

## Response

## “All the King’s horses and all the King’s men . . . ”: What is broken should not always be put back together again

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## Introduction

Miller, Chikritzha, Droste, Pennay & Tomsen (2017) raise multiple methodological questions regarding our recently published study into the night-time entertainment districts (NEDs) of Queensland, Australia (Deville, Allen & Brown, 2017). They also raise wider ethical concerns regarding working with police and allowing research participants to enter their own data into questionnaires. As reported in an earlier issue of this journal (Deville et al., 2017), we conducted a point of entry study into NEDs which found: a high prevalence of preloading (79%); little meaningful difference between the genders in this prevalence and breath alcohol concentration (BAC); a much higher preponderance of people reporting to pre-drink for reasons of socialisation (rather than just price) compared to earlier research; and a large number of people to have no meaningful understanding of the BAC system. Here I respond to concerns raised by Miller and colleagues. First, I provide a context and common ground for this debate in the specific domain of Queensland. Next, I look at the sample we acquired in comparison to Miller (2013), explain motivations for preloading, and why our results are different, as well as more reliable than those reported by Miller. I further point

out why the lack of clarity around rejection rates is important and demonstrate that having police involved in such research is a good idea. I also address issues of misplaced concern in relation to the engagement of the energy drink industry and media, and the multiple charges of ethics violations implied in the commentary by Miller and colleagues. Taken together, I provide a corridor for future research, informed by the use of our scientifically reproducible methodology.

## Common ground

Our research team had been collecting our SmartStart data in Queensland since 2014. In 2016 the Queensland government introduced lockdown laws (since rescinded) and the earlier closing of pubs and clubs in Queensland to reduce alcohol fuelled violence. Miller vocally supported this proposal (Queensland Coalition For Action On Alcohol, 2016) and is currently acting as the government’s independent evaluator (Queensland Government tenders, 2016). I am, therefore, cognisant and appreciative of their great investment of time and energy into this issue, particularly in Queensland.

To address Miller and colleagues’ comments we must surely have a shared goal for paying attention to our research area. A primary focus of research into our area is surely the reduction of alcohol fuelled violence in the entertainment districts in which we are collecting data? With this in mind, I am a little surprised by the complete lack of focus on the finding that our research method

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seemed to be associated with fewer arrests and, by extension, lowered violence, in the entertainment districts in which we operated.

**Cutting nature at its joints: who & how do patrons enter NEDs?**

In assessing preloading to the entertainment district, we wished to find out how inebriated people were as they entered the NED. The NEDs (those designated as a ‘Safe Night Precinct’ in Queensland) have extended trading hours compared to pubs and clubs elsewhere. Miller and colleagues did not use this design, did not collect these data, and so cannot answer such questions.

In our research, conducted with police who have a vested interest in knowing how drunk or violent people are as they enter the NED, we collected data on everyone who had or hadn’t preloaded as they entered the NED. In other words, as we did not omit people we can always look at the different subsamples independently at a later date (as we did in the original paper and do again here below). In effect, we focussed only on those entering the NED, not those already in the NED or leaving the NED, as did Miller and colleagues in their prior research. This is an important distinction: Miller (2013) had collected data on preloading by asking a cross-sectional sample whether they had preloaded (i.e., consumed alcohol before “going out”) but had done so from patrons already in the NED. Underlining Miller and colleagues’ comment regarding the “importance of maintaining transparency in reporting the methods of analysis” we must admit to not being able to tell whether Miller’s (2013) mean BAC was of people who had preloaded and were now drinking in the NED or were from people who had only preloaded and were trying to gain entry into the NED. This is an important limitation of their research method in relation to preloading, as we have a wealth of evidence regarding memory errors in research participants asked to recall their reasons for a previous behaviour. For those interested in this area I recommend any books or articles by Daniel Schacter, Garry Wells or Elizabeth Loftus on this issue.

