

Open Letter to the Editors
Energy Policy

November 24, 2017

Dear editors,

We write to respond to the criticisms raised by Professor Shipworth and his colleagues Elam and Dr. Fell (summarised as SEF) in their letter to you. Joseph Joubert is attributed to have once said that “it is better to debate a question without settling it than to settle a question without debating it.” In that light, we welcome critical discussion about both the Smart Meter Implementation Programme (SMIP) in Great Britain and our own article “Vulnerability and resistance in the United Kingdom’s smart meter transition.” We also agree with SEF when they write “that issues of critical infrastructure system scrutiny, environmental impact, protection of vulnerable consumers, and consumer engagement with the SMIP are important, and that this article brings together a number of issues which it will be important for those leading the SMIP to continue to consider.”

We disagree strongly, however, that the paper “contradicts *Energy Policy*’s stated aims and scope” or that it has “factual errors, methodological issues, and issues of academic impartiality and balance.” Most importantly, the overall conclusions to the article stand.

We also find it strange that SEF refer so strongly to standard editorial and research publishing conventions but then contravene these to post a rebuttal not through the journal, where it would have been subject to editorial and peer review, but publicly. We worry that such a double standard is unfair, and does not seem like appropriate academic conduct.

Here, we focus on offering responses to the three points raised in their letter: factual errors, methodological issues, and issues of academic impartiality and balance. In our response, we also point out where we find factual errors and incorrect assumptions in their own letter.

Factual errors

SEF write that “The paper repeatedly cites incorrect estimates of the savings ... of 5-15%.” This is neither incorrect nor an isolated instance—we note four studies that all reference this point, one of which we explicitly referenced in our article.¹ As we clearly write in p. 770:

The UK Office for Gas and Electricity Markets (Ofgem, 2004: 2) projected that smart meters would deliver “sustained energy savings of 5–10% for many customers through the use of even a limited number of simple improvements.” Other studies suggested savings as high as 15% of consumption ...

Our source here was a 2004 Ofgem report that predates your sources. This was one of the *initial* projections made justifying the programme and it was not the only one. When SEF write that

¹ Their support for this claim is the Smart Meter Impact Assessment 2014 and then the 2016 Impact Assessment. While we do not disagree with these two sources, it is critical to note that we never claimed the 5-15% range came from them.

“this figure is not the basis of the UK SMIP Impact Assessment,” their claim is factually inaccurate, because there have been more than one Impact Assessment, and those Impact Assessments have changed over time. While the estimates have been updated later, it does not make our references to the fact that SMIP was (initially) based on these estimates incorrect. We can point to three other sources between 2007 and 2010 furthering the notion that smart meters would have savings in the range we indicate, one of them an Impact Assessment from DECC/Ofgem:

Mott McDonald, 2007 Appraisal of Costs & Benefits of Smart Meter Roll Out Options, p. xiv:

There is much greater uncertainty on the benefits side, especially for energy savings. Here there is an issue of the size of the energy saving and associated peak load reduction as well as the value to be placed on these reductions. The relevant literature talks of savings of as much as 15%.

BERR 2008, p. 21:

There is a great deal of uncertainty about the likely response of consumers to the full roll out of smart meters. Although a number of international studies exist (summarised by Sarah Darby), sometimes showing dramatic behavioural changes (over 10%), it is usually very hard to transfer the findings to the UK situation (e.g. mandatory take-up expected here, very little use of air conditioning, different counterfactual world, different cultures, different price levels).

DECC/Ofgem Impact Assessment, 2010, GB-wide smart meter roll out for the domestic sector, p. 27.

A number of international studies exist, the most recent a review of 57 feedback studies in nine different countries by the American Council for an Energy-Efficient Economy which finds that on average feedback reduces energy consumption between 4-12%. Sarah Darby and Corinna Fischer also show that feedback can result in dramatic behavioural changes (average reductions in energy consumption of over 10%).

It is therefore entirely reasonable to say that early projections of the SMIP sold the programme on the grounds that it would save energy by 5-15%. We also note in the last quote the somewhat hyperbolic language about the potential for “dramatic behavioural changes.”

