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The Unbearable Lightness of Finger Movements: Commentary to Doliński

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Abstract

In the target article, Doliński (2018, this issue) showed that empirical studies of “real” behaviour are an almost extinct species of research, judged from articles published in the most recent volume of JPSP (Journal of Personality and Social Psychology). This finding continues a trend identified by Baumeister and colleagues ten years ago. The reliance on self-reports and rating scales can hardly be explained as an aftermath of the cognitive revolution in psychology, or a preoccupation with measurements and advanced statistical analyses, as Doliński suggests, but is more compatible with the ease of collecting questionnaire data, combined with the pressure to publish large multi-study papers and to obtain approval from ethical review boards. This development is further strengthened by the accessibility of online participant pools. An informal count showed that students participating for course credit were in 2006 involved more than 90% of empirical JPSP studies, as against 22.5% in 2017. In contrast, Amazon Mechanical Turk workers, non-existent in 2006, participated in 55.3% of the empirical studies published in the most recent volume. Parallel to this development the number of participants per study and the number of studies per article have vastly increased.

Keywords

social psychology; cognitive revolution; research participants; Amazon Mechanical Turk



Dariusz Doliński's target article is intriguing and alarming. Social psychologists profess to study people's thoughts, feelings and behaviours in social settings, but examining all empirical studies presented in the most recent volume of *JPSP* (Journal of Personality and Social Psychology), Doliński found only 6% reporting observations of "real behaviour". The remaining 94% presumably reported people's thoughts. That is, thoughts about opinions, values and feelings, and thoughts about the participants' own and others' behaviours, rather than observations of how they actually behaved. Such thoughts were collected during brief episodes of sedentary behaviour involving nothing more physical than "finger movements" on a keyboard. The finding is consistent with a trend identified by Baumeister, Vohs, and Funder (2007) ten years ago. These scholars had observed a reduction of behavioural studies from 80% to about 15% in *JPSP* over a 30-year period. Doliński's recent analysis has revealed a further decline.

Doliński intimates that social psychologists are so busy investigating presumable causes of behaviour that they forget to examine the actual behaviour that was to be explained. This may in itself reflect a widespread human bias, reminiscent of a concern expressed four hundred years ago by Montaigne: "They leave things and runne for causes [...] They commonly beginne thus: How is such a thing done? Whereas they should say: *Is* such a thing done" (de Montaigne, 1885/1603, p. 526).

A Search for Explanations

In keeping with his own preference for behaviours over explanations, Doliński makes a more convincing case for what social psychologists *do* (or do not do) than *why* they do it. He mentions just in passing that studying real behaviour is far more difficult and challenging than collecting keyboard strokes. But instead of expanding on this most obvious explanation, he highlights two more far-fetched ones: The cognitive shift in the 1960s and the present-day obsession with advanced statistical techniques. However, I doubt their importance.

As for the first: The influence of the cognitive revolution caused (in his view) a shift away from observations of behaviour to internal processes of a cognitive nature. But the so-called cognitive revolution did not displace a previous focus on naturally occurring human behaviour, it displaced a previous focus upon responses of caged rats! It remains true that the novel information-processing paradigm could be blamed for its reliance on highly artificial mini-behaviours of actors in front of a computer screen. But in this respect cognitivism continued rather than broke away from the mechanistic tradition inherited from behaviourism. One of the central spokesmen for the cognitive movement, Ulric Neisser, known for the first main textbook of the new approach (Neisser, 1967), was also one of the first to take a critical view. In his subsequent book, *Cognition and reality* (Neisser, 1976), he warned that the research inspired by the new approach had become disappointingly narrow, flawed by a lack of "ecological validity," downplaying the perceiving individual's

active rather than reactive role, and neglecting actors' interactive relationship with their environment.

In contrast, the classical studies of "real" human behaviour in social psychological experiments (like the seminal studies by Festinger, Asch, Milgram and Schachter) were rather an outgrowth of the Gestalt tradition and the impact of Kurt Lewin and his disciples on the American scene (Patnoe, 1988). This tradition could be regarded as allied to, rather than opposed to a general shift away from stimulus-response models to cognitions (e.g., Korman, Voiklis, & Malle, 2015). My point is that the absence of behavioural studies cannot be attributed to a predominant interest in cognitions. In fact, the graph of behavioural studies reported by Baumeister et al. (2007) shows a strong increase in the decade from 1966 to 1976, in apparent contradiction to Doliński's claim that social psychologists' neglect of behaviour was "clearly linked" to the cognitive revolution in the 1960s.

