

Climate Economics and Normative Expertise

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Abstract

I discuss three families of methodologies that could be used to assign values to the normative parameters relevant to social discounting in welfare economics generally, and climate economics more specifically. First, I argue that in particular circumstances, there cannot be philosophical argumentation for normative questions; specifically, this occurs when the particular values being sought are both non-critical and from a quantitative range. Second, I argue that social preferences are insufficient if we take the problem to be normative and that proposals for informed social preferences face significant challenges in implementation. Finally, I argue in favour of expert elicitation for experts in welfare economics, construed as those who understand the theoretical implications of adopting particular judgments. Those who understand the theoretical implications of adopting particular judgments will be better placed to develop coherent social plans while integrating relevant empirical data.

Keywords: social discount rate, climate change, normative expertise, economic methodology

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1 Introduction

Weighting the value of the present against the future is a normative matter.¹ By normative, I mean roughly that given any set of empirical data, it remains an open question how society, or a benevolent social planner, *should* or *ought* to weight the present compared to the future.² It is thus *not* a matter of normativity whether we (predictably) *will*

¹Economists might be more comfortable saying that particular parameter assignments are *value-laden* or *subjective judgments* (Arrow et al. 2004; Weitzman 2001) or are *policy parameters* (Pindyck 2013). In this context, all these terms are roughly equivalent, since the upshot is that empirical data alone does not suffice to adjudicate correct values.

²Another way of saying this is that not just empirical information, but also *value-judgments*, feed into truly normative judgments.

weight the present against the future in any given way. Stated baldly like this, the normativity of this weighting may sound to some self-evident (especially among philosophers) and to others wrong-headed (especially among American economists). Here, however, I am less interested in discussing whether the claim is true or not, since that has extensively been done elsewhere (e.g. [Arrow et al. 1996](#); [Broome 1992, 2012](#); [Dasgupta 2008](#); [Nordhaus 2007](#); [Stern 2007](#)). Instead, I want to introduce a particular worry that would follow from accepting this claim, and then suggest a solution. In particular, I argue (a) that the kind of *normative disagreement* in welfare economics which is relevant is unusually recalcitrant compared to many other normative problems in moral philosophy and (b) that we should respond to this by appealing to experts in welfare economics. In the jargon, I explicate and defend a particular form of prescriptivism in the discounting debate.

The framework I work with is consequentialist, was developed by [Ramsey \(1928\)](#), and was subsequently extended by [Cass \(1965\)](#) and [Koopmans \(1965\)](#), so it is sometimes called the Ramsey-Cass-Koopmans model. The original framework was developed to answer questions about optimal savings rates, but it is now used inter alia to evaluate the costs and benefits of marginal projects.³ The framework introduces parameters which govern the weighting of the present and the future, and the normative judgments I am interested in are various value assignments to those particular parameters. The context most relevant here, of course, is application of this familiar model to the analysis of policies concerning a relatively new long-term problem: climate change.

It is common to write our social welfare function (SWF) for some homogenous population as follows:

$$W = \int_{t=0}^{\infty} U(c_t)e^{-\delta t} dt$$

where c_t is the consumption of an individual at time t , $U(c)$ is the concave utility function of consumption, and δ is called the pure rate of time preference or the utility discount rate. Positive values of δ weight the future less than the present in an exponentially increasing manner. Assuming a simple and accommodating form, constant relative inequity aversion, $U(c_t)$ will take the form $\frac{c_t^{1-\eta}}{1-\eta}$, for some $1 \neq \eta > 0$, and $\ln c_t$ for $\eta = 1$, where η measures inequity aversion and higher values indicate greater aversion to inequity (in short, $\eta > 1$ indicates “progressive” increases in utility with respect to

³There are important questions about the limitations of the model (e.g. [Pindyck 2013](#); [Stern 2015](#)) as well as the welfarist and utilitarian assumptions that underlie it (e.g. [Caney 2014](#); [Shrader-Frechette 2014](#), Ch. 13), but my intentions here are narrow—which questions arise given the model, not what are the questions we can ask about it. Some may be happy to address the questions I raise by rejecting the presumptions underlying Ramsey-Cass-Koopmans, although my discussion may still hold value for them at least because the recalcitrance identified in §2 applies outside of this particular framework. It may also be of interest to have the opposing position in a more fully fleshed-out form.

consumption and $0 < \eta < 1$ indicate “regressive” increases in utility with respect to consumption).⁴

Under idealized conditions, the optimal social discount rate *SDR*, as applied to consumption, can be derived by maximizing the SWF subject to budget constraints, yielding the well-known Ramsey equation:

$$SDR = \delta + \eta g$$

where g is the (annual) per capita real growth rate of consumption (c_t).⁵ This is familiar, as is my flagging of δ and η as the normative parameters in the Ramsey equation. A helpful distinction will be addressing discounting at the *SDR*-level—i.e. trying to find a value for *SDR* directly—and at the component-level—i.e. trying to find values of δ and η and determining *SDR* from those values.

The judgments or preferences relevant to the discussion of discounting are assignments of particular values to these parameters. I call the assignment of values to these parameters the normative question, or the question of weighting the future against the present. In this essay, I critically discuss three families of methodologies for answering the question: those rooted in philosophical argumentation, those rooted in social preferences, and those rooted in expert preferences.

2 On When to Argue

Disagreement with respect to normative matters (“normative disagreement” for short) is pervasive in moral philosophy; it applies at levels ranging from particular judgments and intuitions to principles and rules and from the ontological status of morality to the semantic status of normative statements. Moral philosophers of course do not just sit on their hands in response to normative disagreement. They argue; that is, they try to give sets of premises and conclusions which bear certain logical relations to each other, usually aiming at deductive validity. By arguing, I intend that philosophical argumentation is something more demanding or principled than merely trading normative intuitions and seemings.⁶ Compare an alternative instance of normative disagreement. For most

⁴Some have argued for a prioritarian form of the SWF (Adler 2012), with an additional concave transform of utility, e.g. $h(U(c_t))$. Under Atkinson/isoelastic forms, the h function can be characterized by another parameter, say γ just as η characterizes utility as a function of consumption. I believe that utilitarians would take h to be the identity function (equivalently, they would take the limit case where $\gamma = 0$). In this more generalized framework, the parameter γ should, of course, be seen as another normative parameter like η and δ .

