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Social Psychology Should Be a Science of Feelings, Thoughts and Behaviour

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Abstract

This article is a polemic with the eminent psychologists whose articles are published in the present edition of this journal. These articles present their views on the introductory article of the same volume titled "Is psychology still a science of behaviour?" The author of this article concurs with his polemicists that the fundamental task of psychology should be to explain behaviour, not merely to predict it. That said, he argues that in order for this to occur, psychology must, by necessity, study real human behaviours (which, in the opinion of some of the polemicists, is not an imperative). At the same time, he admits that an understanding of behaviour also requires studying what people think and feel.

Keywords

social psychology; behaviour; experiments; science of finger movements

The article "Is psychology still a science of behaviour?" (Doliński, 2018, this issue), which opens this edition of Social Psychological Bulletin, and which I wrote at the invitation of the editors, was an opportunity for me to present my diagnosis of the present state of social psychology. I presented the thesis that, at present, we are exploring real human behaviours to a very small degree, while we are far more frequently asking people to imagine that they are in a given situation and to declare how they would behave in it. The argument that contemporary psychology is abandoning the study of real behaviour is not a novel one. It has previously been written about in a controversial text by Baumeister, Vohs, and Funder (2007), whose article I invoked in my own. The result of my analysis of the content of one

volume of *Journal of Personality and Social Psychology* is that the process of departure from the study of real behaviours has gathered strength of late. In my article I also present several of what I believe are the root causes of this state of affairs, and the issues associated with them.

I am greatly pleased that outstanding scholars of international renown have given their comments on my thoughts. Here I would like to offer them my deep gratitude. Some of their texts are complementary to my perspective, while others present a clearly different assessment of the circumstances our discipline finds itself in at present. A careful reading of all the articles has also demonstrated to me that I have not always succeeded in precisely presenting my own ideas, owing to which readers of the polemics might get the mistaken impression that I disagree with other authors regarding issues on which we are actually in agreement. Thus, on the one hand, I consider the present text as an opportunity to provide my own views with greater clarity, while, on the other, it presents a chance to engage in polemics with my adversaries regarding those points on which we occupy different positions.

Is There Reason to Worry?

The key question is whether psychology's abandonment of the study of behaviours that extend beyond the completion of survey instruments by participants and their providing answers to questions about how they would behave in defined situations is cause for concern in our discipline, or whether we should not trouble ourselves worrying about it. Let us begin by exploring whether I am at all correct in my judgement that psychology has abandoned such studies. The same concerns have been expressed by Wolfgang Stroebe (2018, this issue), who observes that both the observations of Baumeister, Vohs, and Funder (2007) and my own regarding the small number of studies of real behaviours may be inaccurate for two reasons. Firstly, we examined only the content of articles published in one journal - *Journal of Personality and Social Psychology* – and secondly, it cannot be ruled out that "observational studies remained constant, but the number of studies that examined internal processes increased" (p. 3). Regarding the first of those remarks, it must be said that he is correct. Thus, for the purposes of the present polemic, I have reviewed the content of editions of three other leading journals in social psychology available to me at the time of writing. In *Personality and Social Psychology Bulletin* (vol. 44 no. 5), studies of behaviours other than self-descriptions and/or self-judgements are presented in just one of eleven articles; in *Social Psychological and Personality Science* (vol. 9 no. 2), one of fifteen articles; and in *European Journal of Social Psychology* (vol. 48 no. 3), in two of eleven articles. Of course, I am aware that this data cannot be considered fully representative of all journals over a longer period (e.g., the last decade), but it does suggest that the rarity with which *Journal of Personality and Social Psychology* publishes articles presenting real human behaviour is not exceptional. When it comes to the assumption by Wolfgang Stroebe that the number of studies in which observations of behaviours

