mediators of the effect of our explanation on credibility (compared to the peer-review condition). For these analyses, we again merged the authors' explanation-condition and the external explanation-condition, as they did not differ on any of the relevant variables. We investigated whether perceived quality control or perceived adherence to publication standards mediated the negative effect of explaining preprints on perceived credibility. To test this, we ran a parallel mediation model (see Figure 4.4) with 10,000 bootstrap resamples, using the R package lavaan (Rosseel, 2012). This model revealed that both perceived quality control (*b* = -0.23, 95% CI [-0.14, -0.33]), as well as perceived adherence to publication standards (*b* = -0.25, 95% CI [-0.15, -0.35]) simultaneously mediated the effect.

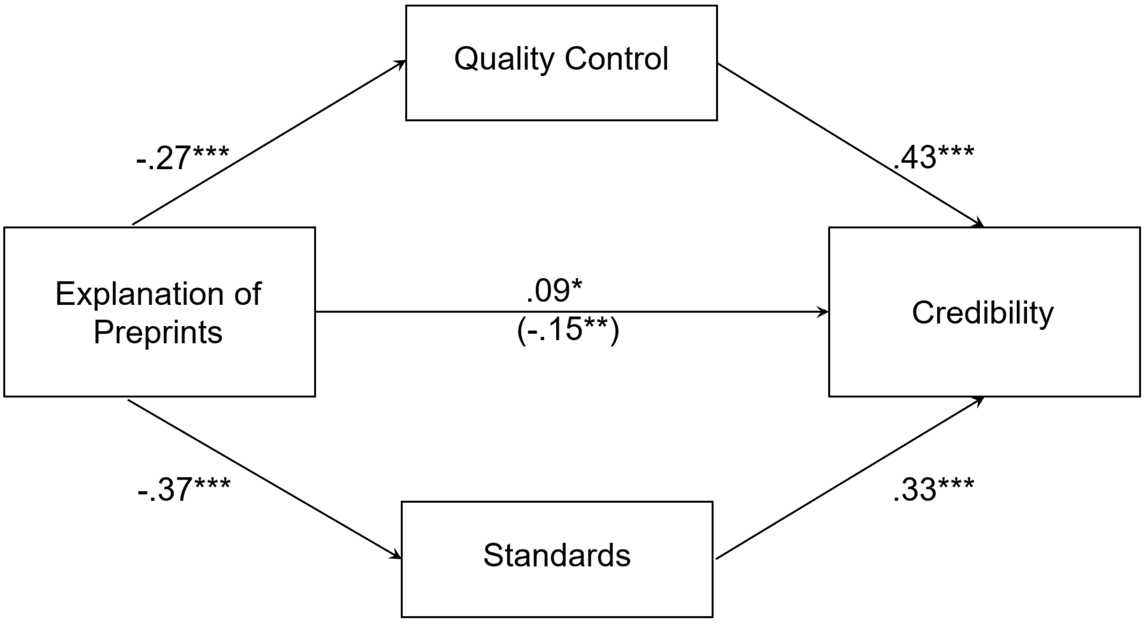

Figure 4.4. Parallel mediation analyses involving perceived quality control and adherence to publication standards as dual, simultaneous mediators for the link between explanations (0 = peer-review condition, 1 = merged explanation conditions) and perceived credibility. Values represent standardized path coefficients. The total effect is presented in parentheses.
* indicates significance at the $p < .05$ level, ** at the $p < .01$ level, and *** at the $p < .001$ level.

4.8 GENERAL DISCUSSION

A central argument against preprints is that non-scientists might fail to differentiate them from the peer-reviewed literature (Fox, 2018; Vazire, 2020). Indeed, non-scientists from Germany (Study 1) and the US (Study 2) perceived research findings published as preprints as equally credible as research findings published in peer-reviewed journals. However, a brief explanation of the peer-review-process, combined with the information that preprints are not peer-reviewed, led non-scientists to perceive identical research findings published as preprints as less credible than the peer-reviewed literature. This effect was observed for research findings in general (Study 3), and specific psychological research findings (Studies 4 and 5). Study 5 further suggested that even a very brief explanation, which could be added to all preprints, allowed non-scientists to differentiate them from the peer-reviewed literature. Importantly, this effect emerged independently of whether this explanation was allegedly provided by the preprints' authors, or by an external source, even though the effect was descriptively smaller in the former situation. The explanation seemed to be especially effective for individuals who are rather familiar with the scientific publication system, and it seems to work by influencing whether non-scientists see preprints as quality-controlled and as adhering to publication standards. In other words, when non-scientists are well-informed about the source of information, they are able to adjust their credibility ratings accordingly.

