

ADAPTATION AND ITS MEASUREMENT

An Editorial Comment

1 Introduction

I strongly endorse the central theme of Mendelsohn's (2000) paper that 'adaptation (including its costs) must ... be taken into account in order to design efficient climate change policies'. Mendelsohn makes several valuable observations that amplify this point. He stresses that there is an efficient amount of adaptation, and that one can have too much or too little adaptation. Using the distinction of Fankhauser et al. (1997) between reactive and anticipatory adaptation, he argues that most adaptation is likely to be reactive. He also makes an important distinction between private adaptation, where benefits accrue solely to the actor making the decision, and joint adaptation, where benefits accrue to multiple individuals. He argues that the public good aspects of joint adaptation may lead to underprovision unless there is some public intervention. He cautions, however, that public intervention is not necessarily guaranteed to attain the efficient level of adaptation. These are interesting contributions to a debate within the climate change community on adaptation and its measurement which Mendelsohn himself has done much to foster.

Adaptation is something that I and my colleague Tony Fisher felt had been overlooked when we were invited in 1992 to review the existing literature on the economic impacts of climate change. We concluded that there was a need for what we called a more 'capital-oriented' view of the impacts of climate change, and we urged that

'more of the economic research be focused on the potentially very large costs of adjustment affecting stocks of physical, human and natural capital. ... [V]irtually all of the economic research that has been performed so far on the subject of climate change is conducted in terms of comparative statics. ... By contrast, the issues lying at the heart of climate change concern dynamics and disequilibrium – how long will it take for people to perceive changes in climate and respond to them? Will they refuse to acknowledge such changes when they occur, or will they quickly anticipate them? Will they adapt readily or with difficulty?' (Fisher and Hanemann, 1993).

It is certainly gratifying that these questions are now receiving sustained attention in the climate change literature.

There can be no doubt that, both as individuals and in groups, people do adapt to changes in their natural, social or economic environment. The adaptation can involve not only changes in behavior but also changes in preferences (habituation, or hedonic adaptation). In either case, the adaptation has the effect of reducing the cost – monetary or psychic – of an adverse change in their circumstances. The important questions in the climate change context are all empirical: When will people adapt? How much will they adapt? And, how much will their adaptation lower the cost of climate change? My reservations towards Mendelsohn's (2000) paper center on his (implicit) answers to these empirical questions. My concerns are centered on two core issues: the efficiency of adaptation and the question of measurement error in assessing the cost savings due to adaptation.

2. Efficient Adaptation

Mendelsohn's discussion of efficient adaptation employs a line of reasoning, very common among economists, that readily mixes normative and positive analyses as though these were equivalent. He argues that adaptation should be encouraged; that it will occur; and that it will tend to be efficient as long as no externalities or collective action are involved; therefore, it will significantly reduce the economic costs of climate change. The normative statement is that public policy should encourage efficient adaptation. The positive statement is that adaptation will tend to be efficient. I certainly agree with the normative statement. But, I have some serious reservations about the positive statement. At bottom, Mendelsohn is invoking Adam Smith's argument that the invisible hand of competition promotes efficiency. But, even if one assumes competition, rules out externalities, and grants the invisible hand as a general tendency in a competitive economy, there can be no guarantee that efficiency is being maximized *in every specific choice and on every occasion*. There is sufficient empirical evidence of inefficient behavior by firms and by individuals to refute the Panglossian notion that whatever happens is always the best.

For example, when economists Hall and Hitch (1939) in Britain, and Lester (1946, 1947) in the U.S., surveyed firms to find out whether they maximize expected profit and adjust production until marginal revenue equals marginal cost, the evidence was overwhelmingly negative. It was in response to the controversy aroused by these findings that Alchian (1950) formulated a defense of optimization based on the hypothesis of market selection: In a competitive environment, firms whose managers do not maximize profits will eventually be driven out of the market. This is probably still the view of a majority of economists, although I am not aware of empirical evidence which supports their belief. On the conceptual level, Dutta and Radner (1999) recently proved exactly the opposite result. In a model with stochastic outcomes and a need for financing to cover working capital, they show that a firm which maximizes profits may be sure to fail in finite time; and,

if there is sufficient diversity, most of the firms surviving in the long-run will be non-profit-maximizers. The key is their assumption of 'a world of uncertainty and dynamics' – an assumption that is hardly inappropriate for climate change.