We acknowledge the point about fixed broadband services. We agree and, thus, we addressed this with a Corrigendum we submitted to *Energy Policy* in October, published on the 21st of November. It indicates:

On p. 776, the phrase “fixed broadband services” should be replaced with “dedicated network connection.” This is because smart meters do not require access to traditional broadband - the DCC is the national communications system for smart meters in the United Kingdom (Data Communications Company 2016). This new piece of national infrastructure came online in November 2016. All smart meters communicate through this secure network.

Nonetheless, beyond the use of the term “broadband,” the claims about limited coverage still apply. Brignall (2015) notes that network coverage in rural areas is about the same as broadband coverage, writing that: “MPs on the energy and climate change committee ... admit we had not considered the meters’ reliance on the mobile network, which in some areas of the UK, is almost non-existent.” The DCC itself (2016) also notes that when launched, it could only guarantee coverage to “at least 80% of Great Britain’s premises” and that coverage will not be universal by 2020.

As we emphasize in our corrigendum, “neither of these corrections alter the findings or conclusions of the study.”

SEF write that “consumers do not have to purchase smart meters. The SMIP requires that smart meters are offered at no additional cost to the consumer.” Again, this does not appreciate the fairly complex history of the SMIP: it was not always voluntary. On p. 772, we emphasize how the mandatory dimension to the SMIP was not changed until 2012:

During the foundation stage in 2012, Charles Hendry, the Minister of State for DECC, stated that the smart meter rollout would no longer be mandatory for all homes; homes could opt-out, they would not be obligatory and people were not required to have one.

We also emphasize that since the nature of our methodology was a 10-year review, it covers a period (2008 to 2012) when the SMIP was mandatory and, thus, such language was often adopted by the studies examined.

SEF write that “A balanced assessment would also note that incompatibility between suppliers does not apply to any SMETS2 compliant meters,” but, again, our study was looking at the body of evidence from 2008 to 2017, when incompatibility issues were evident. Changes post-2018 are not within its remit. We would also note that SMETS2 could be delayed beyond 2018.

SEF write that “the issue of smart meter functionality ceasing on switching will apply to a minority of early issue smart meters, in which case they will retain all the functionality of a conventional meter.” Again, this itself is misleading: we understand, according to comments from the “Smart Meters Bill 2017 Second Reading Debate,” that “by December 2016 some 330,000 smart meters were operating in dumb mode—not operating as smart meters—and that by March 2017, that figure had risen to 460,000, involving a cost of between £30 million and £50 million.” A major contributor to this difficulty was supplier switching. While it may be a “minority” of users, it is still a substantial number of meters and wasted money.

SEF write that “Viitanen et al (2016) ... compares smartphone energy feedback to TV-based feedback.” Firstly, we cite Viitanen et al (2015), not (2016), so SEF may be reading the wrong article. Secondly, this TV-based feedback was a type of in home display, a “customer interface” or “eco-feedback system,” which Viitanen et al (2015) note themselves, see p. 20 (where they mention “data displays”) and p. 21 (“displays based on real-time data”) and p. 22 (“if there is promise in using displays to support energy-efficient behaviors, there are barriers too”).

SEF write that “The conclusions regarding the lifecycle impacts of smart metering are therefore misleading and directly contradict the findings of the citation provided.” Again, another factual mistake on behalf of SEF: we carefully cite Louis et al. (2015) and explicitly note that this paper included not only a smart meter but an entire home energy system. Please see p. 776:

Louis et al. 2015 conducted a lifecycle assessment of an entire home energy management system, including a smart meter, home automation, and IHD. They concluded it had a negative energy payback ratio of 1.6 years—the system as a whole was a net consumer, rather than saver, of energy.

SEF affirm our interpretation of Louis et al. when they write that “the environmental ‘investment’ in terms of home automation does not pay itself back.” We never say that smart meters by themselves are a net energy loss, we only call on more careful research in the future (in the Conclusion). As we note there, on p. 780:

More refined lifecycle assessments should be conducted to determine the range and sensitivities of energy payback ratios and energy return on investment for different smart meter configurations in the UK.

