Remarks on the rebound effect, energy efficiency, and research needs

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I was asked by the workshop organizers, and agreed, to address the following questions:

There are many research gaps in what concerns understanding the impact of energy efficiency investments and policies. How much does rebound matter in that context? What are other research needs in energy efficiency? Are we missing the point by focusing in rebound versus other things?

It is customary in this circumstance to… well, answer the questions. This would presumably entail describing what I consider to be interesting technical economic and/or policy questions attendant to the rebound and efficiency. Regarding efficiency, I have done this numerous times and may do so again in the future.

For the present purpose, however, I will take a different tack. I will instead have a go at deconstructing the questions, as it were. My remarks will be in the vein of, for want of a better phrase, policy and regulatory anthropology and history: I will offer my views on the context for this workshop and the recent renewed attention to the rebound effect, as well as to parallel considerations for energy efficiency analysis and policy more generally.[1]

To that end, let me begin by posing what I think is at present the most important question about the rebound effect:

Why are we still talking about it?

While never having done any actual research on the rebound, I consider myself to be reasonably well-informed about it in the sort of way that one is about topics that are a degree or two of separation from one's core expertise and interests. Whether the "micro" or "macro" version, and by whichever of the now apparently proliferating definitions, the rebound is an interesting and certainly policy-relevant topic; I would say it's important, although not overwhelmingly so, for

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[1] In keeping with the informality of these notes, with one exception I have eschewed citations or references. However, I will gladly supply them to interested parties in instances where my statements are more-or-less fact-based as opposed to strictly interpretive.

quantitative energy policy analysis. However, from this vantage point, as near as I can tell it is not – as far as these things go - technically complex, "deep," demanding of feats of econometric prowess for its resolution, and so forth. A relatively small but nonetheless steady stream of papers have appeared over the decades – enough effort, it seems to me, to have "taken care" of the problem. Yet here we are, thirty years after Khazzoom's paper launched the debate, witnessing apparently undiminished controversy, dueling "studies" from advocacy groups, and this workshop itself, which judging from the agenda will be starting from something akin to *de novo* to sort things out and define a "research agenda."

To posit an answer to my question: The reason we're still talking about it is that something has gone wrong, or at least failed to go right. The network of energy policy-makers, regulators, specialists in universities, national laboratories, and the private sector, economists, technologists, and others, has not managed in thirty years to resolve this important-but-not-unusually-difficult problem. If by chance, you – the reader – are one of those who has worked on this issue, and are thinking "*au contraire*, I already solved this problem," or you are another quasi-bystander like me and are perhaps thinking, "actually, I think [Person(s) A] pretty much worked this out back in [year or decade X], and [Person(s) B] just doesn't/don't want to admit it," or are otherwise having objections along these lines, then you are merely providing evidence for my point. "Resolution" means that some theory, result, stylized fact, etc., becomes generally accepted by all or at least most of the combatants, with the hold-outs finally ceasing and desisting even if they don't overtly surrender. The fact that this workshop is occurring at all is evidence that this has not happened with the rebound effect.

Now, your next objection might be, "well, this is how economics works – look at the efficient markets hypothesis, rational expectations and real business cycle theory, neo-classicism vs. Keynesianism, etc. – these disputes go on indefinitely." The reason that this objection applies only very weakly if at all in this case is that those debates are about problems that are *hard*, and – per my previous remark – the rebound effect is in no way in the same category. A few years of debate would have been expected, perhaps even a decade, but not thirty years and still going strong.

So what has gone wrong? I'm not sure I have an explanation so much as a description, the first element of which is that this phenomenon is not limited to the rebound effect. Alas, large swaths of energy policy analysis, economics, and modeling, either effectively stood still from the mid-1980s until very recently, or actually regressed. This is a reasonable paraphrase of what I have just said about the rebound effect. It is also true about energy efficiency more generally (about which more below).

To draw out one aspect of this, consider the more general topic of consumer behavior and choice related to energy use. Through the 1970s and early 1980s this emerged and grew rapidly into a very active and respectable academic research area, drawing upon virtually all the social sciences – not just economics but also psychology, social psychology, sociology, anthropology,

marketing, and others. There were *hundreds* of papers, or more, published in refereed journals during that time, in addition to a sizable grey literature.

Then…around the mid-1980s, oil prices collapsed, energy's (first) fifteen minutes of policy fame ended, and the attention of the research world shifted to other things. Energy as a public policy issue didn't disappear, of course; in particular, by that time there were established pockets of interest, in national labs, advocacy groups, and most importantly regulatory agencies. But the critical mass as well as heterogeneity of research expertise and interest that would have been needed to see that body of knowledge consolidated, applied, and extended, ceased to exist. With all respect to those who have continued to work on these topics (and this group includes me), theirs' (ours') was ultimately not a concentration of effort sufficient to continue moving things forward in an intellectual or methodological sense, to a degree that would have yielded the sort of progress or resolution I noted above, on things like the rebound effect.