were conducted has remained more or less constant over the last half-century (merely accompanied by a growth in studies of internal processes), this numerical data negates that statement in a more than unequivocal manner. My calculations, which I presented in the article opening this edition, show that the last volume of *Journal of Personality and Social Psychology* contained 18 such studies. In accordance with the analysis presented by Baumeister, Vohs, and Funder (2007), in the period from 1966 to 1980, the percentage of studies addressing real human behaviour oscillated between 50% and 80%. Even if we assume the lowest value (50%), this would mean that the argument stating that the number of empirical studies examining real behaviours has not declined could be defended if a mere 36 (e.g., 2 × 18) studies were presented of any and all types of empirical studies in one volume of the journal. This would mean that in one edition of *JPSP*, only six empirical studies should be presented. It is a fact that both the number of articles in a given edition was lower several decades ago than now, and that a given article presented a smaller number of studies than at present (frequently only one!); nevertheless, these data clearly demonstrate that there are simply far fewer studies of real human behaviours today than was the case in the 1960s and 70s.

Is this cause for concern? An extreme position on this issue is represented by Kruglanski, Factor, and Jaśko (2018, this issue) in their article with the meaningful title "Is 'behavior' the problem?" The authors claim that my concern over the lack of interest in social psychology of real behaviours is entirely groundless. They cite the definition of behaviour given by Johnston and Pennypacker (1993, p. 23): "behavior (…) is that portion of an organism's interaction with its environment that is characterized by detectable displacement in space through time of some part of the organism and that results in a measurable change in at least one aspect of the environment". They go on to state that "The breadth (and vagueness) of the concept of behaviour is best captured by Mallot and Trojan Suarez (2004, p. 9) striking 'dead man test': 'If a dead man can do it, it ain't behavior. And if a dead man can't do, then it is behavior.'"

If we were to accept the argument that behaviour thus defined should be the object of interest of social psychology, we should be consistent and accept that the most important behaviours of a human being are breathing and heartbeats. These actions in all certainty not only clearly distinguish a living individual from a corpse, but they are also behaviours that constitute a *sine qua non* condition of the individual exhibiting other behaviours. If we concur with this line of reasoning (and it would be difficult to argue otherwise), then we should remain consistent and accept that the majority of theoretical conceptions in social psychology are utterly worthless, as they do not explain, to even the slightest extent, either heartbeats or breathing. The theories (doubtlessly of exceptional import and value for psychology) presented in the literature by Arie Kruglanski would also have to be discarded. Thankfully, there is no danger of this happening because we all consider it obvious that if we in social psychology are discussing behaviours, what we have in mind are reactions of an individual resulting directly or indirectly from the influence of other people, and which generate real consequences for both the individual and others.

Is What We Study Important?

Does the formulation of judgements and opinions by people (including about their own predicted behaviours) itself constitute behaviour? The answer is: most certainly yes! What is more, as Klaus Fiedler (2018, this issue) rightly points out, verbal behaviours are in no way less valuable than behaviours of a non-verbal nature. In examining people's convictions, judgements, and opinions, social psychology is studying human behaviours. Indeed! But it should also examine other human behaviours – those that go beyond the sphere of verbal declarations, as, by definition, they constitute the object of its interest. I can thus in no way concur with the positions presented by Garcia-Marques and Ferreira (2018, this issue) and Kruglanski, Factor, and Jaśko (2018, this issue). Garcia-Marques and Ferreira write: "it does not really matter what kind of behaviour is being studied in psych labs, as long as this behaviour is meaningful, relevant to social goals and/or diagnostic of key underlying mental processes" (p. 5). Kruglanski, Factor, and Jaśko (2018, this issue) also clearly ignore the relevance of that which should constitute the object of study by social psychology. On the one hand, they do agree with me that the authors of studies should clearly indicate in an article that they are studying, for example, people's judgements as to whether they would behave altruistically in a given situation, rather than studying real altruism; however, on the other hand, they declare that there is no need to study altruism, because, after all, the formulation of judgements about one's own behaviour is itself a behaviour as well. In polemicizing with both my article and that of Baumeister, Vohs, and Funder (2007), they state in the conclusion to their own article that: "We would like to believe that what these authors really mean is the need to make social and personality research more valid, generalizable, and consequential. Ultimately, the issue is not really about behaviour" (p. 5). Of course, it is difficult to disagree with the first of those two sentences. By the same token, it is equally difficult for me to agree with the second. Social psychology should study real human behaviours. If we do not, it will simply become another science, one concerned exclusively with judgements and convictions. Will it be a better one? If physicians limited themselves to analysis of the verbal declarations of their patients, without examining the real functioning of human organs, we would consider this curious at the very least. It is certain that such conduct would not make any meaningful contribution to the advancement of the medical sciences.