In practice, however, most psychological preprints do not contain such an explanation. The pilot study, in which we coded recent preprints from two popular psychological preprint servers, revealed that less than 30% of preprints contained information that they are a preprint. Even fewer mentioned that they are not peer-reviewed and virtually none provided an explanation similar to the one used in our studies. Taking this current status quo into account, our findings suggest that non-scientists might currently be unable to differentiate between preprints and the peer-reviewed literature.

Some scholars (Elmore, 2018) have pointed to the fact that the term preprint is a misnomer because there may never be a future print version in a scientific journal (e.g., if the preprint does not pass the peer-review process). Non-scientists might, however, believe that preprints are in fact earlier versions of already published and peer-reviewed articles. This discrepancy could explain why Study 5 found that simply stating that preprints have not yet passed peer-review—something that many individuals are probably not aware of—already reduced perceived credibility. The same study, however, also demonstrated that a more detailed explanation led to a stronger differentiation between preprints and peer-reviewed literature regarding their perceived quality control and their perceived adherence to publication standards, which were relevant mediators. We thus recommend that future authors of preprints, but also preprint servers or science journalists covering preprints, should briefly explain the peer-review-process and highlight that preprint are non-peer-reviewed. Our research suggests that such an explanation might be especially effective if it includes elements that indicate that peer-review serves as a quality control process and that it is the standard procedure for scientific publication.

One important discussion point, however, is whether it is desirable that non-scientists differentiate between preprints and peer-reviewed literature in terms of credibility. Even though the peer-review system leads to improvements of a manuscript (Carneiro et al., 2019; Godlee et al., 1998; Goodman et al., 1994; Schroter et al., 2004), it also has serious drawbacks (Heesen & Bright, 2019; Huisman & Smits, 2017; Jukola, 2017), and one might argue that preprints are not necessarily less credible than peer-reviewed articles. Nevertheless, we deem it important to explain peer-review and preprints to non-scientists. In contrast to more patronizing statements, such as the statement by *BioRxiv* (preprints "should not be regarded as conclusive, guide clinical practice/health-related behavior, or be reported in news media as established information"), our approach leaves it up to the reader to decide whether a preprint is less credible. Our findings suggest that non-scientists use this information, and we, therefore, argue that this information should not be withheld.

It is nevertheless important to discuss the generality of our findings (Simons et al., 2017). First, as we replicated our findings in rather different samples (US- MTurk users and German students), we expect our findings to replicate also in more representative samples for these and other (Western) countries. Notably, however, we found that our explanation was more effective for participants who reported a high familiarity with the publication process. This might explain why we observed substantially larger effects in Germany: As the German samples mostly consisted of undergraduate students, they might be more familiar with the publication process, compared to the US-samples of Amazon MTurk users. Thus, familiarity with the publication process might constrain the generality of our findings. From an applied perspective, it seems likely that non-scientists seeking out preprints might be rather familiar with the publication process (e.g., journalists), which means that our explanation would be rather effective in such a situation. However, it is also possible that this is a methodological artifact: participants who read our materials more closely might consequently report a higher familiarity with the publication process, as well as being more strongly affected by the manipulation.

Moreover, it seems likely that the effectiveness of our explanation depends on participants' general trust in science, because our explanation highlights that preprints do not follow the established scientific publication procedure. If, however, participants' trust in the established scientific knowledge is generally low, a deviation from established standards might not reduce, but even increase, trust. This could for example be the case for politically highly conservative participants, who are contemporarily characterized by relatively low trust in science (Gauchat, 2012).