⁵The unflagging optimism of the dismal science can be found in its use of g as the name of the variable in question.

⁶For an influential account distinguishing between moral seemings and intuitions, see (Audi 2013). For my purposes, it is most important that neither is formed through explicit reasoning processes from first principles.

1. We can make philosophical arguments for conclusions with particular values when they can be supported by premises with particular values.
2. Particular values can have empirical or normative sources.
3. Normative sources are not forthcoming when particular values are non-critical and from a quantitative range.
4. Empirical sources cannot support conclusions which are normative.
- ∴ 5. Therefore, neither empirical nor normative sources are able to support normative conclusions with particular non-critical values and from a quantitative range [3,4].
6. Some normative judgments for parameter assignments in climate economics are for particular values which are non-critical and from a quantitative range.
- ∴ 7. Therefore, we cannot make philosophical arguments for some normative conclusions for parameter assignments in climate economics [1,2,5,6].

Figure 1: An Argument about Limitations of Argumentation

if not all actions ϕ , a moral theory will assign ϕ -ing some deontic status, such as permissible, impermissible, obligatory, or supererogatory. When arguing about the status of ϕ -ing, more can be offered in defence of a particular status than simple seemings—namely, a theory which generates a judgment about that status. This appeal to theories (or principles broadly construed) is a prototypical way that a normative status is defended by philosophical argumentation. So in response to the normative disagreement attached to normative parameter assignments in climate economics, a natural response is to suggest that philosophical argumentation can allow for rational and reasoned convergence (or at least clarification) in parameter assignments. In this section, I argue that this methodology will not be able to do this work, at least not directly by arguments in favour of particular values for the normative parameters. Figure 1 is a formalized version of my argument.

The types of judgments that we have normative disagreement about in this climate discounting debate—i.e. assignments of particular values to the normative parameters—are less amenable to the approach of philosophical argumentation than many other debates. Why? Because the parameters govern a *quantitative range* of possible values. The usual purpose of arguments in moral philosophy is to prove that particular actions or objects possess normative properties, such as rightness or goodness. Another way of saying this is that philosophical argumentation is applicable when the range of normative judgments is qualitative, but not when it is quantitative.

One way that this difficulty rears its head is in philosophical arguments that there

must be some particular threshold for some normatively relevant property, but any concrete suggestions for that threshold end up being subject to objections of arbitrariness; there is no further normative argumentation that can be brought to defend the threshold if considered as part of a merely quantitative continuum.

In other words, philosophical argumentation is most helpful for qualitative targets, such as determining whether an object *has* or *does not have* a particular property (e.g. an action's being permissible or impermissible). More generally, we can say that arguments are appropriate when the potential values are qualitative (permissible, impermissible, and obligatory being particular qualitative "values" in this sense). To be clear, when I say particular values, I do not mean value in the sense of value theory, but just assignments to a variable. Philosophical argumentation may also be helpful in some special cases when there are focal values which correspond with mathematical "critical values" (e.g. maxima, minima, inflection points).⁷ However, aside from such special cases, I suggest, philosophical arguments must fall silent.

Sorites arguments are emblematic of the issue.⁸ In a sorites argument, one extreme case is claimed to have some property (e.g. being a heap of sand); there are a series of cases with only quantitative differences (e.g. piles of sand, each with one less successive grain of sand); a claim that for all cases, the quantitative difference cannot make a qualitative difference (e.g. one grain of sand cannot distinguish between a heap and a non-heap); and a claim that another extreme case does not have that property (e.g. a single grain of sand is not a heap of sand). These claims yield a contradiction, but are all supposed to be plausible. How does this relate to my argument? Philosophical argumentation can establish that there is a qualitative difference (accompanying a merely quantitative difference) *at some point* (i.e. the difference claim cannot hold for *every* case), but philosophical argumentation cannot be adduced for precisely where, as my argument suggests. One might think that philosophical argument is still helpful here; at least an instance of sorites tells us about *the critical values* of the range, namely that one has a property that the other lacks. However, it is not philosophical argument itself which tells us this: that the extreme cases have or lack a particular property arises from an appeal to intuition about common linguistic usage.

Another way we can see the more limited use of philosophical argumentation in the context of weighting the future against the present is by considering where the philosophical discussion *has been* focussed: arguments that $\delta = 0$ (e.g. Broome 1992, 1994; Caney 2008, 2009, 2014; Meyer and Roser 2012; Parfit 1984; Sidgwick 1981).⁹ This is because philosophical argumentation is best suited to critical values. The value $\delta = 0$

⁷A critical value is usually found by setting the derivative of a function to zero; maxima, minima and inflection points can be identified in this manner.

⁸I do think that my argument applies beyond normative conclusions, and I discuss this possibility below. Obviously, sorites arguments are usually not intended to demonstrate any normative claims.

⁹Sometimes there is imprecision about whether philosophers argue that $\delta = 0$ or that $SDR = 0$, but I think it more charitable to read them all as arguing for the former.

is a minimum—notice that there is no relevant maximum δ which is similarly a critical value, and we also see no philosophical argumentation aimed at particular non-zero values of δ . The problem is that this relegates philosophical argument to a very small part of the pertinent discussion space, and helps isolate philosophers from discussions of η which does not have relevant critical points.¹⁰

Others have had related concerns, but this particular worry has not been expressed. For instance, Moellendorf (2014, and work in progress) writes that “there is no basis in reason or fact” for particular parameter assignments. Related worries prompted Manne (1995, p. 392) to write that the “rate of time preference and the elasticity of marginal utility are inherently subjective, and there is no generally agreed-upon way to determine their values”.¹¹ Broome (1999) writes in the introduction that philosophers are less likely to think comparatively than economists. This is another similar thought. However, I am not aware of others who have specifically linked this to limitations to the potential targets in philosophical argumentation.