As social psychologists (researchers, reviewers, editors of journals), we should also take a critical view of all temptations to equate behaviours with declarations concerning behaviours, something which Klaus Fiedler (2018, this issue) points out in his article. If, for example, a study participant responds to the question "In the situation described, would you help someone who has fallen down on the pavement?", we are not examining altruistic behaviours, but rather declarations concerning one's own potential altruistic behaviours. As demonstrated by Wojciszke and Bocian (2018, this issue), results thus generated could be (and usually are) different depending on whether we are examining behaviours or declarations about them. For clarity's sake, I must emphasize here that I am

not opposed to asking people questions about their predictions as to their own behaviour. What is more, I am convinced that the study of such declarations can be both interesting and important (constituting a scientific problem in and of itself). However, it should not be equated with the study of real behaviours, nor should it remove from the field of interest of social psychologists the issue of whether an individual really does offer help in a specific situation.

On the Various Movements of Fingers

A particular class of extraverbal behaviours consists of finger movements. Karl Teigen (2018, this issue), in an article titled "The unbearable lightness of finger movements", invoking associations with the famous masterpiece by Milan Kundera ("The unbearable lightness of being"), points to the universality of behaviours consisting of pressing keys on a keyboard with one's fingers. Teigen decided to observe the behaviour of passengers in a train. The overwhelming majority of them, eyes focused on their smartphones, were continually pressing keys with their fingers. Only a few individuals were sleeping or attempting to fall asleep. Teigen did not record any other behaviours. Similarly, Wojciszke and Bocian (2018, this issue) point out that in the modern world, people's real behaviours come with exceptional frequency in the form of pressing keys on a computer keyboard or the screen of a smartphone. It is difficult to disagree. Teigen's article inspired me to perform a sort of replication of his "study". I engaged in observation of travellers in a Polish train travelling from Wrocław to Warsaw. Indeed, pressing keys on a smartphone or laptop was a widespread behaviour. But these people were also reading books and newspapers, listening to music through headphones, reacting to the instructions of railway employees checking tickets, leaving to go to the restroom or dining car, and returning to their seats. Primarily, however, they got on and got off at stations. In his article, Teigen addresses my argument that real behaviour is frequently of a binary character. He writes: "'Real' (physical) behaviour is not as binary as Doliński claims." (p. 3). I would like to point out in this context that the passengers I observed either completely got on or got off, or they completely did not disembark at all. And as it turns out, their behaviour was binary. Of course, we could analyse how quickly they got on and off, how gracefully they did so, how many bags they were holding in their hands, how many minutes they waited in the corridor at the exit prior to the train arriving at its destination. But all of this is far less import than the binary act of entering and disembarking. I don't think that the fact that Teigen observed something different in trains in Norway than I did in Poland was the result of cultural differences between our two countries. Rather, I would suggest that the observer sees what they want to see. Human behaviours are frequently binary and are not restricted to the movement of fingers. Thus, the study of human behaviours by social psychologists should not be limited to the examination of finger movements, even if those are frequent behaviours. At the same time, I am not at all suggesting that studies of real behaviours in which participants press specified keys is illegitimate. However, fundamen-

tal questions remain to be answered here, as I have clearly not done a sufficient job of doing so in "Is psychology still a science of behaviour?"