Finally, it would also be vital to test whether our findings generalize to other forms of non-peer-reviewed science communication, such as blogs, podcasts, or popular science magazines. For example, during the COVID-19 crisis, some scientists shared their findings through non-peer-reviewed podcasts and even press conferences (Kupferschmidt, 2020). In such a situation, it might also be desirable to inform the public that the presented research findings have not been peer-reviewed to avoid public overconfidence in the presented research. It however remains possible that the public already perceives such publication formats as rather uncommon and thus less credible, leaving no room for such an explanation to have an additional effect. This remains an interesting question for future research.

In sum, our work suggests that concerns about non-scientists not differentiating between preprints and peer-reviewed psychological literature are legitimate. However, we also suggest and test a solution: preprint authors, preprint servers, and other relevant institutions can likely mitigate this problem by briefly explaining the concept of preprints and their lack of peer-review. This would allow harvesting the benefits of preprints, such as faster and more accessible science communication while reducing concerns about public overconfidence in the presented findings.

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SUPPLEMENTARY MATERIALS

Prior to Study 5, we run an additional study. Crucially, and in contrast to Study 5, this study did not employ a text comprehension check, which we believe led to noisy responses undermining any effect of our manipulations. This interpretation was supported as the pattern of results changed substantially when utilizing text comprehension checks (see Study 5).

Method

Participants and designs. Participants were US-based individuals recruited on the Amazon MTurk platform in exchange for $0.50. We randomly assigned participants to one of three between-subject conditions (peer-review-condition, limited information-condition, authors' explanation-condition). The design was thus identical to Study 5, except that we did not employ an external explanation-condition. The target sample size was set to 867, which allowed us to detect group differences of $d = .27$ ($1- \beta = .95$, one-sided α of .05) and moreover provided sufficient power for an equivalence tests ($1- \beta = .95$, equivalence bounds of $d = .3$). To compensate for potential exclusions, we recruited 923 participants, of which 845 passed the preregistered attention check (35.74% female; age: $M = 37.42$ years, $SD = 10.68$) and responded to all credibility items and were thus included in the analyses. One participant provided no response to the perceived adherence to scientific publication standards, reducing the sample size regarding this measure to 842.

Procedure. As in Study 5, participants learned that they would judge the credibility of five research findings. Depending on condition, participants either learned that these findings were published in peer-reviewed journal article, as preprints, or as preprint with an additional explanation (see Study 5 for details). The further procedure was identical to Study 5, except that we did not employ a text comprehension question but instead relied on a more general attention check ("If you read this, please choose strongly disagree").

Results

We again computed an average credibility score across all five credibility ratings. However, in contrast to Study 3, 4, and especially 5, none of the conditions differed significantly from the others regarding perceived credibility (all *p*s > .298, all ds < .09). Equivalence test showed that all observed differences were equivalent with an interval containing only small effects (*d* < .3), all *p*s < .002.

Moreover, we did not observe any difference regarding perceived quality control between the authors' explanation (*M* = 4.60, *SD* = 1.55) and the peer-review condition (*M* = 4.60, *SD* = 1.48), *t*(561) = 0.06, one-sided *p* = .476, *d* = 0.01, 95%-CI [-0.16;0.17]. Despite explaining that preprints did not undergo a quality control process, participants did not perceive them as less quality controlled. This further adds to the notion that the observed data had a low quality, as people likely did not read the text. We did however observe a very small, yet significant difference regarding the perceived adherence to scientific publication standards. Participants in the authors' explanation condition reported a slightly decreased perceived adherence (*M* = 4.65, *SD* = 1.59), compared to participants in the peer-review condition (*M* = 4.93, *SD* = 1.24), *t*(560) = 2.30, one-sided *p* = .011, *d* = 0.19 (95% CI = 0.03;0.36).