Unlike any past research with a large sample, and because we collected all these data *only* from people as they were entering the NED, we were later able to look at whether people who had drunk at a suburban pub were entering the NED any differently to people who had drunk in a home. This is a strength of our research and adds to our knowledge base, hopefully moving the field forwards. As outlined in the paper, and seemingly missed by Miller and his

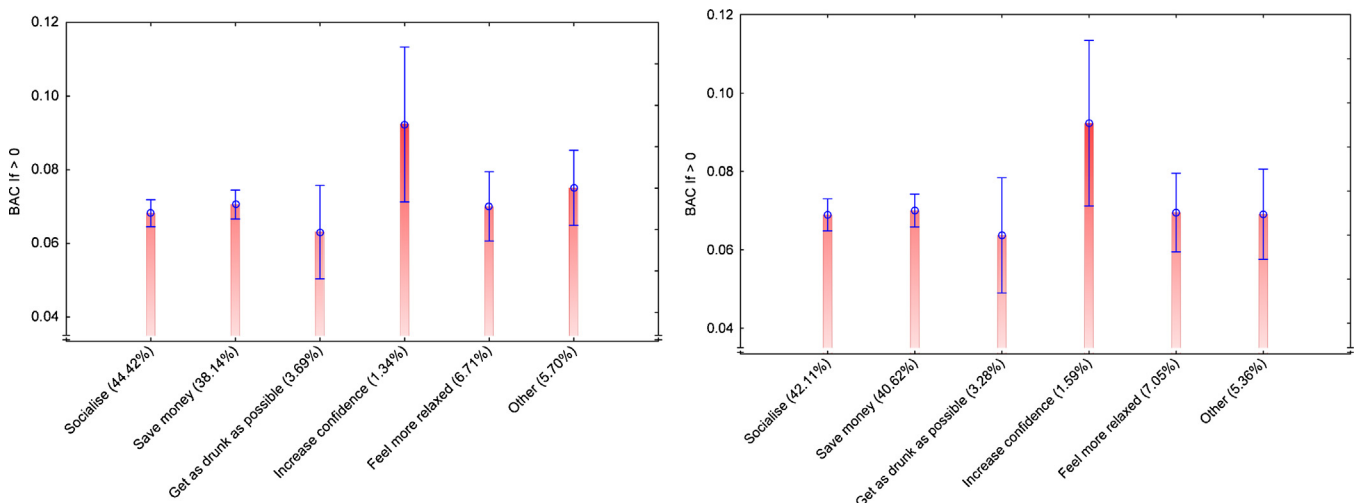
colleagues, we analysed these people (those who had drunk in suburban pubs) separately and found them to be similar to the rest of the sample (page 137 not only included a full section devoted to this but also a graph for those less inclined to rely on statistics). I am unsure why one would include preloading at a sports club (as did Miller, 2013, p.36) but not the suburban pub next door to the sports club? In effect, why is Miller measuring preloading? There seems, here, to be a lack of theory and utility by using a simple prevalence approach to research and this leaves the investigators to guess at effective interventions, if they do this at all.

But, for argument’s sake, let us look at this as Miller and colleagues would have it. If we were to remove from our Brisbane sample anyone who mentioned preloading in a suburban pub, hotel, motel, or hostel (all similar types of places), then of the remaining 1,884 participants, 78.4% of our sample said they had preloaded. Not a vast difference to the 79.72% who said ‘yes’ to preloading in our original data, but very different to a pre-drinking rate of 60% which Miller and colleagues mistakenly believe we would get. However, I believe that where Miller and colleagues are making their mistake is by including these 281 people as a “No” to preloading. Of course, if one were to see these people as having already been drinking ‘in town’ or ‘out there’ or ‘having already entered the night time economy’ (as Miller and colleagues have said we should) then they would not have met inclusion criteria into our study of people entering the NED. That they would have been included in Miller’s previous and current studies and classified as a ‘No’ for preloading demonstrates why we may be concerned regarding those studies.

**Pre-loading motivations: to socialise or to save money?**

Miller and colleagues also point to the inclusion of these people (suburban pub drinkers) in our sample as to why we had so many people (and vastly different to their research) selecting “to socialise” as their main reason for preloading. So, to cut along the dotted line that has been drawn in their commentary we reanalysed our data, both including and excluding these people—it made no difference.

In our original paper we had 44.57% reporting to preload in order ‘to socialise’ and 37.43% preloading ‘to save money’. Removing anyone who drank at a suburban pub, motel, hotel or hostel means that we now have 42.55% preloading ‘to socialise’ and 39.95% preloading ‘to save money’. I am sure Miller and colleagues



**Fig. 1.** Comparison of original (Devilly et al., 2017; left) and new (right) graphs for motivations for preloading, if BAC greater than zero—with and without data from people preloading in suburban pubs.

now see that their reason makes no difference to the interpretation of the data and can, hopefully, understand why we bring this very substantial difference into the open for further exploration by other researchers.