The current debate on the efficiency of adaptation in the climate change literature misses the point. In my view, the real issue is not whether economic agents act optimally; it is *whether they optimize in the specific manner assumed by the analysts*. An example is the debate on 'dumb' farmers versus 'clairvoyant' farmers. The real question underlying this debate is which modeling approach best predicts the farmers' behavior when they are faced with a changed environment. Optimization is certainly part of the issue, since many of the models are rooted in explicit stories of optimization. But optimization is a complicated affair, and it is relatively easy for an analyst to mis-specify how individuals perform it. There are many possibilities – as Simon (1955) pointed out, satisficing can itself be a form of optimization. In general, what is optimal depends on a variety of considerations: What you think the choice is about; what you see as the alternative courses of action (the choice set); what are your objectives; what you perceive to be the link between the alternatives and your objectives (perceived attributes, perceived costs, etc.); and what are your constraints. It is a mistake to assume that these are readily knowable based just on external observation. They are likely to vary with individuals and, for a given individual, to vary with circumstances. Economists often attempt to solve the problem by resorting to simplifying assumptions – assuming that the choice set for everyone is the global set of alternatives ever chosen by anybody, assuming that the same attributes are relevant for everybody, disregarding perceptions and relying instead on objective measures of these attributes. However, these simplifying assumptions can produce poor predictions of how people actually behave.

An example where assumptions about optimization can be problematic in the climate change context is the use of programming models to analyze climate change impacts on agriculture. Programming models are widely used to simulate farmers' decisions regarding cropping pattern and input use. In California, modelers have to deal with the fact that farmers grow a large variety of crops – about 80 different crops are raised in the state. They typically assume that, while yields may vary, any farmer can grow any crop throughout the state. This specification of the choice set is hardly a minor issue, since it can significantly affect the results of the analysis. If one uses too large a choice set, imputing to farmers choices that they do not actually possess, this leads to an overstatement of the price elasticities of input demand and output supply and an understatement of the reduction in profit caused by an adverse change in farming conditions such as might arise from climate change. Conversely, if one unduly restricts the choice set, omitting choices that farmers actually do possess, this leads to an understatement of demand and supply elasticities and an overstatement of adverse impacts on profits. Determining the correct choice set, therefore, is crucial to the analysis. My own view, based on interviews with farmers in California and detailed analyses of their behavior, is that most farmers in the state have a substantially smaller choice set

than modelers assume. I find some corroboration for this view in the fact that the programming models which I have seen for California systematically fail to predict the cropping patterns that are actually observed; adequate predictions are obtained only by adding artificial constraints requiring the model to grow the crops that are observed. In short, what is at stake in the debate on dumb versus smart farmers is not so much the competence of farmers to run their business but rather the competence of analysts to model the farmers.

A second example concerns the *timing* of adaptation. Even if the *level* of adaptation is efficient, the *timing* could be sub-optimal, causing additional costs to be incurred. In Fisher and Hanemann (1993), we suggested that the timing of adaptation could turn out to be a key determinant of the magnitude of the economic impacts associated with climate change. We wrote:

‘some of the most important impacts of climate change arise because of the effect on capital stocks which, if not destroyed, are rendered prematurely obsolete. The costs of these effects depends critically on their timing relative to the normal replacement cycle of the affected capital. If the capital was going to be replaced anyway and the effects of climate change are well anticipated, an adjustment to climate change can be incorporated with minimum cost and disruption. If the capital was not due for replacement – or is difficult or costly to replace – then the costs are much greater’ (Fisher and Hanemann, 1993).