CHAPTER 5: GENERAL DISCUSSION

Trust in science is necessary for the functioning of modern society and is related to a variety of beneficial outcomes (Hendriks et al., 2016). Chapter 2 provided additional evidence for the importance of trust in science by showing that trust in science is an important predictor of behavior during the COVID-19 pandemic. Here, trust in science predicted the acceptance and adoption of recommended behaviors, even after statistically controlling for many other key factors such as risk perception, socioeconomic status, gender, or age. It seems likely that trust in science also plays an important role in other (future) crises. For example, the reaction of people to the climate crisis, which is probably the most pressing global challenge of our time, is also related to trust in science (Anderegg et al., 2010; Rutjens et al., 2018).

Importantly for the scientific community, trust in science does not exist in a vacuum, independent of the internal processes of the scientific community. Instead, the scientific community can (sometimes) influence trust in science through their own methods, processes, and behaviors. For example, Chapter 3 (Studies 1 to 3) shows that public trust in psychology is reduced when scientists produce many non-replicable research findings. This damaged trust seems difficult to repair: Neither information about open science and transparency (Chapter 3, Study 3), nor explaining the reasons of low replicability (Chapter 3, Study 4), and not even an increased replicability (Chapter 3, Study 5), restored public trust damaged by low replicability.

However, not all scientific outcomes are equally trustworthy, and the scientific community must take great care not to create public confusion about this issue. This is especially the case because many research findings are now becoming widely available, even when they are not yet peer-reviewed, for example as preprints. Chapter 4 (prestudy) shows that currently, most preprints from the social sciences do not contain explanations of their lack of peer-review and that in such a situation, the credibility-ratings of non-scientists do not differ between preprints and the peer-reviewed literature (Chapter 4, Studies 1 and 2). This may indicate public misunderstanding over preprints and lead scholars to argue against this publication format. However, a solution to this problem could be to give a short explanation of preprints and their lack of peer-review, which enabled non-scientists to differentiate the two types (Chapter 4, Studies 3 to 5).

Overall, this dissertation highlights the importance of trust in science as a central predictor of behavior (e.g., during the COVID-19 pandemic; Chapter 2) and suggests that trust in science can be influenced by various scientific practices (e.g., replication studies, publishing preprints; Chapter 3 and 4). However, it should be noted that not all investigated practices show significant effects on public trust (e.g., information about open science, increasing replicability; Chapter 3). Moreover, this dissertation suggests that some new research practices, such as publishing preprints, may lead to public confusion about the credibility of certain research findings, a potential issue that the scientific community needs to consider carefully (Chapter 4).

This dissertation, therefore, suggests that methodological practices within the scientific community matter beyond that community. Non-scientists care about complex methodological issues (e.g., replicability) and their perspectives hence need to be considered by the scientific community, given the importance of public trust in science. When researchers consider this perspective, such as when writing public press releases, it seems reasonable to ask whether the used communication strategies have the desired effect. For example, attributing replication failures to the high context sensitivity of research might be an ineffective strategy to maintain public trust (Chapter 3, Study 4), despite being the central argument in a press release on this topic (German Psychological Society (DGPs), 2015).

Finally, this dissertation illustrates that debates and new research practices within the scientific community can themselves be a fruitful subject of research, either from a meta-scientific or a public perception perspective. Whereas work in this dissertation investigated the public reaction to low replicability and preprints, other work in this area has coded the content of scientific blogs (Nicolas et al., 2019), investigated whether non-scientists can predict the replicability of research results (Hoogeveen et al., 2020), and investigated public attitudes toward questionable research practices (Bottesini et al., 2019). However, the public perception of many aspects of the open science movement has not yet been investigated (e.g., reactions to preregistrations) suggesting a large potential for many future insights.

5.1 Implications for Theoretical Perspectives on Trust in Science

This dissertation examines three main theoretical perspectives on trust in science: the stereotype content perspective, the information deficit perspective, and the public engagement perspective. But how are the findings of this dissertation related to each perspective?