Kruglanski, Factor, and Jaśko (2018, this issue) seem to be surprised by my criticism of the dominance of "finger movements" in social psychology, pointing out that, after all, Milgram's participants also pressed keys. So, employing the language of action identification theory (Vallacher & Wegner, 1987, 2012), we may state that they were pressing keys only at a very low level of action identification. At a higher level of identification, Milgram's participants were inflicting pain (to be more precise, they believed they were inflicting pain) on a person sitting behind a wall; they proved incapable of behaving in a manner contrary to the expectations of the experimenter and consistent with their own moral norms. The majority of participants in contemporary psychological studies press the keys on a computer keyboard at a low level of identification, while at a higher level of identification they are describing themselves, responding to survey questions, estimating on a scale of 0 to 9 the probability they would purchase a bicycle, etc. In today's psychological experiments, pressing keys on a keyboard is thus not usually the behaviour expressly inflicting real consequences on the participants and/or other people. Thus, such clicking rarely constitutes, for example, confirming a bank transfer, deceiving or offending other people, inviting someone on a date, or provoking aggression. Thus, the primary issue is not the use of a computer keyboard in studies, but rather the significance of pressing particular keys by an individual. By the same token, I agree with Garcia-Marques and Ferreira (2018, this issue), who write that "key pressing can be meaningful behavior" (p. 1). Similar declarations can be found in Wojciszke and Bocian (2018, this issue) and Jetten and Haslam (2018, this issue). The problem, therefore, is not that social psychologists conduct their studies in paradigms in which people are supposed to press keys on a keyboard. Rather, the problem is with what the pressing of those keys signifies. Let us observe that in my analysis of the last edition of *Journal of Personality and Social Psychology*, I qualified as studies of real behaviours all experiments in which pressing keys on a computer keyboard went beyond completing a questionnaire and responding to questions about one's own hypothetical behaviour in the presented hypothetical situation.

Analysis of the Unequivocal Consequences of Behaviours Is Equally as Valuable as Analysis of Behaviours

A very interesting observation related to analysis of behaviours is made by Wolfgang Stroebe (2018, this issue). He writes with exceptional accuracy that learning about the real behaviours of individuals does not mean we have to observe their behaviour. Rather, we can draw conclusions from the effects of their behaviour. He cites a very convincing example from his own research: if someone's weight has clearly dropped after they had to struggle with the temptations of various food, then this weight drop is an excellent indication that they have avoided fatty food. If a social psychologist were to study altruism by treating a bank transfer to the account of a charitable organization as an indicator of behaviour, I

would also applaud such an approach, and I would fully concur that there is no need to directly observe people in the process of performing the transfer. In turn, in studies on pro-ecological behaviours, reduced use of water and electrical energy can be outstanding indicators of pro-ecological behaviours. Trends in the value of bills for telephone calls and analysis of billing records indicating individuals with whom conversations were held can be a good indicator of trends in the social contacts of an elderly, ill, homebound person. The content of death certificates is also a good, and potentially even better basis for conclusions about the death of study subjects than direct observation of their deaths. In all of these cases, however, we are dealing with the behaviourally unambiguous (or practically unambiguous) consequences of behaviours, and not the verbal declarations of study participants about their own hypothetical behaviours. An individual who uses a computer keyboard to perform a bank transfer to support a female rape victim is exhibiting a real social behaviour. In turn, someone using the same keyboard to respond to a question about their readiness to assist a victim (on a scale of 0 to 10) is merely formulating a judgement about their own, hypothetical behaviour. If it were the norm for social psychology experiments to use the dependent variables described by Stroebe, I would never dream of writing an article titled "Is psychology still a science of behaviour?" Unfortunately, in the articles I analysed from the entire volume 113 of *Journal of Personality and Social Psychology*, I did not come across a single use of such dependent variables, which justifies the authoring of such text.

Can the Meaning of Behaviours Be Studied Without the Study of Behaviours?