As for the second claim, about the effect of overly refined statistical models: I agree that many studies, especially in the last couple of decades, appear scientific mainly because of their sophisticated (and sometimes inscrutable) ways of handling data, demonstrating what Elster (2012) has called "hard obscurantism" in the social sciences. But such methods do not require self-reports or tick marks on a rating scale. "Real" (physical) behaviour is not as binary as Doliński claims, but lends itself to graded measurements on all kinds of physical scales: intensities, latencies, completion times, drops of saliva, eye movements, heart rates, and occurrence frequencies. A passion for measurement and statistics has been an integral part of behavioural studies from the beginning. Francis Galton suggested in 1884 that people's inclinations towards each other could be measured behaviourally by placing pressure gauges under the chair legs of his dinner guests. Norman Triplett's (1898) legendary social facilitation study measured performance time with a stopwatch. We still want measures of behaviour. A study of alcohol consumption would not be complete unless we knew how much a person drinks. In studies of aggression we would like to know the frequency and severity of actual fights. Regions of personal space can be measured in inches and feet, and studies of risk taking in economic psychology record amounts to be invested, gained or lost. In fact, statistical measures and models appear to be at least as applicable to behavioural indices as they are to thoughts.

So, if the cognitive revolution is not what started it, and the reliance on measurement and mathematical procedures is not what keeps it alive, what drives social psychologists' avoidance of real behaviours? Baumeister et al. (2007) suggested several mechanisms that are, in my opinion, no less important today than they were ten years ago. Here are two obvious ones:

- (1) The pressure to produce as many papers as possible, preferably with several studies and a large and powerful *N*. Students need to earn their doctorates in a span of a few years; post docs need a number of published papers to compete successfully for academic positions; professors, engaged in a Darwinian struggle for continued existence, understand they need a large batch of offspring to ensure that some of their

- ideas may survive and reproduce. They realize that they can keep their productivity unchecked by repeating rather than renewing their message and their methods.
- (2) More strict enforcement of ethical principles makes researchers shun away from procedures that have a potential of harming participants in any conceivable way. We cannot study people's need for intimacy by actually touching strangers or asking them to undress. The sheer intention to do so would be cut short by an institutional review boards (IRB), before the first participant would have a chance to report the incident to a #MeToo campaign. A more admissible project might still be delayed by the bureaucracy involved in obtaining ethical approval. Perhaps paradoxically, experiments of the Zimbardo, Milgram, or Darley and Latané type can nowadays only be staged by the entertainment industry in TV and film productions, not by researchers.

Mechanical Turks

The last decade has seen one additional reason reinforcing the trend. While behavioural studies are made more difficult, data based on “finger movements” have become increasingly easy to obtain, thanks to the availability of participants through online platforms like Amazon's Mechanical Turk. Many researchers, including myself, consider this a blessing. Not only are we less dependent on a pool of students, collections can be done almost overnight for a relatively modest cost, and although respondents cannot be regarded as a truly representative sample of the population, they are more mixed in terms of age and educational background than student participants working for credit points. By regular participation, they have achieved some skills in answering questionnaires in a professional way, uncontaminated by reactance and misunderstandings. Most research has shown that their responses are of good quality and can be relied upon (Paolacci & Chandler, 2014). However, like other non-native invasive species, they multiply in their new habitat, perhaps too fast.

To highlight the reliance of social psychological research on Mechanical Turk, let's take a second look at the individual studies listed in Doliński's Table 1 (Doliński, 2018, this issue). An inspection of participants described in the studies' methods sections, revealed that traditional student samples are on the decline. A rough count (by the present author) showed that students participated in only 22.5% of them, while MTurk workers took part in no less than 55.3% of all studies. The remaining empirical studies included a mix of participants recruited from other websites, people representing specific organizations and interest groups, archival data and so on. For comparison purposes, I also inspected the March and May issues of JPSP from 2006 that featured in the overview of Baumeister et al. (2007). It turned out that out of 88 empirical studies reported in these two issues, as many as 85 (96.6%) were based on student samples. This was before the advent of MTurk, so only one drew its participants from an internet site (of the remaining two, one studied couples and another the personality of orangutans).

Parallel to this development the number of individual studies per paper had increased. In 2006, only one of 22 papers contained more than five studies, whereas in 2016, 21 out of 46 empirical papers surpassed this number. Thus, a quiet revolution seems to have taken place in social psychology, with Mechanical Turkers replacing the indigenous species of undergraduate psychology students, like rabbits in Australia, increasing both the number of studies and the number of participants in each.

Behaviour in the Wild

Inspired by Doliński's plea for observations of real behaviour, and emboldened by Cialdini's (2009) call for breaking up, I decided to start my day in an unusual way, by simply observing people's activities in their natural habitat. What could be a better and less intrusive place than watching the behaviours of fellow passengers in the train? From my seat I had 10 unsuspecting individuals in view. What did they do, apart from sitting? Not much. One (myself) observed the others. Another slept. The remaining eight were bent over their mobile phones, watching a small screen and—much to my amazement—performing tiny but unmistakably real finger movements.

I admit that one low-powered convenience sample does not prove much, so a few days later, I decided to run a replication study. I had this time 12 passengers in front of me (I exclude myself this time to avoid duplicates). One slept, another tried to. A third leaned forward over an object obstructed from my view. The remaining nine were busy with their smartphones. Perhaps they participated in a JPSP experiment.

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