A stereotype content perspective would suggest that trust in science can be understood by focusing on stereotypes about scientists (Fiske et al., 2002; Fiske & Dupree, 2014). In contrast, this dissertation shows that trust in science is not only influenced by stereotypic images of scientists but also (and often strongly) by the methods and practices used by scientists. For example, while a certain stereotype of psychological scientists and their trustworthiness may exist, psychological scientists were perceived differently, depending on the replicability of their research results, which considerably influenced public trust (Chapter 3). This is in line with a variety of other research findings, which suggest that scientific practices such as openness, self-correction, and methodological rigor influence public trust in science (Altenmüller, Nuding, et al., 2021; Ebersole et al., 2016; Schneider et al., 2019). These are all aspects not directly captured by a stereotype content perspective. This dissertation, therefore, suggests that public trust in science cannot fully be understood simply through a stereotype content perspective and that evaluations of specific research practices also have a strong impact on public trust. Of course, stereotypes about scientists may still play a role in the public perception of science, especially when other, potentially more diagnostic information, is missing (De Dreu et al., 1995).

Whereas the stereotype content perspective focuses on broad perceptions about scientists, the information deficit model focuses strongly on scientific findings and knowledge and seemed at a first glance better suited to account for the findings in this dissertation. The model assumes that the public is largely scientific 'illiterate', which is seen as the root of distrust in science (J. D. Miller, 1983; Sturgis & Allum, 2004; Wynne, 1982). The model predicts that when the public becomes more informed about science, trust in science should increase (Abunyewah et al., 2020; Pearson, 2005; Simis et al., 2016).

These predictions are in stark contrast to the findings presented in this dissertation. First, providing additional information often had no significant effect on trust in science, for example in the case of providing explanations for low replicability, or when informing people about the open science movement (Chapter 3, Studies 3 and 4). Second, some information about science even had negative effects on trust in science. For example, factually correct information about the replication crisis damaged public trust in science (Chapter 3, Studies 1 to 3), and providing more information about preprints reduced the credibility of scientific findings published in this format (Chapter 4). Therefore, non-scientists are not simply receivers reacting positively to any form of scientific information, but instead show much more nuanced reactions to information about science, apparently taking into account the (assumed) quality of research findings. Finally, even the correlational results presented in Chapter 2 provide (limited) evidence against the information deficit model: Socio-economic status, which is strongly related to education (Dubois et al., 2015; T. Wingen, Englich, et al., 2020), was not associated with trust in science, or with the adherence and adoption of scientifically recommended protective measures (Chapter 2, Study 2). This suggests that highly educated individuals, who are better informed about science, were not significantly more likely to trust scientists or their recommendations.

In contrast to the information deficit model, the public engagement model assumes that non-scientists bring their own knowledge, beliefs, and goals to the table when engaging with science (Feinstein, 2011; Llorente et al., 2019; Wu et al., 2019). This model conceptualizes non-scientists as active learners who reflect on scientific content (Marteau et al., 1998). Further, the model assumes that, instead of a one-directional top-down communication, science communication is best characterized as a reciprocal relationship between scientists and the public (Dudo & Besley, 2016).

This perspective can account well for the evidence presented throughout Chapters 3 and 4. If non-scientists do not just passively receive scientific information but instead actively reflect on this information in relation to their own values and beliefs (Llorente et al., 2019; Marteau et al., 1998), it seems likely that not all scientific knowledge has positive effects. Moreover, if the link between the public and the scientific community is best characterized as a relationship (instead of a passive sender-receiver communication), then information about (perceived) transgressions and mistakes would likely damage public trust (Dirks et al., 2009; Kim et al., 2006).

A public engagement perspective further highlights the relevance of the present research. If non-scientists actively engage with scientific information, it becomes more likely that they learn about scientific debates and movements such as the replication crisis or the open science movement, even if this information is not actively communicated to them. It, therefore, seems important to consider, for all aspects of the scientific enterprise, how these aspects are perceived by the public. The research projects presented in Chapters 3 and 4, therefore, fit well within the scope of a public engagement model.

It should however be noted that the public engagement model is very broad, and it is difficult to derive precise empirical predictions from this model. Even though it is valuable to characterize non-scientists as active learners, who engage with and reflect on scientific content (Marteau et al., 1998), this view makes no direct predictions on how exactly they will react to certain scientific debates, movements, or practices. For example, one could also predict that non-scientists could interpret the replication crisis as a positive sign of self-correction (Mede et al., 2020) and not as a sign of low trustworthiness of science. It would therefore be a valuable endeavor for future research to develop more specified models that allow more clear-cut predictions, for example on whether and how people will react to low replicability or open science practices.