Normative problems of this type, i.e. with a quantitative range of values which lack critical points, are recalcitrant to direct philosophical argumentation. It is not—I suggest—possible for philosophical arguments to demonstrate evaluative or comparative claims in this context, e.g. that $\eta = 1.3$ is normatively correct or that $\eta = 1.3$ is more normatively correct than $\eta = 1.9$. Philosophical argumentation underdetermines the answer to this question. Obviously, the issue under discussion—discounting or weighting of the present against the future—is only an example of a much larger potential class of such recalcitrant problems.

There are at least two ways the conditions on this argument could be relaxed, making for a stronger claim. First, the argument does not require that we are considering specifically *normative* judgments, so we could *relax the domain*. Normative judgments are defined to be independent of (merely) empirical considerations; in short, given any empirical results, genuinely normative questions remain open. But there may be other classes of judgments which are non-normative that this argument applies to, such as ontological and metaphysical claims, as long as they involve particular non-critical values from a quantitative range. To illustrate the normative version in this context, it may be that some group of individuals, selected in any desired manner, either state or reveal particular levels of relative risk aversion, which we can model with a constant relative risk aversion utility function characterized by $\eta = n$, where n is a (non-negative) real num-

¹⁰Technically speaking, of course, $\eta = 0$ is *also* a minimum since η is defined to be always non-negative, but this parameter assignment corresponds to a linear utility function, and decreasing marginal utility of consumption is taken to be indispensable, for both psychological and theoretical reasons. For one thing, distributing consumption differently would not have any effect on social value if $\eta = 0$ were adopted, and this contradicts deep intuitions about equity and fairness, as well as making some relevant questions trivial.

¹¹Although perhaps this should be read as an objection to phenomena which are difficult to examine empirically.

ber.¹² If, as we are assuming for the purposes of this essay, the social welfare function we should use as a society is genuinely normative, this does not tell us which function to use. This is true regardless of the group or how it is selected (i.e. by simple random sample, regional representative selection) or even if (per impossible) the group is a census of all affected by potential policies. Note that this does not mean that such data would be *unimportant*, either normatively or otherwise. I think it normatively relevant, just not normatively determinate (I return to this discussion in §3). I return to how philosophers can respond to this argument after introducing the second potential relaxation.

Second, I do not think that all critical points can be addressed by philosophical argumentation. However, I am less sure when this can be relaxed, so I do not include in the basic argument. For an example in the current context, under the Ramsey-Cass-Koopmans framework, with the assumption of constant relative risk aversion, $\eta = 1$ is a critical value: it represents a discontinuity in functional forms of the utility function (specifically, by definition, $\eta = 1$ generates a logarithmic utility function, but it is not continuous because in standard representations, setting $\eta = 1$ leads to dividing by 0). But I am not aware of a way that this helps philosophical argumentation to be more helpful in this context, such as by arguing for the claim that $\eta = 1$ is the (or a) correct value or that $\eta \neq 1$ is normatively more defensible than $\eta = 1$.

One important implication of this argument is that, if, for instance, we adopt a moral realism where those truths do include content about non-critical, particular values in quantitative domains, we cannot argue for such truths (at least not directly). Similar remarks apply in other relevant domains.

However, there is a way that philosophers can get around this limitation and contribute meaningfully to this debate, without relegating their arguments to a small part of the discussion space. This is to apply philosophical argumentation to the various *methodologies* used to assign parameter values, without arguing directly for particular parameter assignments themselves.

One might think that this is unlikely to help, since arguing about methodology appears to be merely a slightly removed or abstracted way of arguing about the values themselves. However, this is not the case. Arguments about ways or methodologies of determining particular values need not themselves appeal to particular values in some range, such as by including particular values in premises.

For this reason, the following sections rely upon philosophical examination of methodologies, not of particular values. Of course, adoption of a particular methodology conjoined *with* relevant empirical work will generate particular values, but we cannot directly argue for particular, non-critical values.

¹²There are likely to be many more issues, since given any set of stated or revealed preferences, it is unlikely that the resultant utility function would be representable so simply, but I can grant these issues for the sake of argument.

3 On When to Listen to Everyone

Some have argued that the parameter values should be fixed by appealing to the preferences of society, usually as revealed through market behaviour.¹³ In practice, this means determining the real risk-free rate of return r and arguing that, under optimal conditions, $r = SDR$.¹⁴ There are many objections to this methodology, and it is beyond the scope of this article to enumerate and articulate them fully. However, it is worth introducing them briefly. For simplicity, we can divide these objections into problems with implementation and moral objections.

Problems with implementation range from temporal inconsistency of actual individuals (e.g. [Frederick et al. \(2002\)](#) explain how actual decision-making can and does diverge from temporally consistent utility functional modelling) to distortionary taxes (e.g. [Portney and Weyant \(1999\)](#) and ? discuss various methods of approximating the real risk-free interest rate given that we do not live in a first-best world—e.g. our markets include taxes and subsidies which distort signals) and from the incompleteness of markets (e.g. *inter alia* we lack complete long-term markets, so long-term preferences must be inferred from medium- or short-term market behaviour) to the inability of markets to capture non-marginal effects or projects at a global level (e.g. [Dietz et al. \(2009\)](#) point out that if marginality does not apply, then individual agents do not affect the resultant outcomes).