To be perfectly clear on this point, Fig. 1 above compares the BAC of those registering above zero by preloading motivation—from the original paper and with these people removed as requested by Miller and colleagues. As can be seen, these graphs are, for all intents and purposes, identical.

It is important that this cultural shift is appreciated by researchers using the old methodologies and government law makers relying upon them. That said, I tentatively offer one other explanation for our different results. Our sample, as outlined above, was acquired to see how people were entering the NED. We have other studies looking at other samples in submission at journals, but this paper even had in the title “Point Of Entry Study”. Miller’s (2013) study seems to be less specific and was cross-sectional in nature. However, the problem with looking at reasons for preloading in people who are already drinking in the NED is that their reasons will change once they start paying the high prices in the NED. To place this into some perspective, the price of a schooner of beer in a local suburban pub (the one closest to our University) is \$5.80, but the price for that same glass of beer in one of the major night clubs in the Brisbane CBD and Fortitude Valley is \$12. A glass of wine in the suburbs costs \$7, but is \$14 in some NED clubs—this surely conforms to the “to save money” answer, yet socialisation is still exceptionally important as a primary answer. This hypothesis, as to why socialisation was missed by Miller (in both 2013 & now in 2017), is currently being investigated experimentally. This is an important omission of past research because Miller (e.g., Australian Broadcasting Corporation, 2013; Miller, 2015) promotes a unitary price of alcohol and has advocated systematically increasing the price of alcohol at outlets to reduce consumption (similar to the Australian approach to tobacco products). But no matter which approach we take, we need to surely base our decisions on data that accurately captures the full picture and not just from the snapshot taken by one research group.

It should be noted that Pedersen (2016) very nicely commented on this very point: he notes a vast amount of prevalence work that has been done in the preloading area yet calls attention to the lack of intervention options that have been presented and a general lack of advancement in the field. I believe Pedersen is correct and we need greater diversity and researchers who are sensitive to the cultural nuances that separate different locations, and where the researchers use more sophisticated methodology aimed at testing hypotheses derived from, and informing, theory.

Miller and colleagues also claim that our BAC levels for preloaders should include only those who identify themselves as preloaders and that the reported BAC should be the mean BAC—including people who scored zero at entry to the NED. The problem, though, is that governments and police want to know the state of people as they enter these districts. If Miller and colleagues are happier remaining with the more traditional way of doing things then they need only look at our original Table 1, which also listed the BAC of people who defined themselves as having preloaded (bottom of page 132). All the data are there and I am perplexed by the exception that is taken.

Relatedly, Miller and colleagues would like us to report these means as the major outcome variable in our analyses and conduct our analyses with this mean (made from zero and non-zero readings) as he has done in the past. I present here (Fig. 2) the distribution of BAC readings for our entire sample. I would expect Miller’s data to be very similar, and would be amazed were it not so. I leave it to the readers of this journal to decide for themselves whether this represents one naturally occurring sample from

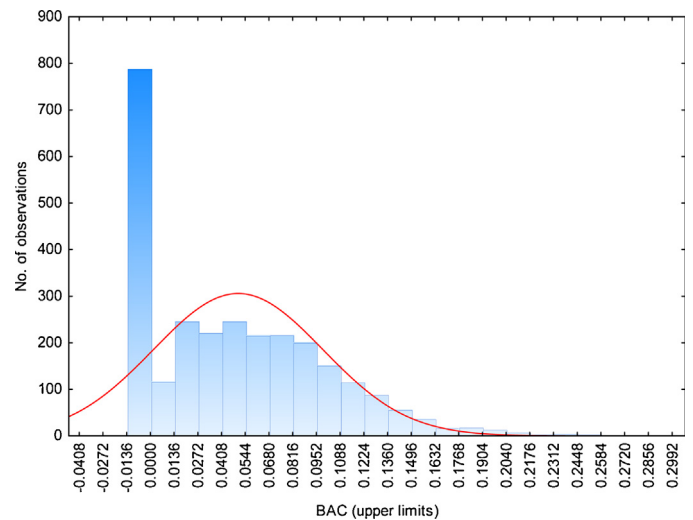


Fig. 2. Breath Alcohol Distribution For Total Sample.

which we can test and make inferences about the world around us, or whether this is the representation of two samples. I am unclear why one would want to mix them, except to downplay the role of preloading? Even if one were in favour of doing this, the data now fail the normality assumption underlying parametric analyses. Relatedly, a participant having a BAC of zero did not lead to the exclusion of the participant, as suggested by Miller and colleagues. Rather, we made it abundantly clear that such participants need to be analysed and seen differently.