A careful reading of articles published in this volume of SPB allows me to make the assumption that we are all in agreement as to the necessity of social psychology studying the meaning of behaviours rather than the behaviours themselves. "Human beings are symbolic beings that care more about the meaning of an act than the act in itself", write Garcia-Marques and Ferreira (2018, this issue, p. 4), while Kofta (2018, this issue) observes with exceptional incisiveness that "objective behavior is nothing more than an expression of internal dynamics of the human mind (p. 3) . Other authors express similar views. Nevertheless, if we intend to follow the dominant research trend – in other words, to ask study participants to imagine a situation and then induce them to respond to a question about how they would behave in such a situation, or ask participants to spend an hour completing various surveys, we will simply not be studying real behaviours, nor will we get any closer to an understanding of the causes of those behaviours.

I am in total agreement with Kofta (2018, this issue) that the focus of contemporary psychology on perceiving the individual as an object of their own actions, rather than on the behaviours themselves exhibited by people, attests to the fact that psychology has become a mature scientific discipline. If we include in this mix extensive methodological and statistical refinements (Pettigrew, 2018) that allow us to observe the processes and mecha-

nisms undergirding behaviours, we should be truly satisfied. What, then, is the problem? Kofta observes that while psychology rarely examines behaviours as such, we have no reason to worry because this subject matter is present in interdisciplinary studies which psychologists are participants of. However, in this context, it is difficult not to ask if this means that these "other disciplines" themselves are not mature, considering that their practitioners, working in concert with psychologists, clearly insist on studies of behaviours? And if these sciences are not immature, what has led to the state of things we have just described? And why is it that psychologists conducting interdisciplinary studies explore behaviours, whereas in their own studies they prefer to ask people what they think? Let us therefore ask the key question: Can social psychology be a mature science in the way Kofta understands but at the same time study the mechanisms underlying real behaviours? Why not? In my opinion, the avoidance of such studies has nothing to do with the maturity of our discipline. I would venture that the arguments employed by other authors in this edition (Garcia-Marques & Ferreira, 2018, this issue; Kruglanski, Factor, & Jaśko, 2018, this issue) to explain the aversion of social psychologists to the study of real behaviours are symptomatic. The reason is that such studies are of course difficult, time-consuming and laborious. It is far easier and quicker to perform a series of studies on convictions, opinions, and judgements. But the speed and ease with which studies can be completed is unacceptable as a criterion for determining the development of science.

It's so Good That It's Bad

Examination of the individual articles in this edition clearly demonstrates how diverse particular authors' assessments of the current state of social psychology are. The most enthusiastic among them are probably Garcia-Marques and Ferreira (2018, this issue), who optimistically declare that the crisis in psychology resulting from insufficient links between empirical studies and theory "was overcome by new and improved theoretical proposals that led to innovative (theory-dependent) research methods" (p. 4). There is no way I can agree with this position. It is similar to hotel management deciding that the solution to a leak in one of the rooms is to close the entire floor to guests. Overcoming the crisis of social psychology consisting of the weak anchoring of studies of real human behaviours in psychological theories cannot be done by neglecting to study of such behaviours. Here I agree with Klaus Fiedler that the enthusiasm expressed for these "innovative research methods" is not always justified. Contemporary social psychology is replete with studies in which the reactions of participants, contrary to the convictions of researchers, are far from unambiguous. The speed of mouse clicks during studies on prejudice need not necessarily attest to the scale of real prejudices, while the completion of words in which some letters are missing is not necessarily indicative of the tendency to behave aggressively. Contrary to appearances, this problem may also affect indicators of a neuropsychological character, which, to psychologists (see e.g., Kofta, 2018, this issue), usually seem unambiguous. However, they are *de facto* frequently anything but unambiguous. The following example