Moral objections range from the biased manner in which such a methodology measures social preferences (e.g. [Atkinson \(2001\)](#); [Caney \(2008\)](#); [Quiggin \(2008\)](#) point out that employing market interest rates to determine social preferences effectively makes preferences matter on a one-dollar-one-vote basis instead of one-person-one-vote) to potential divisions between economic behaviour and relevant values (e.g. either due to different aims in economic life and moral life or due to behavioural biases or heuristics) and the distinction between intrapersonal discounting and interpersonal discounting (e.g. [Dasgupta \(2008\)](#) suggests that models should distinguish between (plausibly permissible) intrapersonal discounting and (plausibly impermissible) interpersonal discounting).

I list these objections partially because I endorse them, but more importantly to contrast them with the response that we can adopt here. Since we assume that weighting the present against the future is a normative question, neither social beliefs nor social behaviour suffice to determine the answer. In short, it may be the case that society does not hold the correct moral beliefs.

Phrased this way, this response invites two further lines of thought. Appeals to

¹³One justification is that preferences revealed through behaviour are likelier to be honest than those merely stated ([Dietz et al. 2009](#)).

¹⁴Obviously, this underdetermines values for η and δ , so this is only the starting point of such a methodology if we are adopting the social rate of time preference version of the Ramsey-Cass-Koopmans framework.

social preference in moral philosophy are often defended in two ways: epistemological and constructive. The epistemological route is that the average views (either the mode, the median, or the mean, but sometimes unspecified which) of society are likelier to be accurate.¹⁵ On this route, we assume some belief-independent target and that people's average beliefs will be more reliable at hitting this target than any given belief, perhaps over some given domain.¹⁶ The constructive route is that the moral truth is *constructed out of* or *constituted by* the beliefs or values of individuals. On this route, we assume there is no belief-independent target; instead, the normative truth is a function of the beliefs or values of society.¹⁷ Thus, on this view, it is not surprising if society holds correct moral beliefs—their being correct is constituted by society's holding them.¹⁸

Either of these answers could in other contexts help buttress the claim that we should appeal to social preferences but neither of them help here. The constructivism of the second route is ruled out by our assumption that the answer is normative, and not merely an aggregation of societal beliefs or behaviours. In terms of the first route, I would agree that the beliefs of society would be normatively relevant (even if not normatively *determinate*), since they could be used to check the plausibility or acceptability of particular parameter assignments. However, as stated above, it is unconvincing that individual normative beliefs about weighting the present against the future in a social context can be read off of market behaviour, even if we assume that individuals *have* coherent and consistent beliefs in this domain. Moreover, since parameter assignments are part of a framework that most individuals do not understand, directly eliciting stated preferences is, at the very least, challenging.

It is worth considering a proposal of this latter stated preference type. A strong con-

¹⁵We can trace this at least as far back as Aristotle (1984, 2033f), from the *Politics* III.11, 1281b1–10:

For the many, of whom each individual is not a good man, then they meet together may be better than the few good, if regarded not individually but collectively, just as a feast to which many contribute is better than a dinner provided out of a single purse. For each individual among the many has a share of excellence and practical wisdom, and when they meet together, just as they become in a manner one man, who has many feet, and hands, and senses, so too with regard to their character and thought. Hence the many are better judges than a single man of music and poetry; for some understand one part, and some another, and among them they understand the whole.

¹⁶In the jargon, we assume a form of *metaethical realism*.

¹⁷In the jargon, we assume a form of *metaethical constructivism*.

¹⁸I believe that the consequentialism underlying standard economics could be construed in this manner, roughly with it being normative to satisfy people's preferences *because* those are people's preferences. However, an alternative—and popular—understanding is that economics is not at all concerned with normativity, and that economics is concerned with getting people what they want, not critiquing their wanting of it. I am less certain that this second interpretation is stable, however, since it seems to presuppose that we should try to get people what they want or that it is valuable to get people what we want. Presumably, we should do so, once again, *because* it is what they want, which seems to me like constructivism once again. Discussion and further relevant citations would be greatly appreciated.

tender is [Kopp and Portnoy \(1999\)](#), who suggest mock referenda to elicit stated preferences. Information and detailed questions are submitted to selected households. The questions must be detailed enough that one can infer willingness-to-pay and willingness-to-accept for various outcomes. They admit that this would require a significant amount of information to be provided, ranging from potential environmental costs and benefits to the households in question to international comparison classes, and that this would be complex and costly to assemble. To illustrate the kind of information required, consider their suggestions:

For instance, [sample households] should be told that a program that prevents, say, a half-meter increase in sea-level rise will do the most good in low-lying undeveloped countries such as Bangladesh. . . They might be told that a policy that helps slow forest secession would be especially valuable to some countries or parts of countries, but not to others. And, they might be told that the reduced incidence of vector-borne diseases will do the greatest good in tropical countries where these diseases would be most likely to proliferate. (92)

Note that this is before the households would even be informed of the policies under consideration and how those policies would impact that particular household. On the one hand, I agree with Kopp and Portnoy that such a basic international understanding of environmental policy is necessary for households to understand the implications of policy choices, even at the national or regional level; if their information were limited to the impacts of policies on a purely regional or national level this could potentially discount the impacts on environments at greater risk. For this reason, I do not think that mock referenda where households had a limited understanding of the interrelated nature of environmental policy would be meaningful or normatively relevant (so I agree with their strong informational requirements). On the other hand, the quantity and complexity of such information makes implementation of such a proposal practically difficult or perhaps unworkable. Kopp and Portnoy are aware of such challenges and, in response, they suggest that the goal would be to educate the household on a particular issue to the level of an American senator (i.e. a non-expert who we trust to have some information on the issue, or at least enough to meaningfully vote on it).¹⁹ If such mock referenda

¹⁹Although it's worth noting that, according to former American Senate Majority Leader Tom Daschle, the harried modern American senator may not cut quite so ideal a figure: "You don't have a clue what's on the floor, your staff is whispering in your ears, you're running onto the floor, then you check with your leader—you double check—but, just to make triple sure, there's a little sheet of paper on the clerk's table: The leader recommends an aye vote, or a no vote. So you've got all these checks just to make sure you don't screw up, but even then you screw up sometimes. But, if you're ever pressed, 'Why did you vote that way?'—you just walk out thinking, Oh, my God, I hope nobody asks, because I don't have a clue" ([Packer 2010](#), p. 42).

were successfully run, we could answer pressing environmental concerns by appealing to the stated preferences of the actual citizenry.