5.2. LIMITATIONS

One general limitation relevant for Chapters 2 to 4 is that all investigations were carried out in a transparent study context (i.e., participants knew that they took part in a study), and the dependent variables were assessed using self-report scales. This makes applying our findings to real behavior difficult, due to potential limitations such as demand- and experimenter effects (Orne, 1962; Rosenthal, 1963). Future work may benefit from using field studies and measuring real behavior (Cialdini, 2009).

For example, in the studies presented in Chapter 2, we asked people to self-report their adoption of a variety of protective behaviors during the COVID-19 pandemic. However, such self-reports of health behavior may be biased, for example due to social desirability or memory biases (Bish & Michie, 2010; Blome & Augustin, 2015). Such biases could be addressed by using experience sampling methods, which are less reliant on memory (Mareva et al., 2016), or by using field observations that are less influenced by the experimental context (Cialdini, 2009; Piff et al., 2012). For example, behavior in a shopping mall (e.g., social distancing or mask-wearing) could be observed covertly and correlated with subsequently assessed trust in science.

Similarly, regarding the work presented in Chapter 3, it is important to consider that we created the information (e.g., on the replication crisis or on the open science movement) ourselves as part of the study materials. It would be interesting to investigate how people react to such information in the real world, for example by using real media reports about the replication crisis and to test whether these have similar effects on trust in science. This is particularly interesting because past work produced conflicting findings, for example on the effects of information on open science (Altenmüller, Nuding, et al., 2021; Anvari & Lakens, 2018; Schneider et al., 2019, see also Chapter 3). This suggests that these effects may depend on how open science is explained, which is a promising direction for future research. Furthermore, it would be interesting to use behavioral measures of trust in psychological science, for example, whether participants would like to subscribe to a psychological newsletter, as information seeking is an important aspect of public engagement with science (Altenmüller, Nuding, et al., 2021; Siebenhaar et al., 2020).

The same reasoning regarding external validity also applies to Chapter 4. Here, it would be interesting to investigate if the effect of our explanation on perceived credibility could also be observed in the field. For example, explanations may be experimentally added to a set of real preprints on a preprint server, and real behaviors, such as sharing behavior in social media, could be overserved (O'Brien et al., 2021). Even when relying on more controlled experiments, it could be valuable to use more realistic stimuli. In our work, we explained preprints in a short text, presented without any distracting information. However, in reality, preprints are presented alongside a variety of other pieces of information, which also include social cues such as downloads or endorsements (Soderberg et al., 2020). It would be relevant to investigate whether our explanation also works when presented with such additional relevant information (T. Koch et al., 2021).

Another limitation of the studies presented throughout Chapters 2 to 4 (except for Chapter 2, Study 2) is that the studies relied on convenience samples, often consisting of online crowd workers or students. Even though crowd worker samples are rather diverse and results often can be generalized to the general population (Casler et al., 2013; Coppock et al., 2018), it would be reassuring to replicate our central studies in samples representative of the general population. This is particularly relevant because we focused on public trust in science, and it would be desirable to get a more representative view of the public's opinion. Especially the findings presented in Chapter 3 have subsequently been criticized for relying on non-representative samples (Mede et al., 2020).

Moreover, it would be interesting to study our effects in samples with a strong applied or theoretical relevance. For instance, the adoption of COVID-19 related protective measures is lower among people who endorse conspiracy theories (Imhoff & Lamberty, 2020), and therefore it would be relevant to investigate the role of trust in science in such a sample. Certain conspiracy theories seem to reflect distrust in scientific competence (e.g., assuming that the COVID-19 Coronavirus has escaped from a lab) and others relate to distrust in scientific honesty (e.g., assuming that the COVID-19 Coronavirus is a hoax made up by scientists), indicating that common conspiracy theories (Imhoff & Lamberty, 2020) relate to different components of trust (Hendriks et al., 2015). This could be a promising direction for future research to explore. For example, one could predict that increasing the perceived competence of scientists would reduce the belief that they accidentally released the coronavirus from a lab, but could ironically strengthen beliefs that scientists have the ability to fake a global pandemic (e.g., the hoax belief), suggesting a complex interplay between trust in science and the endorsement of conspiracy theories. For Chapter 3, it would be particularly interesting to study the effects of lower replicability on trust in science among policymakers, because their perception strongly matters for psychology's societal impact (Ruggeri, 2016; Ruggeri et al., 2021). Regarding Chapter 4, it would be relevant to study the effects of our explanation on people who have a relatively low scientific literacy, as a moderation analysis suggested that the explanation might be ineffective in such a situation. Potentially, adapted versions of our explanation could be more effective in such a sample.