may be given: studies were conducted of the brain activity of people playing a computer game. A player observes two people on a screen throwing a ball back and forth. From time to time, they also throw the ball towards the player, who then tosses it back to one of the characters on the screen, only to receive it again shortly thereafter. In a certain phase of the game the characters on the screen stop throwing the ball to the player, only passing it back and forth between themselves. It was demonstrated that in such circumstances there is an increase of activity in the region of the anterior cingulate cortex. Because this region of the brain is active during the experience of somatic pain, the researchers concluded that they had evidence of social pain – a reaction to ostracism by a group (Eisenberger & Lieberman, 2004). The paradigm of this computer game has thus become the primary one in social psychology for examining such issues as ostracism and bullying. However, *de facto* the same region of the brain clearly increases its activity levels when an individual becomes interested in some kind of problem (see, e.g., Behrendt, 2012), which would be entirely understandable in the context of the aforementioned game: the individual is surprised and wonders why the action has changed, why the characters on the screen are no longer sharing the ball. Thus, the same indicator (increased activity in a specified part of the brain) can be the result of (at least) two entirely different psychological processes. Let us observe as an aside that this ambiguity has not provoked any reflection at all on the part of researchers engaged in the study of bullying and ostracism, and thus the described computer game is used in a wide range of studies. As long as social psychology is based on such murky indicators, I will continue to insist on the necessity of studying real behaviours.

Against Studies "Instead of"

This is an appropriate place to make an important declaration. I completely agree with my adversaries that the social cognition trend has contributed to a significantly better understanding by psychology of the processes involved in individuals' processing of social information (see primarily: Garcia-Marques & Ferreira, 2018, this issue; Kofta, 2018, this issue; Stroebe, 2018, this issue). This section of social psychology, in which the default mode is to study not real behaviours but rather verbal declarations (thus, e.g., stereotypes, changes in attitudes, value structures, self-awareness, convictions about the nature of the social world, etc.), has indeed made tremendous progress. At the same time, regression can be observed where it is natural to examine real behaviours rather than verbal declarations. As it is, the study of altruism in contemporary psychology has been replaced by the study of convictions about one's own altruism; the study of aggression by the study of convictions as to one's own aggression level; the study of discrimination by the study of verbal declarations about discrimination. By no means am I saying that social psychology should abandon the study of convictions, opinions, or emotions felt. However, I am saying that it should examine real behaviours in those areas where it speaks of such behaviours. My point is not that the study of beliefs as to one's own altruism is a worthless endeavour. Just the opposite: such empirical determinations can be of exceptional importance. But these should not be

studies "instead of" – in other words, studies of beliefs rather than an attempt to understand the reasons as to why people behave altruistically. A chapter titled "Altruism" in a contemporary social psychology textbook can and should present studies on beliefs about own altruism, but it cannot be comprised solely of presentations of such studies. It must also present studies of real acts of altruism. The same is true for chapters on aggression, prejudice, or morality. And if we agree that social psychology textbooks should look like this in ten or twenty years' time, we must study real behaviours.

Concluding Remarks

The problem our discipline is presently facing goes beyond the mere question of the object of attention in our studies. Klaus Fiedler (2018, this issue) also devoted significant space in his article to the issue of how studies are designed and how their results are calculated. I would like to say very clearly that I fully agree with his diagnosis that this is one of the key issues facing contemporary social psychology. We are inundated with a tremendous amount of empirical studies presenting graphs containing structural equation models, in which the number of factors taken into account and a muddle of dozens of arrows present something incomprehensible not only to the reader, but probably to the authors as well. Leonel Garcia-Marques and Ferreira (2018, this issue) disagree with me and state that the 1960s and 70s were not really the golden age of social psychology. Proof here includes the famous "fun and games" critique of Ring (1967). Sharing the criticism by Ring, at the same time I do not doubt at all that this "fun and games" gave us far more information than the studies common today in which people complete a battery of questionnaires, and the authors publish a diagram displaying a mass of arrows. Several authors in this edition of the journal refer to studies by Milgram on obedience. Nevertheless, despite Milgram's scant interest in theory, the empiricism he presents remains one of the most interesting empirical studies in social psychology. And while this is an exceptional case, it is this empiricism ("a fact in and of itself") that demanded that psychology take a different view of the mechanisms determining a person's behaviour. And this fact is not diminished even by the serious controversies concerning the fundamental thing – in other words, what, exactly, Milgram succeeded in demonstrating (see: Jetten & Haslam, 2018, this issue). Thus, if the 1960s and 70s were not the golden era of social psychology, the present years are most certainly not either.