### The ‘Rejection rates’ of Miller (2013)

Miller and colleagues are correct in that they reported “a response rate of 93%”. However, this does not let us know about the rejection rate of preloaders (this is in the opposite direction of a response rate but tells us a little more). Miller and colleagues keep missing that our study is a “point of entry” study and is looking at people entering the NED. Pointing to a number of subsequent rehashings of their study in dismissing ours is just a misguided appeal to authority.

Their 93% response rate was not: operationally defined; broken down by geographical location (they were in multiple cities); or broken down by source of rejection (i.e., entry to the NED, leaving nightclubs, etc). It is also noted that the researchers devised their own method of deciding whether people were intoxicated (apparently they can tell how intoxicated people are by looking at them) and so it is unclear whether looking at the refusers in the crowd (observational data do not need participant approval) were included in this novel approach to intoxication estimation.

### Police in research and further sampling issues

An issue also raised by Miller and colleagues is the use of police in data acquisition. Rather than focussing on the exciting possibility that this form of research could actually lead to a reduction in violence in the NED (as outlined in our paper and currently being further investigated by my research team), they prefer to raise the remarkable suggestion that it may be unethical to have police being involved in this research. I would argue quite the opposite. In our data acquisition we have found that police actually act as an attraction to many participants, and it is concerning that there seems to be the implication that inebriated

people were harangued into participation by the police. This research went through full ethical consideration and I can only assume that the authors are questioning how to ethically obtain consent from inebriated people as we have done. I am happy to educate them on this matter but, due to lack of space here, will happily forward to them a description personally.

The presence of the police was initially to overcome the problem that Miller et al. (2013) very clearly defined in their study protocol: “Interviewers will be trained in the identification of intoxicated persons and patron interviews will not be conducted with people who are heavily intoxicated. Where intoxication is not evident until the interview has begun, the interviewer will end the survey prematurely, thanking the participant for their time and informing them they have answered all the questions. . . . All prematurely ended surveys will be recorded as such, and all refusals will be recorded, so that response rate is captured” (p.72, Miller et al., 2013). That Miller and colleagues now state that only 12 of 7028 people met this criteria can mean only one of three things:

1. These data were not collected in areas that resemble anything like a major night-time entertainment district in Queensland;
2. People meeting this criteria were systematically ignored and not counted in data acquisition or rejection rates;
3. The study protocol, as published, was not followed.

Even now redefining a past study protocol as “too intoxicated to give consent” and referencing a 2017 study (one published after I brought this to the first author’s attention in 2016) does not obviate this inconsistency. And this is why the more researchers we have working in this area the better. In fact, I would go even further regarding our police presence and recommend other researchers utilise this approach—at least initially when gauging the environment being assessed. Having a university and police grouping meant that data were always shared as they were collected, we benefited from a synergy that led to ideas being put into our protocol from more than one domain, and meant that there was a degree of mutual oversight—a process which gives us, and others, greater confidence in our final results.

But there is indeed an empirical question here that is interesting. Does having police present during such research change the results? While Miller and colleagues state that they do not have data on this – we do. After our SmartStart study was completed, we conducted a Last Drinks study – looking at the end of the night. However, we also collected data at the beginning of the night and, due to the success of the SmartStart study, we gained ethical clearance to conduct the research without a police escort. Matching for the same months, and careful to only use data from before the change in legislation in Queensland, we have: SmartStart data with a police escort from October 2014 to February 2015 ( $n = 1990$ ); and a smaller Last Drinks preloading data set from October 2015 to February 2016 ( $n = 427$ ). There was no difference in BAC between conditions: when looking at those with a BAC over zero ( $F(1,1734) = 0.075, p = 0.78$ ); in the number blowing a BAC of zero (Police Presence = 28.74%, No Police Presence = 25.53%;  $p = 0.18$ ); and when violating statistical assumptions and using Miller’s method of looking at the mean of everyone ( $F(1,2415) = 0.43, p = 0.51$ ), or looking at the mean of everyone using the correct non-parametric analyses ( $U = 412094, p = 0.33$ , Police presence  $\bar{x}$  BAC = 0.05,  $sd = 0.049$ , No Police Presence  $\bar{x} = 0.052, sd = 0.048$ ).