Of course, under our normative assumption, any such mock referendum would not determine to answer the question of intertemporal valuation. This is because (a) the questions would need to be carefully framed to distinguish between what the household prefers *for themselves* and what they prefer *for social or moral decision-making*; and (b) even if this condition were met, what society prefers does not make it morally true. However, if the first condition were met, I would consider the results to be normatively important. The reason is that, such households would *develop* expertise in the sense I defend in the following section. In particular, they would learn about the theoretical (and perhaps social and scientific) implications of adopting particular judgments.

However, my more pragmatic concern would be that very few households would take this responsibility seriously. It is helpful to consider a contrasting case: Nielsen households, which report their television-watching habits so that American networks can gauge audience share. Unlike Nielsen households, I doubt that mock environmental referendum households would be (a) engaged enough to actually do the tedious work of reading and digesting the information provided to them, or (b) be considered socially desirable or exciting (Nielsen households know that they influence which television shows will be renewed, so for some this imbues the drudgery of recording watching habits with social cachet). Furthermore, inevitably some of the information will be mathematically or intellectually taxing for non-experts. This will be exacerbated by the fact that, unlike senators, understanding this information is not the *job* of these referendum households. Of course, [Kopp and Portnoy](#) could respond that the houses in their mock referenda would know that they influence actual policies, but for most of the public (I hypothesize, sadly) environmental policies will be much more abstract and unimportant than which television shows we will see again next season.

So who both finds these questions interesting *and* is informed about them? I argue in the next section that these concerns suggest that parameter assignments should be a function of those who are closest to these questions, experts.

4 On When to Trust the Experts

In this section, I argue that there are both theoretical and practical benefits to adopting a methodology which appeals to expert judgment in assigning values to these normative parameters.

The account of expertise which I endorse holds that, within normative domains, expertise consists in understanding the theoretical implications (or potentially also the practical implications) of adopting normative judgments.²⁰ Although I use the term

²⁰A paper I've written which is under consideration expands on the summary here.

“normative experts”, this should be thought of as elliptical for the more perspicuous but unwieldy “experts in normative domains”. This can be contrasted with expertise in non-normative domains, where expertise consists in understanding the meaning (in the jargon, the truth-conditions) of judgments in the domain and knowledge of the relevant states of the world. Experts within both normative and non-normative domains must have interactional expertise, meaning facility with the basic terms of the domain and their relationship in such a manner that they can communicate *with* experts in that domain (Collins and Evans 2007). In addition, in non-normative domains, expertise is characterized by having knowledge of the aspects of the world relevant to that domain whereas, in normative domains, expertise is characterized by understanding the theoretical implications *of* adopting particular judgments. The ideal normative expert understands both the relevant empirical facts of the situation *and* the theoretical implications of adopting particular normative judgments, but of course, one could contribute relevant normative expertise by understanding only one of these types of implications.²¹ Who is a normative expert is naturally also a dynamic question; as mentioned above, one can impart relevant information to a layman to the extent that they become a normative expert—on one common view, this is the purpose of citizen deliberative bodies in democratic decision-making. However, I take this to be a more uncommon case; usually, experts are those who already understand the relevant implications, due to *inter alia* training or relevant research interests.

Of course, all of these types of expertise are comparative, so they require an implied contrast class. It is worth noting that my account of normative expertise is in stark contrast with some more traditional accounts of expertise, which appeal to truth or knowledge; I discuss this further below.²²

In the present context, the upshot is that many (although of course not all!) of

²¹Along these lines, one way to instantiate this methodology could be to have those who understand the non-moral theoretical implications (e.g. environmental or welfare economists) of adopting particular normative judgments explain the potential outcomes and then have those who understand the moral theoretical implications (e.g. moral philosophers or theologians) attempt evaluations of these implications. This is a familiar methodology in contexts such as bioethics, but there are two reasons I think it less applicable here. First, there are issues of complexity (the welfare and risk implications of particular normative judgments in climate economics can be extremely technical). Second, the framework of Ramsey-Cass-Koopmans *assumes* consequentialism in general—and discounted utilitarianism in particular. Since we are adopting the framework (at least hypothetically), there will be relatively few issues of moral theory that can ground particular value assignments to the normative parameters. In a bioethical context, in contrast, there are usually several substantive moral theories which are in play.

²²Those familiar with this debate will see that I am advocating appeal to *ethical expertise* over the stronger (and more “traditional”) *moral expertise*, although I am calling both versions of *normative expertise*. The distinction between ethical and moral expertise is that moral experts know (or reliably believe) the true moral theory, whereas ethical experts merely know candidate moral theories and can apply these candidates to particular cases. As I assumed that we are operating under normative disagreement and are unable to identify the moral truths, appeal to moral expertise is unavailable to me; I also suspect that this assumption obtains in the real world.

the relevant normative experts in the present contexts would be economists who work with welfare and climate economics, due to their greater familiarity with the theoretical implications of the relevant normative judgments (namely, particular value assignments to the normative parameters), would be the relevant experts in this normative domain.²³ Note that, although the judgments at issue here—parameter assignments in welfare economics—are directly related to economic, normative expertise about these judgments need not be limited to economists. Whoever understands relevant implications (or more precisely, to the extent that they understand the relevant implications) would possess normative expertise in the relevant sense. Among others, this is likely to include legal analysts and social scientists, although to a lesser extent than working economists since the relevant implications concern the importance of these parameters within broader economic theory. For instance, in the simple Ramsey-Cass-Koopmans framework, the utility function is not only relevant for comparisons of intergenerational consumption, but also for risk-aversion. Those who know the ways that various parameter assignments have implications for ideal savings rates or the cost-effectiveness of particular policies, for instance, are also likelier to have experience working with the relevant models. However, to be consistent with our initial normative assumption, we cannot say that this type of expertise is likelier to make their preferences morally *true*.