Moreover, we would maintain that some smart meter configurations *can* be net energy losses when integrated into some home automation or energy management schemes.

SEF write lastly that “the article notes that British Gas was fined £4.5m ‘for their slow rollout’ but that this focuses on advanced metering to business customers.” We don’t disagree—we are mentioning problems in this paragraph across the board with smart/advanced meters and suppliers, not only those specific to the SMIP. We also do this elsewhere in the manuscript, noting concerns with the SMIP and then studies that have affirmed similar results, e.g. p. 776 (study about Finland) or Table 5 on p. 778 (data from the UK plus Ireland, Netherlands).

Methodological issues

As a starting point to this part of the discussion, we would note that in the energy studies field (as well as the energy social science field), the nature of what is considered to be an “appropriate method” is divergent rather than convergent. SEF perhaps unintentionally convey the idea that there is a single, absolute, objective way to undertake a review, and that there is a single code of conduct or practice. In reality, the selection and execution of methods is diverse—a diversity to approaches (and ways of conducting research) that we celebrate and want to continually open up, rather than close down.

Put another way, there are multiple ways to design—and execute—a systematic review. These range from qualitative/narrative approaches to more quantitative ones or even formal meta-analysis or content analysis (which we never claim to do). Systematic reviews can be a sole method used in isolation, or a method mixed with others (meaning it can be a primary method, or a secondary method, etc.). We used a systematic search combined with qualitative review of the contents, as a mixed method, coded by one member of the team, and then supplemented with a search of the popular literature. Although our approach appears not to meet the standards of what SEF consider a systematic review, we maintain that it is a valid approach (as is the approach taken by SEF)—just different, not deficient. We would also note that our type of

systematic review has been used in many other academic articles and moreover, that it was subject to thorough editorial and peer review within *Energy Policy*.

SEF write that “As a systematic review, the study is not replicable, as it does not provide sufficient details of elements of the search strategy.” Although there are differing interpretations of what constitutes a systematic review or replicability, we would note that SEF *did* replicate a part of our study (the search strings) and reached “similar results.” It is precisely the fact that our study clearly states and executes a method that enables SEF to test it – we have an explicit, transparent research design, which many studies in the energy social science field do not.

SEF write that “We replicated the stated search strategy using the same keyword combinations, search fields, and date ranges on the ScienceDirect database and returned similar numbers of results.” To be clear here: this statement is not entirely accurate. SEF did not replicate our methodology *exactly*. SEF searched for articles after 2008 and before 2017; we searched for articles published from March 2008 to March 2017. (This likely explains why for ScienceDirect, SEF returned 14 results whereas we returned 15 results). SEF also only searched one database; we searched five.

SEF write “analysis of the results returned showed that only half of these references, 7 of 14, appeared in the list of references in the article.” Yes. We did not cite all of the 47 articles collected in our review—only those most relevant to our research questions. It is typical or at least common for some systematic reviews not to explicitly cite all the sources, but to summarize them and cite only core ones, especially to keep the length of the articles (and the length of reference lists) manageable.

SEF write “No explanation is given as to how the subset reviewed for this article were selected.” This is incorrect—on p. 768, we write that “All of the resulting 47 articles were analyzed, and assessed both for topical coverage (what challenges facing the SMIP did they identify, what socio-technical barriers did they discern, if any?) as well as lacunae (what gaps within the literature existed?).”

SEF write “no search or selection strategy is provided for the non-academic sources that are referenced throughout the article.” Here we admit as such—but we maintain it is an accepted practice to use non-systematic searches of the popular literature to supplement systematic reviews of academic literature. This is in fact what many, if not most, academic energy social science studies do to ground the debate. We also want to note that the majority of citations are *not* to popular media—only 12/79.

SEF write that we neglected to control for “inter-coder bias by blind double-coding of sources to avoid conscious or unconscious bias in coding the textual data for analysis.” We maintain this is not always necessary – moreover, there is no need for an intercoder reliability test when only one author read/coded all of the 47 articles individually (there was no “intercoder team”).