Mendelsohn takes a more sanguine view of the timing issue. He approvingly cites the study by Yohe et al. (1996) on the economic cost of sea-level rise:

‘By carefully building the sea walls only where needed and only when they were needed, the Yohe et al. study was able to reduce the present value of these adaptations by over an order of magnitude. For example, suppose a low sea wall (\$500,000) would protect an important set of properties from being inundated in 2030 and a high sea wall (\$2,000,000) would protect more properties from flooding by 2080. . . . The dynamic response calls for building the low sea wall just before 2030 and the high sea wall just before 2080’ (Mendelsohn, 2000).

This seems to confound the positive question of what *will* be done with the normative question of what *should* be done. It assumes that the sea wall protection will be constructed at exactly the correct moment in time – neither too soon nor too late. This strikes me as far too optimistic, especially given the uncertainties regarding climate change and the natural variability associated with storms and flooding. Recent history certainly provides examples that refute this. There are instances of *under-adaptation*, where individuals and organizations have failed to respond adequately to a change in their environment, as well as instances of *over-adaptation*, where they have over-reacted and magnified a problem out of proportion to its true significance. Therefore, it is a large leap to go from the observation that there will be *some* adaptation to the inference that there will be *perfectly efficient* adaptation. Rather than being assumed away, inefficiency in the timing of adaptation needs

to be made an object of analysis. I am reminded of a story I heard when I was growing up in England: it was said that one of the great fortunes of the Victorian era, associated with the food purveyors Coleman & Company, was made up from the mustard that people leave on their plates. By analogy, some of the costs of sea-level rise are made up from the errors associated with sub-optimal timing of sea wall construction. If the construction of sea wall protection occurs too soon, this raises the discounted present value of the construction cost; if it is constructed too late, there can be additional property damage from flooding of unprotected coastal lands. An empirical question that needs to be investigated – not assumed away – is the likely magnitude of these costs.

3. Measurement Error

The question of how much cost reduction can be expected as a consequence of adaptation by economic agents has been discussed most extensively in the context of measuring the impacts of climate change on agriculture. Two distinct approaches have emerged in this literature: An agronomic approach and a Ricardian rent approach. The agronomic approach – associated with various researchers including Adams et al. (1989, 1990, 1995), Crosson and Katz (1991), and Rosenzweig and Parry (1994) – employs agronomic analysis to predict the impact of climate change on crop yields and then uses mathematical programming models to predict the economic effects on agriculture of the changes in yields. The Ricardian approach, pioneered by Mendelsohn et al. (1994, 1996), estimates a statistical relationship between farmland values and climatic, economic and soil variables using cross-section data, and then employs the fitted relationship to predict the change in farmland value as a consequence of changes in the climatic variables. Its proponents have argued that the Ricardian approach is more likely than the agronomic approach to reflect the full range of adaptation possibilities, including not only adaptation by farmers but also market adaptation in the form of shifts in land use into or out of agriculture. They argue that, because the agronomic approach understates the extent of adaptation, it over-estimates any economic costs from climate change and under-estimates any economic benefits. These assertions appear to be supported by substantial differences in the empirical estimates reported in the literature. For example, for a specific climate change scenario based on Goddard Institute for Space Studies (GISS) general circulation model with no CO₂ effect, Adams et al. (1995) predict a positive annual impact on U.S. farm income amounting to \$10.8 billion in 1990 dollars while the equivalent prediction from Mendelsohn et al. (1994) is a positive impact of \$16.4 billion (Lewandrowski and Schimmelpfennig, 1999).

On the conceptual level, I agree that the agronomic approach can understate the full extent of adaptation and, if it does, this will bias the measurement of climate change impacts. However, I do *not* agree that the empirical difference

between the agronomic and Ricardian estimates can be taken as a reliable measure of the economic effect of adaptation, whether efficient adaptation or otherwise. The reason is measurement error. In economics or any other science, the empirical implementation of a theoretical model involves a host of ancillary decisions about data and measurement – what data are relevant, whether they should be adjusted, what estimation procedure is appropriate, what assumptions must be made in order to connect the data and estimation to the model, etc. Data never speak for themselves, without any supplementary judgment by the researcher. The outcome of the measurement reflects the full suite of the researcher's decisions, and these can all be a source of measurement error. This is a well accepted proposition in the philosophy of science, where it is known as the 'Duhem-Quine' thesis. Empirical researchers in economics are all too aware of it; economic theorists, in my experience, tend to be blissfully ignorant. This makes it naive to presume the following equations:

Agronomic approach estimates = climate change without adaptation

Ricardian approach estimates climate change with perfect adaptation

so that

Difference in estimates effect of perfect adaptation.