This is an important finding considering the ethical charge by Miller and colleagues and presents data, as demonstrated in Fig. 3, which are completely at odds with their theory (that “people who are more compliant, less intoxicated, and probably overall less risky drinkers would be likely to engage with this

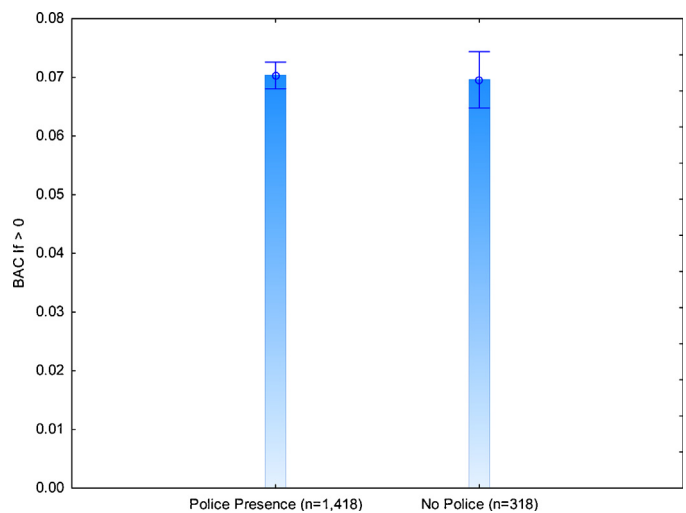


Fig. 3. BAC Of People Registering A BAC Over Zero, With And Without Police Presence.

research method than methods not involving police”). As such I am surprised by such a bold, unexpected and wrong suggestion – that police should be removed from this research domain on ethical grounds.

### Energy drinks & the independence of researchers

Miller and colleagues also draw exception to our trying to find some possible explanation for why Miller (2013) reports such diverse results to us in energy drink usage. We suggested various reasons why we may have arrived at different results and gave Miller the benefit of the doubt by suggesting cola may have been included or the data came from retrospective reports (on p.131, Devilly et al., 2017). We gave these possibilities because our results show significantly smaller energy drink usage than in Miller’s study. We found that only 9.84% of our sample, and 11.65% of preloaders, mixed energy drinks. This is a remarkable difference to their past research and, if Miller and colleagues found their results without including cola drinks, then we are left with limited options for their results: retrospective reporting; sample bias; or methodological phenomena (such as leading questions—see below).

Miller and colleagues also state that the authors we cited are Red Bull funded researchers and that we should have cited McKetin, Coen & Kaye (2015) and Peacock, Pennay, Droste, Bruno, & Lubman (2014) instead. There is an implication here that there is a larger, clandestine, issue. So, let me be clear: none of the SmartStart authors (Grant Devilly, Corey Allen or Kathleen Brown) have ever received any monetary or material backing from any energy drink company and, until the SmartStart study, none of us had ever even looked at the topic of energy drinks. That said, we are now finding very different results to Miller and are currently submitting a manuscript on this topic elsewhere. Having not nailed our flag to any mast we feel that we can be seen as quite independent on the topic and believe that readers will be convinced by our data. However, that said, and having read Miller’s view on this topic and the dismissal of research and researchers on ethical grounds (again), I am disturbed by this emerging pattern of attacking the man rather than the data.

I cannot speak for Verster and colleagues, but I can explain why we didn’t cite McKetin et al (2015): this review included no data analysis. It is a description of energy drink studies and categorises



them as independent or industry supported using their specific method. The main outcome of this review is that 2 studies with industry links found that people who have energy drinks imbibe more alcohol, while 4 studies without industry links found this same result. We preferred to cite a meta-analysis.

However, what I am finding difficult to explain is why Miller and colleagues raise this, because it creates more questions than it answers. The McKetin et al review and the Verster et al meta-analysis (which we did cite) *both* agree with our results: Those who drink energy drinks imbibe more alcohol than those who don't (although a more interesting question is "why?").

Further, Peacock et al. (2014) was classified in the McKetin review as having an industry relationship, and so I find it strange that Miller, Chikritzha, Droste, Pennay & Tomsen (this issue) would refer to Peacock et al. (2014) as independent and wish us to cite this work but not Verster, Benson, Johnson, Scholey, & Alford, 2016. As a hitherto uninvolved researcher, this seems to again be a turf war disparaging the competition by attacking the researcher and not the data. In our SmartStart paper we cited two articles (not one, as claimed) in reference to energy drinks: Miller (2013) and Verster et al. (2016). That our results disagree with Miller on prevalence is unrelated to our agreement with Verster et al on inebriation levels of those imbibing energy drinks. It is most perplexing why Miller raises this issue.