When discussing our method of events, SEF write that “While some event selection criteria are provided, it is not clear whether these were all the events which fit these criteria, or a convenience sample meeting those criteria.” Although this was not suggested by the peer reviewers, we admit that more detail about event selection would have been useful in the

published article. During the period September 2015 – November 2016, we participated in all of the smart metering events in the Greater London area that we could identify based on regular searches in the internet. Thus, the only sampling strategy was event availability (or in other terms, there was no sampling – all events we identified were attended). To analyze the events we used both our notes and public transcripts of the events, making sure that any references to the event data made in the article could be verified through the public transcripts (and indeed it can also be done by others when reading the transcripts). Two authors of the team have both read the event material.

Finally, and critically, SEF seem to be holding the paper to excessively high standard which a great many of published studies would not meet.

Impartiality and balance

SEF write that “The article states that “Bradley et al. (2016) noted that another level of resistance relates to the devices being potentially managed by the smart meters” (p.777). While we accept that Bradley et al. (2016) noted this, it is factually incorrect ...” We suggest you take this up with Bradley et al.

SEF write that “The article states that “Citizens Advice (2017) echoed similar concerns ...” and that “The Citizens Advice report does offer recommendations to improve safeguards ...” We don’t disagree with either point, and are confused over SEF’s main argument. If it was that we should have mentioned the Early Learning Project, we do so in Table 5 and in our reference list. If it is that we should confirm Citizens Advice offers recommendations, we indeed applaud their efforts in a later part of the manuscript. See p. 779, where we write: “Recent partnerships between the Citizens Advice and Smart Energy GB ... are an encouraging sign that such concerns are beginning to be addressed.” If it is that we should have cited the Citizens Advice report more, we were already dealing with a manuscript close to the journal’s limits for length and were cognizant of trying to keep the article as short as possible.

SEF write that “The comparative weighting given to different references is unbalanced and does not reflect the comparative authority of the different sources.” We would remind them that having 12 references to popular sources out of 79 is by no means unusual, and also that citation frequency does not necessarily correlate with citation strength. We also note the conflict with this comment and the one that follows, that we should always cite material with acknowledgment.

SEF write that “The article cites out of date material which is used without acknowledgement ...” Apart from being unclear (which material(s) is/are used without references?), we would like to remind SEF that our review did cover older sources, which we state clearly when we say our timeline went back a decade. Furthermore, many of the issues mentioned in Rose and Thed (2014) related to SMETS1 are still occurring—all the more so given that SMETS1 meters may continue to be rolled out into 2018. We finally note that minimum DCC network coverage was still limited in 2017 to “at least 80%.”

SEF write that the article “does not reference” some research. Correct—we cannot be expected to reference everything.

SEF write that “quotes are unattributed and unverifiable,” referring to the statement made on p. 744. Firstly, this is a single “quote” (singular) rather than “quotes” (multiple statements). Secondly, it was cross checked and verified with that person for accuracy. Third, it is a common (and accepted) practice to guarantee anonymity or confidentiality to protect respondents from potential retaliation.

SEF write that we use “emotive terms” and are “unbalanced” in our appraisal. We have multiple responses: first and foremost, as our research questions for the study actively (and only) focus on barriers, of course the article focuses more substantively on obstacles and constructive criticism rather than praise. As we note on p. 779, “an appreciation of challenges and failures ... serves as an antidote to recent studies framing the smart meter transition in Europe as an “imaginary” full of cooperation, hope, democracy, and sanguinity.”

In addition, what SEF call “emotive terms” we see as strong elements of style and “good writing.” We are careful to caveat many of these statements for nuance, also: we didn’t say the SMIP was “thoughtless,” only that it could appear that way to those attending the events; we don’t say “hubristic” in an absolute sense, we say it may be an “almost hubristic agenda.”