Instead, what we have is:

Agronomic approach estimates = climate change with less adaptation +
measurement error

Ricardian approach estimates = climate change with more adaptation +
measurement error

leading to

Difference in estimates difference in adaptation +
difference in measurement errors.

Adaptation aside, where might there be measurement error in the agronomic approach? Several factors have already been noted, including the specification of the choice set imputed to farmers by the researcher as well as the specification of the objective function and the constraints. Another potential source of error is that the programming models are calibrated to data on input requirements, input prices, demand for outputs, etc. developed for some specific time and location, and then projected to a different period of time (e.g., a model calibrated to 1990 data used to predict outcomes in 2065) and perhaps to a different location. A third factor is that the programming models generally ignore capital stocks and present

short-run decision making based on annual revenues and annual variable costs of production. Hence, the impact estimates that emerge from the agronomic approach ignore effects on the capital account in the farming sector. For these and other reasons, it is a mistake to assume that, adaptation aside, there is no measurement error in the agronomic impact estimates or that their error is necessarily in the direction of overstating damages from climate change.

What about the potential for measurement error in the Ricardian approach? This approach relies on regression analysis of cross-section data aggregated to the county level for U.S. counties in 1978 and 1982. Measurement error can arise from the data, including the county-level aggregation, the specification of the regression equation, or the method of estimation, as well as the extrapolation from 1978 or 1982 to the effects of climate change at a much later date. There is reason to believe that all of these may create some problems.

The data are what are known as *observational data* – they do not come from a randomized experiment designed to measure a particular *treatment effect*, namely the change in farmland value caused by a certain change in climate. That observational data can be problematic is well known to statisticians. In a recent paper, Rosenbaum (1999) notes: ‘In an observational study, the investigator lacks control of treatment assignments and must seek a clear comparison in other ways’, which he argues should focus on careful choices by the investigator with respect to the design of the research. If the design choices are inadequate, this will compromise the quality of the resulting evidence about effects caused by treatments: ‘Many observational studies do not succeed in providing tangible, enduring and convincing evidence about the effects caused by treatment. ... Passive observation of a natural population followed by regression analysis is often unsuccessful as an approach to inference about treatment effects’. Similarly, Freedman (1997) argues that regression analysis of observational data in the social sciences tends to be unsuccessful if the goal is to predict the results of interventions and ‘to make counterfactual inferences about the past: What would Y have been if X had been different? This use of regression to make causal inferences is the most intriguing – and the most problematic’. The difficulty in these cases arises because ‘investigators have only vague ideas as to the relevant variables and their causal order; functional forms are chosen on the basis of convenience or familiarity; serious problems of measurement are often encountered’. The outcome is application of regression in circumstances where the assumptions that underlie it are violated – errors fail to be independent or identically distributed across observations, there are unobserved variables which create problems of confounding, there is measurement error in the variables that are included, or these variables are endogenous. These hamper the ability of regression to succeed as an ‘engine for discovering causal relationships’.

There is reason to believe that these strictures may have some relevance for what are known as hedonic studies – regressions of economic variables such as property values or worker’s wages on attributes of the property or attributes of the job – a

category which includes the Ricardian model of Mendelsohn et al. (1994, 1996). These studies tend to fit the description of 'passive observation of a natural population' in a situation where economic theory provides little guidance regarding the specific functional form or the specific attributes, there are omitted variables, and measurement problems arise with variables that are included. Such problems have been documented quite extensively for what is perhaps the best known hedonic study in the environmental economics literature, a regression analysis by Harrison and Rubinfeld (1978) of the average home value in a cross section of 506 Boston area census tracts on air quality indicators and other attributes. Belseley et al. (1980) re-examined the regression residuals and detected violations of the standard regression assumptions; they then applied various methods of robust estimation and observed significant sensitivity of regression coefficients to the method of estimation. Since then, what Gilley and Pace (1996) call a 'cottage industry' has sprung up using these property value data to demonstrate alternative diagnostic techniques for regression residuals and alternative methods of robust, semiparametric or nonparametric estimation; examples include Breiman and Friedman (1985), Breiman et al. (1993), Krasker et al. (1983), Lange and Ryan (1989), Pace (1993), and McCulloch (1998).