But this is only half the story, in a manner of speaking. The "pockets of interest" that I referred to above were established enough that they were able to maintain – but not advance - the standard paradigms for energy demand and efficiency analysis, whether economic, policy, or engineering, essentially as they existed twenty-five to thirty years ago. And so, as tends to happen in such circumstances, those persons and institutions who continued to work on and/or regulate energy demand came to *rely upon* the maintenance of this paradigmatic status quo.

The consequences are illustrated by the so-called "micro" rebound effect, specifically that of households marginally increasing their use of energy services (and thus fuel) in response to cost declines of these services resulting from any of the standard energy efficiency interventions – utility DSM, codes, standards. My $0.02 on this is as follows. There is now – actually has been for at least a decade – a body of *empirical* research showing that the effect is "real." Notwithstanding that much of this work is microeconometric, one does not have to believe in the standard caricature of the uber-rational *homo economicus* to take this evidence seriously; increases of demand in response to decreases in prices of goods or services in general (not energy alone) are what I would call a robust empirical regularity, figuratively visible from the proverbial armchair. It is certainly the case that there is, as with most demand behaviors, a good deal of uncertainty in the magnitudes of energy service –specific rebounds as well as great variation in these magnitudes among different such services. But basically, the effect is not zero, and appears to be mostly not "big," but big enough that it needs to be accounted for in quantitative policy planning and evaluation.

Recognition of this phenomenon appears to be no longer universally anathema among energy efficiency experts. For example, the analytical procedures used by my colleagues at LBNL in the appliance efficiency standards analysis for the U. S. Department of Energy incorporate an explicit rebound effect. But neither does it appear to be broadly accepted at this point. Why? I think there are two related reasons, both having to do with the process I described several paragraphs ago. First, the technology-based end-use efficiency paradigm that emerged in the

1970s was strongly grounded in a methodology that assumed an independence of efficiency investments from prices, whether of equipment or fuel. (Maintaining this disjunction was one of the explicit aims of the so-called "conservation supply curve" methodology.) Thus, one can imagine that the rebound effect – a price response – would be viewed as a paradigm-threat, if you will. Second, even without such a reaction, the emphasis on *ex ante* estimates of efficiency "potentials" in policy and regulation could still be seen as broadly at odds with an empirical factor (i.e., the rebound effect) that tended to only reduce the observed *ex post* realizations of policy or program-induced energy savings relative to the prior estimates. This would be particularly true as, in more recent years, the estimated magnitudes of these potentials started to trend upward (following a previous decline through the 1990s) as efficiency policies were increasingly advocated for CO2 emissions abatement.

Mutatis mutandis, this narrative applies more generally to energy efficiency analysis and policy during this era. But stepping back from the rebound issue specifically, there is an important difference: Whereas work on the rebound effect *per se* never seems to have gained enough traction to move forward in a definitive way, a great deal of work on consumer behavior and energy use more broadly, including efficiency investments, was done and then… essentially forgotten. This was the fate of a large body of social science research on energy demand done in the 1970s through roughly the mid-1980s. The past several years have seen a recrudescence of interest and activity in social science research on energy demand, typically under the rubric of "behavior." With some important exceptions I mention below, there is a great of re-inventing the wheel in this.

Perhaps the most important of the forgotten lessons has to do with the canonical "market barriers," putative gaps in information available to consumers regarding energy efficiency, and limitations on the availability of financing to make efficiency investments. In the past two-and-one-half years, with energy efficiency newly re-emphasized in federal policy to an extent not seen since the Carter era, these barriers have come to occupy pride of place in the justifications for efficiency policies. Now, some readers will be aware of the debate over market barriers to energy efficiency that erupted in the early 1990s and flourished (if that is the right term) for several years. To over-simplify a bit, this was focused primarily on the question of whether these and other market barriers to efficiency constitute bona fide market *failures* as those are defined in neo-classical welfare economics, and were therefore admissible as justifications for policy (from a cost-benefit perspective). Even more than the rebound debate, this barriers issue has been the locus for the generic "engineers vs. economists" battles over technology-based energy efficiency policies and regulations.

Those of us who engaged in this area in the 1990s may now shake our heads in both judgment and wonder at the fact that the work that was done then – some of which was quite good - has had no apparent impact whatsoever in the policy arena. However, this is not the point I wish to emphasize; rather, I raise the 1990s barriers/failures debate in order to distinguish it from, and call attention to, what came earlier, again harking back to the late 1970s and early 1980s.

The first era of technology-based efficiency policies and regulations, which began in the 1970s, was anchored quite firmly upon the claim that a lack of information and financing prevented consumers and firms from making efficiency investments deemed "cost-effective" by *ex ante* engineering calculations. These factors were among those most exhaustively studied by researchers in the 1970s and 1980s, in both academic work and program evaluation. By the mid-1980s it was very clear, and extensively documented, that providing information and financing, *per se*, on average did not significantly stimulate private efficiency investments, certainly not enough to close the gap with engineering estimates. This is not to say that, for example, utility "demand-side management" programs didn't work (although there was wide variation in outcomes); rather, their effects were generally very modest relative to expectations and predictions.