Moreover, many of these specific choices in language resulted from the peer review comments, or from the comments received from colleagues at the Department for Business, Energy and Industrial Strategy and the Environmental Change Institute at the University of Oxford, as well as our Advisory Board (consisting of members of groups such as Ofgem, Carbon Trust, Green Alliance, and others).

Most importantly, we try to use such language in a balanced way – we use similarly (emotive) language when discussing the positive attributes of the SMIP in various places, including a (novel) Table of 67 benefits. Here are a few other examples:

- “A switch to smart meters offers the potential to capture numerous sustainability benefits” (p. 770)
- “numerous other possible benefits” exist which can be “distributed across consumers, utilities and society as a whole” (p. 770)
- the SMIP offers “an opportunity to transform transactional and largely negative billing interactions with customers into valued exchanges which deliver satisfaction all round, via reduced costs, improved transparency, and empowerment” (p. 770, a direct quote from another source)
- “It could also facilitate new business models that incorporate electric vehicles, heat pumps, and other storage devices in a “vehicle-to-home” or “vehicle-to-grid” configuration (p. 771)
- “Admittedly, resistance and ambivalence do not always or even frequently occur. A commercial survey in 2017 of more than 1000 consumers in the UK suggested that 64% of those with meters in place were “enjoying better visibility of their energy costs, 36% said they had achieved savings and 76% said they were impressed with the technical and service expertise of the individuals who completed the installation” (p. 777)
- “SMIP should not be condemned before considering the counterfactual in which smart meters were rolled out with no thought at all for engagement” (p. 779)

- “The earlier phases of the SMIP also deserve credit for putting an unusual amount of effort and attention into trials coupled with a genuine commitment and willingness to learn on the part of both government and industry” (p. 779)
- “UK planners should be lauded for their visionary, ambitious attempt to decarbonize homes and buildings with the world’s largest smart meter program with the aid of consumers” (p. 780)

This last point in particular is emotively positive. (We would also underscore the subjective nature of this discussion).

Lastly, on the topic of bias and impartiality, we note SEFs declaration of interests in advising on and promoting the UK’s smart meter programme, some of which they state goes back ten years. We thank them for this, and leave others to judge its relevance to SEFs Open Letter. The authors of the paper complained of have no such interests.

Conclusion

To conclude, we summarize some factual mistakes or incorrect assumptions SEF make in their letter to us. SEF presumed our reference to 5-15% energy savings was incorrect (it is not). SEF refer to Viitanen et al. 2016 whereas our study drew from Viitanen et al. 2015. SEF improperly and incompletely replicated our research design. SEF accused us of having multiple unattributed quotations when in fact it was only one, which is justifiably anonymous. SEF fail to acknowledge the diversity of approaches to both conducting a systematic review and writing an academic paper. (SEF’s criteria seem to relate more to quantitative meta-analysis or full content analysis than a qualitative systematic review). It is dogmatic and wrong for SEF to presume there is only a single way to write an article, or interpret results. Indeed, the ability to interpret, discuss and critique in any direction is core driving force of academic scholarship in itself. SEF also improperly confuse errors of fact with differences in interpretation.

That said, our study is not entirely free of error—we proactively submitted a Corrigendum correcting two mistakes in October 2017. We welcome the substance of SEF’s critique that we could have been clearer about the coding process; that we could have referenced all 47 studies collected in the review; that we could have mentioned the non-systematic nature of our review of the popular media literature, and offered more detail on event data analysis. Yet, the lack of this information does not make this study inherently flawed.

Furthermore, it is typical for journal reviews to ask more detail on methods where needed, and in this case, the reviewers were satisfied with the level of detail provided. The information provided reflects choices in the research design and emphasis of detail wanted by the reviewers, rather than any shortcoming. Alongside the clarifications mentioned in this letter, we will enthusiastically engage with those looking to build upon or replicate our study.

Ultimately, these differences do not in any way rise to the level of “materially changing the conclusions of the paper,” nor do they violate any of the stated criteria for publication in *Energy Policy*.

Sincerely,

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Disclosure of interests

We know of no conflict of interest that would affect the impartiality or independence of our research.