Finally, Chapters 3 and 4 investigated trust in science in the context of research findings from psychology and the social sciences. It seems possible that the observed effects (e.g., of low replicability) would differ when other scientific areas are investigated, for example, the more traditional "hard" sciences, such as physics, chemistry, or biology. Past work already showed that non-scientists perceive these disciplines to be different (e.g., more scientific) than psychology and the social sciences (Allum, 2011; Suldovsky et al., 2019) and thus the public reaction to information about these disciplines might differ. Future research could address this limitation by replicating the studies from Chapters 3 and 4, with a focus on other scientific disciplines. Addressing these and the other limitations outlined throughout Chapters 2 to 4 would be a valuable endeavor for future research.

5.3 Directions for Future Research

There are further promising directions for future research, going beyond explicitly addressing limitations. For example, Chapter 2 highlights that trust in science strongly correlates with the acceptance and adoption of protective measures during the COVID-19 pandemic. As argued above, it would be interesting to test the external validity of this finding for example in field studies. However, it would also be relevant to empirically test whether this correlation reflects a causal effect of trust in science on COVID-19 related behavior, and first research projects in this direction have already started (Dohle et al., 2021). But how could trust in science be manipulated? Perhaps, the work presented in Chapters 3 and 4 could provide useful directions. The work in Chapter 3 suggests that scientists who routinely produce unreliable (e.g., non-replicable) knowledge are perceived as less trustworthy. Likewise, research findings that are not quality controlled (i.e., peer-reviewed) are seen as less credible (Chapter 4). Describing science as either lacking quality control and/or as producing low-quality outputs may be a promising way to manipulate trust in science and test downstream consequences, for example for the acceptance of recommended measures.

If established as a causal effect, it would further be interesting to investigate the underlying process, to understand why trust in science leads to the acceptance of recommended measures. It is important to note that during the COVID-19 pandemic, officially recommended measures (in Germany) were also typically recommended by scientists (e.g., Grote et al., 2021). According to the Elaboration Likelihood Model (Petty & Cacioppo, 1984, 1986), messages from trustworthy sources are more persuasive especially when participants lack the ability or motivation to engage with the content of the message (see also Chaiken et al., 1989; Siegrist, 2000, 2021). In such a situation, people are more likely to become attentive to peripheral cues, such as source characteristics (Petty & Cacioppo, 1984, 1986). This perspective thus conceptualizes trusting science as a form of peripheral/heuristic processing. This suggests that when people become more motivated or more able to engage with scientific messages (making peripheral processing less likely) the influence of trust in science on the adherence to recommended behaviors is reduced. This reasoning leads to some interesting predictions. For example, when people become more educated about COVID-19, they may feel enabled to rely on their own opinion instead of relying on scientific recommendations. Such potentially detrimental effects of making scientific knowledge accessible have been observed in other areas of science communication and are known as 'the easiness effect' (Scharrer et al., 2017).

Regarding the results from Chapter 3, one of the most important directions for future work will be to find ways how to communicate low replicability without damaging public trust. Of course, one could argue that such a damaged trust might be a justified consequence of low replicability. However, this argument overlooks the possibility that such a public backlash also works as an incentive against conducting replication studies (Białek, 2018), as aspiring replicators may be afraid to damage the reputation of their field (Baumeister, 2016) and thus refrain from conducting valuable replication studies. Therefore, it is important to find ways to communicate low replicability without damaging public trust, perhaps by framing the replication crisis more strongly as a necessary form of self-correction (Altenmüller, Nuding, et al., 2021; Mede et al., 2020). This would also be helpful for lecturers in psychology, who might worry that teaching about the replication crisis could otherwise damage students' attitudes towards psychology (T. Wingen & Berkessel, in press).