Since this notion is comparative, we can constrain our population of experts depending on how informed we want our expert preferences to be. For instance, if we wanted a very high level of expertise, we could consult so-called blue ribbon economists who specialize in this topic and have extensive experience examining the details of various parameter assignments. This has already been done in this context (e.g. [Arrow et al. 2013, 2012, 2014](#); [Weitzman 2001](#)). If we wanted a broader population to draw from, we could appeal to economists in general (e.g. [Weitzman 2001](#)).²⁴ However, in line with the intention of drawing a broader community of relevant experts, [Drupp et al. \(2015\)](#) have adopted a methodology which is more conducive to the position I espouse: to select those who have published articles with relevant keywords. [Drupp et al. \(2015\)](#) thus select their sample in such a way that the results are not limited to economists, since non-economists may have written or co-written papers which are relevant to discounting. For my purposes, this is a strength of this selection process; those who are familiar with the economic implications of particular parameter assignments need not all be economists. I expand on some examples of expert elicitation in §5.

It is also important to note that methodological consistency urges consistent approaches to the different normative parameters. I endorse what [Dasgupta \(2008, p. 159\)](#)

²³Of course, I am not the first to suggest this. Recently, [Pindyck \(2015\)](#) and [Sunstein \(2014\)](#) have endorsed more appeal to expert preferences in the context of discounting. Thanks to Moritz Drupp for drawing my attention to these.

²⁴[\(Weitzman 2001\)](#) includes *both* more general economic elicitation as well as blue ribbon elicitation. In the sense of expertise relevant here, the latter are more justifiably labelled as “normative experts”, although it is a weakness from my point of view that it limits the pool exclusively to economists.

points out, namely, that assigning a value to η on the basis of revealed preference but assigning a value to δ based on moral principles and theory is “neither good economics nor good philosophy”. So one can be methodologically consistent by adopting a descriptive approach to both (which is ruled out by our assumption that these are genuinely normative parameters) or by adopting a prescriptive approach to both. There are different ways to be prescriptive, but adopting expert elicitation is an obvious candidate, and adopting it for all of the normative parameters thus has an advantage over any mixed approach.

The reasons for appealing to this conception of expert judgment in the context of normative parameter assignments are multiple: first, it encourages transparency in methodology; second, it encourages coherent social planning where parameter assignments like η cannot be settled by argumentation; and third, it applies with minimal metaethical and moral presumptions; and finally, it holds pragmatic advantages over descriptive methodologies.

The first reason is about transparency. In climate economic modelling—as with many other types of modelling—there are many degrees of freedom, and there may be political pressures to assign values to the normative parameters to fit particular preconceived ends (Pindyck 2015). Appeal to normative experts in general allows both the given modeller to contribute their preferences to the assignment to these parameters as well as the check of non-unilateral parameter assignments. This separates the judgments which rely explicitly on values from those that do not. Such separation can also increase the trustworthiness of such estimates (cf. Reiss 2014).

A closely related, but stronger, version of this worry is that climate economists are *unable* to explicate their value-judgments, perhaps because these permeate their analysis or perhaps because the terms are thick, with inextricable evaluative and non-evaluative components (cf. Dasgupta 2005, 2009; Putnam and Walsh 2007). More generally, prescriptive approaches are meant to avoid disguising normative judgments under claims of empirical objectivity. In this context, Stern (2007, 2015) can exemplify a laudable attempt at explicating his normative judgments (labelling them such) and defending them (as well as in Stern 2014). This is at best an existence proof and it might not convince all such detractors, but it is suggestive. Furthermore, the influence of (Stern 2007) has led and presumably will continue to lead to more explicit discussions of the ethical assumptions underpinning such analyses. This can also be done with expert elicitation surveys, if they are done with fine-grained questions, as with (Drupp et al. 2015). This allows us to determine important distinctions such as whether given expert judgments are to be read as prescriptive (as opposed to descriptive, for instance). This last point is a significant worry with previous expert elicitation exercises, since the basis for prescriptive and descriptive normative parameter assignments are not directly comparable (Freeman and Groom 2014). This can be seen in the fact that the prescriptivists and descriptivists can broadly agree on the empirical data (e.g. market interest rates) without agreeing on their implications for assignments to these parameters. Of course, none of these remarks are

intended to substitute for standard sensitivity analyses to variations in these parameter assignments.

The second reason is that normative experts will be able to sketch both more comprehensive and more consistent *social plans*. In defining this term, I can appeal to the figure in welfare economics we call a social planner and say whatever it is that we usually think she *should design* is a social plan. Normative experts understand more implications of adopting particular parameter assignments. The importance of this understanding is that experts will be able to fit particular parameter assignments into coherent social plans. Coherency is obviously not *sufficient* to make a social plan good or desirable—it is trivial to generate coherent social plans which we would reject. But my rather minimal normative claim is that coherency is *necessary* for social plans. If this is so, expert preferences should be, ahem, preferred to general social preferences.

Taking η as an example, I would suggest that this is a very sensible methodology to adopt. As argued before (cf. Figure 1), we cannot generate arguments that establish particular values of η (assuming that conditions like these parameter assignments are genuinely normative hold). To simplify slightly, all we have are various risk-tolerances for society, supported by particular intuitions or value-judgments.²⁵ However, parameter assignments for η have implications for savings and investment behaviour as well as potentially responses under types of risk. Since normative experts understand (some of) these implications, they can test them for coherency with *inter alia* observed behaviour under risk and theoretical distributions of investment and savings. In the ideal case, economic experts are aware of the parameter assignments that are suggested by social behaviours but can, among other things, adjust these values to account for various behavioural biases. In other words, experts can treat social preferences as normatively relevant, but not normatively determinate. Of course, none of this means that normative experts will know the normative truth, but on the minimal assumption that social coherency of planning is necessary for truth, this favours my methodology.