Karl Teigen (2018, this issue), in turn, takes issue with my hypothesis that one of the causes underlying social psychology's abandonment of the study of real behaviours was a cognitive shift. He writes: "But the so-called cognitive revolution did not displace a previous focus on naturally occurring human behavior, it displaced a previous focus upon responses of caged rats!" (p. 2). Teigen failed to take two things into account. First, social psychologists have with extreme rarity engaged in examining the behaviour of rats enclosed in cages. Second, the cognitive shift became ascendant in social psychology later than in motivation psychology or learning psychology. Teigen's argument would thus be

true in respect of psychology as a discipline in its entirety, but I maintain my position that the cognitive shift (if not cognitive revolution) played a tremendous role in concentrating the interest of social psychologists on the issue of people's beliefs, judgements, and opinions. At the same time, I agree with Teigen that the recent appearance of such inventions as internet-based surveys and online platforms like Amazon's Mechanical Turk, which social psychologists greeted with open arms, can be treated as one of the leading factors contributing to the dominance of the "finger movements" school.

Bogdan Wojciszke and Konrad Bocian (2018, this issue), in their article with the symptomatic title of "Bad methods drive out good: The curse of imagination in social psychology research", point out that the command "imagine that …" in psychological laboratories is not only heard or read by participants when psychologists take a measurement of a dependent variable. Manipulation of independent variables also takes place in precisely this manner with exceptional frequency. The authors write that Gresham's law concerns not only bad money driving out good,but is also significant for social psychology. "Cheap, easy, and low labor-intense methods substitute methods that are time-consuming, high labor-intense, and tough to obtain" (p. 1).

With my text "Is psychology still a science of behaviour?", I sought to draw attention mainly to the fact that difficult and time-consuming studies of real social behaviours are being replaced by easy and quick studies using the "imagine that …" paradigm. Klaus Fiedler (2018, this issue) suggests that I have perhaps adopted a poor persuasive strategy. He states that my appeal to increase the number of studies in which the dependent variable is a real behaviour would be more convincing if, rather than complaining about the lack of such studies, I had given positive examples, and thus demonstrated how just such an approach contributes to the development of psychological knowledge. At the same time, he relieves me of the task by subsequently listing a few suggestive and spectacular examples. I think that from this perspective it is highly important to confront two things: on the one hand, showing how valuable such studies can be (which Fiedler has notably done), while on the other, "complaining" that there are appallingly too few such studies in contemporary social psychology (which I myself have tried to do). I have thus allowed myself to treat the examples given by Fiedler and his drawing attention to the fact of the value of such studies as a very valuable complement to my argumentation.

At the end of this article, I will allow myself to return again to one of the responses published in this issue, the authors of which do not perceive the necessity of conducting such studies. Kruglanski, Factor, and Jaśko begin their polemic article with a quote from Henryk Sienkiewicz's "Deluge", a Polish historical novel published in 1886: "Every windmill thinks that flailing of wings is what counts". The accuracy of this quote in the context of the polemic on the condition of contemporary psychology is problematic at the very least, as these words are said by the knight Zagloba to his friends, the point of which is to illustrate the idea that battles and wars are not won by brute force, but rather through intrigue and ruses. Let us rather assume that the flailing of wings by the windmill is, for Kruglanski and his collaborators, a synonym of, as they write, "gross bodily movements,

and dramatic gestures" (p. 2) and has nothing in common with what Zagloba was really talking about. Zagloba's thesis might be true in relation to war, but does not translate to social psychology. One should agree that the windmill must flail its wings in order to grind the grain into flour. Asking the windmill if it would flail its wings, inducing it to imagine that it is flailing its wings, or asking it to complete a battery of questionnaires will not produce flour. And without flour, no bread can be baked.

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