The Harrison and Rubinfeld data share some similarities with the data used by Mendelsohn et al. (1994, 1996) – both are cross-section data from relatively heterogeneous populations with a significant degree of spatial aggregation, and both involve a strong likelihood of unobserved variables and some degree of measurement error. Mendelsohn et al. do not report any diagnostic tests on their regression residuals. Tony Fisher and I have re-examined their data and we have found violations of the standard regression assumptions, and significant sensitivity of the regression coefficients to alternative specifications and methods of estimation. Furthermore, we believe that their regression model is likely to involve some form of specification error because they combine irrigated and non-irrigated areas in a single equation that is a function only of local precipitation. Our own view is that, in areas with irrigation, the linkage between climate and farm profitability is mediated through the operation of a water storage and conveyance system. With irrigation, agricultural production in any given county and any given year no longer depends on the rainfall in that county and that year; it depends on rainfall at the distant locations where streamflow is stored, and it depends on the physical capacity and operating rules of the reservoirs. Consequently, the relevant measure of water availability to farmers in an irrigated area is not the local precipitation but rather a measure of the quantity and reliability of their water supply from the water project that serves them.

The data used by Mendelsohn et al. provide some support for this hypothesis. Precipitation is represented in their model by monthly rainfall in January, April, July and October. A consistent feature of their results is that the farmland valuation function is increasing and concave in January and April rainfall – consistent with diminishing returns to water as an input – but *not* in July and October rainfall.

In July and October, according to their fitted models, farmland value *declines* as rainfall increases until it reaches a minimum at a fairly high level of rainfall, beyond which it increases with further rainfall. For July, at least, this is an odd result because it suggests that less rain in July, the height of the growing season, is better. This is not consistent with the agronomic literature on crop evapotranspiration, nor with information I have received from farmland appraisers. Instead, I suspect it is a statistical artifact arising from an inappropriate treatment of irrigated farmland. California and Arizona have some of the most valuable farmland in the country, and they also have extremely low precipitation in July; if one fails to control correctly for irrigation, this could produce a spurious negative correlation between land value and rainfall. To test this, we re-estimated the Mendelsohn et al. model omitting all counties where more than 5% of farmland is irrigated; the result was that the anomaly disappeared and farmland value was now increasing and concave in July rainfall (Fisher et al., 1999).^{*} These findings are certainly consistent with our hypothesis that the determinants of farmland value are different in irrigated and rainfed areas.[†] Moreover, we find that they alter the estimated climate change impact on agriculture in rainfed areas in a non-trivial manner. These results will be reported in more detail elsewhere. The point I want to emphasize here is that measurement error can indeed be a consideration in both the Ricardian and the agronomic approaches. I certainly recognize that measurement error is a problem in any scientific research. However, I believe it needs to receive more attention than hitherto in the climate change literature.

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^{*} Omitting irrigated counties also eliminated another anomaly involving slope length, which is associated with increased soil loss in the Universal Soil Loss Equation. One would expect farmland value to decline with slope length, but Mendelsohn et al. consistently find that it *increases* with slope length. However, the sign of the coefficient on slope length switches to negative when irrigated counties are excluded from their sample.

[†] Mendelsohn et al. (1999) have recently attempted to control for irrigation by including the percentage of irrigated farmland as an additional variable in their model. This turned out to have little effect on the coefficient estimates and still left a negative marginal value for July rainfall. In the course of replicating their analysis, however, we found what appears to be a mistake in weighting the irrigation variable – Mendelsohn et al. employ weighted least squares regression – with the result that this variable was apparently weighted twice. When this is corrected, it changes the coefficient estimates and yields a positive marginal value for July rainfall.

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