A third reason is that this methodology is purposefully neutral in metaethical assumptions. For instance, in moral philosophy, some argue that moral expertise is possible (e.g. Driver 2013; Singer 1972) and others deny it (e.g. Weinberg et al. 2010; Williams 1995). They discuss moral expertise in terms of having normative knowledge or true normative positions, whereas this more ecumenical account of expertise can remain agnostic about the outcome of that debate while still offering a substantive and potentially illuminating account of normative expertise.²⁶ Intuitively, knowing the *im-*

²⁵There is a critical value of $\eta = \infty$ which can be argued for. It ends up representing something like an intergenerational Rawlsian Difference Principle, but I set this aside since the inter-temporal implications would be absurd (for instance, no net savings and/or almost no mitigation at all) (cf. Arrow 1973; Dasgupta 1974).

²⁶It is also likely—although not logically necessitated—that experts on those accounts would be a subset of the experts on my account, since those who endorse “the” true substantive normative positions are likely to have reflected on alternative positions and so know the consequences of adopting different

plications of adopting particular substantive normative positions is independent of one's adoption or rejection *of* those positions.

Another related advantage of this account of normative expertise is that it does not commit one to any metaethical position. Regardless of whether you accept normative realism or anti-realism (such as constructivism or non-cognitivism), you can accept normative expertise construed as understanding the theoretical implications of normative judgments. This is not true with traditional forms of normative expertise, which require knowledge of the normative facts, presupposing a form realism. I suspect this is also more congenial at this disciplinary interface, since it seems to me that normative realism is better represented among philosophers than among economists.

A fourth benefit is that, unlike with non-experts, those familiar with the theoretical economic implications should be more easily able to distinguish between their personal utility function (and associated risk-aversion) and an ideal social welfare function. For non-experts, these may be difficult to distinguish, and it is difficult to design non-technical questions which are aimed at questions of social welfare and not at an individual's personal utility function. For instance, some have advocated finding values of η based on the inferred risk-aversion of market actors or psychological results. But this is clearly problematic if we distinguish between the question of how society should decide and how individuals decide. One important way that these two can come apart is that individuals are susceptible to preferences which are inconsistent in terms of risk or intertemporal trade-offs, whereas presumably we intend social decisions to be consistent.²⁷

Again the claim is not that what the experts believe necessarily tracks the normative truth more than non-experts or society at large. That would not be consistent with the assumption of genuine normative uncertainty assumed. Just as the claim that "Societal exhibits such-and-such savings and investment behaviour" leaves the normative question "How should a social planner invest in various mitigation and adaptation projects?" open, so does "Experts state that they have such-and-such social preferences over long term decision-making" leaves the same normative question open. However, this ties in with other worries that should be addressed.

The first, and I think most important, objection to this type of account draws on worries about economic expertise. There are two strains that this worry often takes: a worry about special biases that arise in the field of economics, and a worry about creeping technocracy.

Philosophers often worry about particular biases that arise in the field of economics.

substantive positions. The converse is less likely: given that there is divergence among substantive normative positions adopted, even among those who reflect on alternatives, only those which correspond more closely with the normative truth would belong to moral experts on their accounts.

²⁷This point is not without its detractors. [Arrow et al. \(2013, 2014\)](#), for instance, suggest using a declining discount rate, which under some circumstances generate intertemporally inconsistent evaluations. This potential implication appears problematic from a decision-theoretic point of view.

Since I am interested in questions that arise *given* the Ramsey-Cass-Koopmans framework, many of the foundational assumptions are beyond the scope of this essay (for instance welfarism, consequentialism, and anthropocentrism).

Another familiar concern is that economists—as with almost any other type of specialist—are subject to overconfidence in their own judgments and that their policy predictions could suffer as a result. (Of course, as mentioned, the normative experts will not *exclusively* be economists, although they are likely to be disproportionately represented.) A modern form of this argument, surveying some of the relevant behavioural psychology research, comes from Angner (2006), but these are familiar themes from Hayek. These worries have been reinforced by recent (perhaps unreasonable) public expectations that macroeconomics should have been able to predict the recent recession—or that once it occurred, economics could speak with a consistent voice about what policy would ameliorate it.²⁸ I share concerns that economists may be overconfident in their predictions of macroeconomic parameters and/or social impacts of various policies (Jimenez-Buedo 2014; Reiss 2014). However, it is important to note that *these* are not the parameters under consideration. For instance, the value of the parameter g , the real per capita growth rate of consumption, *is* a parameter for which overconfidence could be a problem. But overconfidence is not relevant in the same way to normative parameters, since overconfidence requires that there is a measurable standard by which one can fail, and—by assumption—normative parameters are not determined by any empirically measurable phenomena.

In practice, one might be worried that, since much depends on the discount rates that society adopts, there could be corruption of expert elicitation—e.g. by trying to have interested parties be legitimated as experts or by spuriously publishing papers with “discounting” as a keyword by lobbyists so as to include them. This can be thought of as an application of Goodhart’s law that “When a measure becomes a target, it ceases to become a good measure” because interested parties will attempt to hijack it. This is a potentially serious practical issue, but it is worth thinking about it in comparison with alternative methodologies, such as the referenda approach of Kopp and Portnoy (1999). Non-experts are much easier to persuade with sophomoric reasoning than experts, so it could be difficult to prevent representative households from being convinced by determined well-financed interests. In contrast, publishing papers in recognized journals is difficult; it would represent a fairly high barrier of entry for such parties. Similarly, while it might well be the case that it is possible to introduce ersatz experts into surveys, it could be difficult to mass produce them in such a way that they would pass any level of scrutiny. So while this is a concern that should be considered in practice by expert elicitation exercises, it seems comparatively challenging for interested parties to have

²⁸I am not myself sure whether this is a fair charge. For instance, British economists were at least fairly united in that the British policy was failing, cf. <http://cfmsurvey.org/surveys/importance-elections-uk-economic-activity>.

substantial influence.