This idea of dismissing researchers on funding or 'ethical' grounds is a double edged sword. For example, I would feel as uncomfortable completely dismissing a research team which has received funding at some point from the alcohol or energy drink industry just as much as I would feel uncomfortable dismissing all of Miller and colleagues' research because they have received funding from FARE Australia (a non-government temperance society). Personally, I would rather deal with the data generated and not focus on the people involved.

### Bouncing freedom of speech

Rather bizarrely, Miller and colleagues claim here that we released our results to the media, pre-review, and in support of this cite Edwards (2014)—a TV news story screened 6 months before we completed data collection. This issue of speaking with media at all has been raised in a recent letter to the editor at another journal (Devilly, 2017). Our field study was from 21st August 2014 until 27th February 2015, as outlined in our paper. Ms Alyse Edwards is an Australian Broadcasting Corporation journalist who came out with us on our *first week* of data collection. They knew about this research because it had been announced by the Queensland Police as a joint research initiative: in Queensland the Police quite rightly see community engagement as a good idea and explain their use of tax payers' money. Such openness should be applauded—no secrets, more than one invested group having access to all the data as they are collected, and no political masters to please. However, I am confused by the exception with media engagement, with Miller recently discussing his ongoing research in the Queensland press, during the current lead-in to the State elections (Brisbane Times, 2017). Either way, we are in the fortunate position of being researchers who do not have any non-disclosure arrangements with any government or funding body regarding our current research. Therefore, we can speak to whom we please without prior approval or vetting of the words used.

### Replicating and extending from SmartStart

We need more, not fewer, research groups investigating this area. Besides the need to assess geographical nuances in the

population before a one-size-fits-all approach to intervention is made, we also need to have data that can be meta-analysed across research groups and not just within one main research group. To facilitate this I have made resources for researchers available at our website <http://www.smartstartqld.com.au>. I would be happy to further help any research group initiate studies.

I would encourage that future research in this area have some theoretical reason for collecting the data that is collected. In our study we wished to gauge how drunk people were as they entered the NEDs in Queensland and what had contributed to their state. From this we have since developed a theory of why people preload to the extent that they do (still in preparation for a publication) and likely interventions that may reduce this behaviour. Relatedly, we suggest others adapt their methodology to test their theories or provide answers that are meaningful for their specific context.

I would further encourage collaborations between universities and police departments. Purposefully avoiding such knowledge capital when initiating a research team can be naïve and, under some circumstances, counterproductive or suspicious. This is also a two-way street as the police engage with the community on a positive level, raising the profile of the police. It should not be forgotten through the fog of argument and accusation that our study also suggested that conducting such research may have diversionary properties and actually reduce alcohol-fuelled violence.

### Conclusion: is our research comparable to Miller's?

There are tantalising glimpses of where and why our and Miller's research may be getting different results, and these have been discussed above. But there seems to be a fundamental difference yet unaddressed.

As mentioned above, Miller and colleagues refer to their interviewers as providing "further clarification where needed" to participants and, quite alarmingly, state towards the end of their commentary, that participants should not be allowed to answer questions themselves directly onto the iPads. Even though Miller et al. (2013) stated that "there was no way to ensure participants were telling the truth" in their original study (p. 24), they argue that interviewers should enter the data instead of allowing people to enter their own data. They then state that they conduct "fact-checking during the interview" before entering the data. There is a wealth of research on the inherent dangers of shaping performance by expectations from the researchers. Having published on this topic in relation to clinical trials (Devilly, 2001), I am quite shocked by this casual admission. For example, as far back as 1964 Rosenthal and Lawson demonstrated that bias affected the rated outcome of experiments where the investigator had a pre-conceived notion of the expected outcome. This 'Rosenthal effect' has also been shown to effect a chief investigator's assistants even when the chief investigator does not verbally communicate the nature and direction of the bias (Rosenthal, Persinger, Kline, and Mulry, 1963). I would argue that having "a trained field researcher enter and check responses" (as stated by Miller and colleagues) could well initiate the finessing of data into a preconceived direction. As outlined in our SmartStart paper (notes on Table 1 and in text descriptions, p. 133–134), and as usual for just about every other quantitative research field in the human sciences, we statistically controlled for purposeful and errant data entries and have much more faith in our presented data. Therefore, and in consideration of the demonstrated methodological idiosyncrasies of Miller and colleagues' approach, I think that future research may benefit from using our SmartStart study as the baseline to compare their results into NED preloading.

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