Another worthwhile direction for future research would be to reconcile some inconsistencies between our work (presented in Chapter 3) and other work on the effect of open science practices on public trust. In our work, information about open science practices had no significant effect on public trust in psychology, and a related study even found a detrimental effect of explaining open science reforms on trust in future research (Anvari & Lakens, 2018). Notably, however, both studies described open science reforms as a field-wide movement and measured trust in either the whole research community (Chapter 3) or in all research findings (Anvari & Lakens, 2018). In contrast, other work that investigated open science practices focused on individual researchers or studies, for example by describing a specific researcher to be open to adopting open science practices (Altenmüller, Nuding, et al., 2021) or by highlighting specific articles that received open science badges (Schneider et al., 2019). Importantly, this work found positive effects of open science practices on public trust (in these specific researchers and research findings), contradicting our results. Perhaps this different focus on individuals versus groups could explain the differences between these findings, as the perception of individuals and groups can differ (Nicolas et al., 2021), potentially also related to the level of mental construal (Trope & Liberman, 2010). Engaging in open science practices may only affect the reputation of individual researchers, but not of the whole research community. Importantly, this would still constitute an incentive for researchers to engage in these practices, thereby moving research fields as a whole forward towards more openness.

Regarding the work presented in Chapter 4, it would be rewarding to test if our explanation of preprints is also effective when preprints are discussed in the media. Indeed, preprints are routinely featured in the traditional and new media (e.g., Chivers, 2020; Niggemeier, 2020), often without any detailed explanation. Moreover, non-scientists are probably much more likely to encounter research results from preprints in the media, compared to reading the actual preprint. It is therefore highly relevant to investigate whether, even in such a situation, non-scientist can sufficiently adjust their credibility rating after receiving a short explanation of preprints. If an explanation also works in this context, this could help overcome a dilemma recently highlighted by science journalists: They are pressured to be the first to report new research findings (otherwise it would not be news after all) but at the same time they want to avoid confusing their readers with preliminary and potentially unreliable findings (Sheldon, 2018).

5.4. CONCLUSION

To sum up, this dissertation contributes to the literature on trust in science in two important ways. First, this dissertation adds to a growing body of evidence that trust in science is a central predictor of beneficial behaviors (Rutjens et al., 2018; Soveri et al., 2021; Sturgis et al., 2021), particularly during the COVID-19 pandemic. Second, this dissertation demonstrates that public trust in science can be influenced by the scientific community itself:

Public trust in science is (sometimes) influenced by scientific controversies such as the replication crisis and by specific research practices such as publishing preprints.

Therefore, when discussing methodological issues or proposing reforms, the scientific community needs to keep in mind that these debates do not happen in academic seclusion within an isolated ivory tower. Instead, such discourses can reach the public and have the potential to influence public trust in science. This does not mean that necessary reforms should not be taken just because the public might disapprove, but that this perspective at least needs to be considered. This is especially important because such information is nowadays much more likely to reach the public, due to the rise of social media and science communication (Collins et al., 2016; Huber et al., 2019).

Moreover, the results presented in this dissertation suggest that non-scientists are not just passive receivers of scientific information who just need to be educated (i.e., an information deficit perspective), nor that trust in science is simply related to a stereotypic image of scientists (i.e., a stereotype content perspective). Instead, our findings suggest that non-scientists react to complex methodological details, such as replication studies or publication modes. This suggests that non-scientists actively engage with and reflect on scientific context, an observation in line with a public engagement perspective.

Overall, this dissertation provides insights on predictors as well as on consequences of public trust in science, together with valuable directions for future research. Following these directions and deepening our understanding of trust in science is crucial: In times of climate change, pandemics, and the global rise of populism and conspiracy theories, trust in science is more important than ever.

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