The final objection I will address here is that, dialectically, there is no advantage to expert elicitation over social preference elicitation given by appealing to coherent social plans. The reason is that social plans are plans *of society*, so there is an important (although trivial) sense in which social plans developed as a function of social preferences will be more coherent. I agree that there is a sense in which this is true, since social preferences (if we can effectively and correctly infer them) will cohere better with current market behavior. However, this is a limited effect, since intertemporal social behavior is rarely internally coherent (just consider inconsistencies in risk-averse behavior). In exchange for this type of (I believe limited) social coherency, we lose potential coherency and determinacy in social plans themselves. Some will be willing to accept this trade-off, but it seems to me unappealing, especially in light of the advantages that experts in welfare economics can bring to the table. Furthermore, I suspect (and certainly hope) that welfare economists whose preferences are elicited are both interested in, and consider, the empirical data that could be used to inform their normative judgments.

5 Some Examples of Expert Elicitation

I was very pleased to discover after having begun this project that the type of methodology that I advocate is—while still fairly young—being performed in the economic literature. It behooves me to briefly discuss the exercises that have been performed in light of this discussion.

Weitzman (2001) suggests—somewhat tongue-in-cheek—a rather pessimistic view of expertise: “with very little exaggeration or cynicism, an ‘expert’ here might be defined as an economist who knows the literature well enough to be able to justify any reasonable social discount rate by some internally consistent story” (261). His survey was given over a large ($N > 2160$ from “about 2800” requests) “professional Ph.D.-level economist” sample and a smaller ($N = 50$) named blue-ribbon sample. One thing that strikes me as helpful throughout the expertise elicitation exercises performed to date is that this sample seems to self-select; for instance, “the most common objection from respondents was their complaint that they were ‘not an expert in this area’ and consequently ‘have no idea’ what to answer” (267). Insofar as this indicates some epistemic humility and was the most common objection, I think it serves to the credit of those respondents and is some evidence against worries about overconfidence that many have made.²⁹ Contra Weitzman, I think it is not best to include these respondents regardless (Weitzman tried to convince those who answered in this way to give a response); I think we should take those seriously who claim not to be experts. Perhaps—although we can-

²⁹In terms of the whole population, Weitzman suggests that approximately twelve percent were answering “under duress”, so the most that we can conclude is that the largest plurality of this twelve percent were claiming lack of expertise.

not know—they did not consider themselves experts because they had not reflected on potential values and/or they were not familiar with the theoretical implications of adopting particular values. If that were so, then they would have (less) expertise in my sense and so it would be sensible to exclude them. (The results found for the large sample were a mean $\mu = 3.96\%$ with a standard deviation $\sigma = 2.94\%$ and for the blue ribbon sample mean $\mu = 4.09\%$ with a standard deviation $\sigma = 3.07\%$.)³⁰ Of course, a major disadvantage of this study is that only economists were consulted in both samples, but, even more importantly, the economists considered were not necessarily expert in the sense relevant here: with respect to discounting, intertemporal valuation, and (climate) cost-benefit analyses.

There is less to say about the blue ribbon panel held at RFF in 2012 (Arrow et al. 2012) which was developed into recommendations in (Arrow et al. 2013) and (Arrow et al. 2014). All of these members are easily thought of as experts in my sense (probably in many others), having worked with welfare economics, its assumptions, and its theoretical interconnections for many years. Again, it may be that their level of familiarity with the implications of adopting particular normative judgments are matched by non-economists, but this is unlikely. Their conclusion is that, to accommodate future uncertainty about consumption growth rates and investment rates of return, declining discount rates are applicable.

Finally, the most recent expert elicitation exercise performed by Drupp et al. (2015) has several advantageous features. The most obvious is that selection via relevant keywords in publications does not limit the sample to economists (although it does limit the working timeframe by publication date). Another feature is that they elicit the components of the Ramsey formula, allowing to distinguish between *SDR*-level and component-level responses. Yet another is explicitly asking sample members to what extent they believe prescriptive and descriptive considerations apply to determination of the social discount rate. This was a similar large-scale survey ($N = 197$ out of a pool of 627 requests), and a large majority (80%) believe both prescriptive and descriptive considerations are relevant and the modal response ($N = 42$) was both should have equal weight on a scale of 0 to 100.³¹ If we take this mix seriously, I think that expert elicitation is well-justified, since experts in discounting are likeliest to be aware of the relevant empirical data to discounting, but can potentially adduce normative considerations and/or account or adjust for inconsistencies in social behaviour. I fully grant that this is a best-case scenario, and many experts may not reflect this carefully, but non-experts are unlikely to be able to perform either of these tasks in any case.

For my purposes, Weitzman's, Arrow et al.'s and Drupp et al.'s blue-ribbon experiments can more easily be defended as (relevant) expert elicitation methods, with Drupp et al.'s as the most defensible, since he allows for some non-economists with relevant

³⁰The mean is indicated by μ and the standard deviation is σ .

³¹Part of this modality could be explained by the high salience of assigning both 50.

research interests. Determining who to include as an expert into a sample is non-trivial, but it seems to me that Drupp et al.'s methodology is a good first approximation of the relevant population. The premise required is that those who know the consequences of adopting particular normative judgments in parameter assignments are also those who publish on discounting (in the journals considered). Obviously, this is neither conceptually nor logically true, but as an approximation, it seems defensible. It is far more helpful than Weitzman's general economics PhD population, many of whom may have little or no relevant expertise (as can be gleaned from their responses to Weitzman). Methodologies of the style of Drupp et al. could help break the deadlock in evaluating the impacts of climate—and other social—policies.

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