In fact, social scientists had by that point concluded that the model of behavior underlying the technology paradigm for efficiency policies, which dictated a focus on information and financing, was simply incorrect. This conclusion was argued quite forcefully in, for example, several reports of the U. S. National Academy of Sciences. In one of these, the authors stated that

> "Federal energy information programs have…proceeded from implicit assumptions about the way information works – and those assumptions are fundamentally wrong. The programs tend to be constructed as if people presented with accurate estimates of, for example, thermal performance of a variety of furnaces, would use this information in purchasing decisions. Even when people are acting as investors, however, this is not the case: information that reaches a person's eyes or ears is not necessarily noticed, understood, assimilated, or used. For information to be effective in a decision process, making it available is not enough." [2]

This is but one illustration of how in this instance applied social science provided a knowledge base for policy that was both broad and deep, as well as timely. This could be considered a success story, I suppose. Unfortunately, this knowledge base was for all intents and purposes ignored, and the policy paradigm that had been developed and implemented in the 1970s persisted, and following many years in the wilderness is now – as I described above – once again pre-eminent, with "information" and "financing" at its core.

This historical excursion provides background for me to return to the workshop organizers' questions regarding research needs. To start, it should now be clear why I used the term "anthropology" at the beginning of these remarks. The debates about the rebound effect, and the energy efficiency 'gap,' have for most of the past thirty years had a distinctly tribal character, and it should not be assumed that additional and perhaps "better" research, *per se*, will resolve them. For this last assertion, the history I have sketched provides empirical evidence: More and

[2] Page 74 in Stern, Paul, and Elliott Aronson. (1984). *Energy Use: The Human Dimension*. Washington, DC: National Academy of Sciences.

better research on energy demand – relative to most of what's been done since – was conducted in the 1970s and early 1980s, and it had essentially no effect. One might also invoke economics here, and ask: Who (or what, e.g., institutions) has which incentives or disincentives to settling some of these questions one way or another, once and for all? I would strongly suggest that this question be considered quite carefully before designing, or attempting to implement, a new research program on the rebound effect and/or energy efficiency investments.

Now, having said all this, it may be the case that, in the event, the Workshop will prove to be a turning point in re-directing the rebound debate toward the realm of something like "normal" scientific and/or intellectual discourse. I say this specifically because of the invitation list that was circulated last month: The organizers are to be commended for at least attempting to gather some key protagonists in order that the battle be joined in earnest. At the time of this writing, however, I do not know exactly who'll show up.

To conclude: At several points in these remarks I have qualified my admittedly dreary assessment by alluding to certain exceptions, and I will now describe those; they are exceptions both in a "tribal" sense – as far as who I'm talking about – and in a methodological sense – as far as what they're doing.

In the past several years, as climate change emerged as a major policy issue, and energy efficiency policies and regulations were advocated for their CO2 emissions abatement potential, a small (but I hope growing) community of "new" economists has taken an interest in the details of demand behavior and demand-side energy policies; they are new in the sense of being new to this area (and, for the most part, young). Although not an economist myself, I have inferred that both the analytical tools deployed by, and the general methodological orientation among, micro-economists (at least the ones I observe) have changed – for the better – in the past several decades. First, there is a very strong empirical orientation, a focus on measurement. Second, as opposed to past decades in which empirically-minded economists may have emphasized things like parameter estimation, there is now an emphasis on experimental design and inference, and econometric techniques to support these are in common use.

These researchers have begun to make important contributions by, as the phrase goes, actually going out and looking at (i.e., conducting solid empirical analyses of) the phenomena of interest in energy consumption, efficiency investments, and policies: "split incentives" for efficiency, how information affects home energy retrofit decisions, the effects of building codes, the characteristics of consumer decision rules, and so forth. In my judgment, in the realm of energy efficiency, such work is precisely what is needed at this time, and it is picking up where things were left off several decades ago. *To be clear: The overarching research need relating to energy efficiency is to subject hypotheses, claims, and beliefs about behavior, and the policies and programs that are based on them, to rigorous empirical tests.*

For the reasons I have discussed, neither this nor any other research will easily affect the existing policy infrastructure, and of course the drive for national climate and greenhouse gas legislation has at least for the time being stalled. However, there are also reasons to be optimistic that social science might in the coming years become a more effective contributor to public policy, in energy and other domains. First, the evolution of information technology is driving a phenomenal expansion of computationally-intensive data analysis in many areas, so that "empirics" in general are becoming more accepted and employed. Second, the U. S. fiscal and political situation may – one can hope – result in closer attention to the practical results of government policies and programs rather than to their ideological significance. Third, the risks of climate change, and the stress they will place on the energy system, are not going away. For these reasons, both a more pragmatic, empirically-grounded approach to energy policy, and a sustained research effort to support it, could indeed be in our future.