Valuing changes in utility for non-market outcomes: Experiences alongside preferences

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ABSTRACT

The current practice in economics is to allocate resources on the basis of our preferences as they are expressed in real or hypothetical markets. Our preferences suffer from a number of problems, however, especially when we value non-market goods, such as the environment or health. An alternative way to value non-market goods is through our experiences, or subjective well-being (SWB). SWB is based on our subjective reports of we think and feel about life. This thesis shows how the values of non-market goods may differ according to whether preferences or SWB are used. There are a number of problems with using SWB as an alternative method of valuation but, overall, this thesis concludes that SWB provides a promising way of valuing non-market goods.

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1. INTRODUCTION

When deciding about legislation, policies or projects, governments and other such organisations usually determine whether the benefits outweigh the costs. This cost-benefit analysis (CBA) is central to welfare economics, so that we allocate scarce resources that provide the greatest amount of benefit, which is termed as utility. Since the influential work of the economists Pareto (1909) and Fisher (1918), the utility a person derives from a good is a result of the choice that she makes in the market. Problems arise however when the policies or projects have benefits or costs that cannot be valued by choices in the marketplace. An obvious example would be pollution (air, water, noise etc.). In an unregulated market, polluters would have no incentive to account for the damage borne by third parties.

Where markets do not exist (i.e. non-market goods, such as pollution), economists have developed methods for valuing such goods, which involve inferring a monetary value from observing people's choices. This behaviour can either be from revealed preferences (RP) or stated preferences (SP), which generates an individual's willingness-to-pay (WTP) for an intangible good.

RPs are elicited from people's choices in a range of markets such as the labour or housing market (Rosen, 1974). Many economists, however, such as Bishop (2003), argue that there are not enough revealed preference data to value the different attributes of the environment and health and other non-market goods that need to be valued. If these attributes are not valued, economists will find it difficult to implement a full CBA to legislators and policy-makers. For instance in the environmental domain, it would be difficult to set optimal pollution taxes, determine the quantity of permits for a good, and estimate natural resource damages, and other such policies. As a result, there is a potential need for SPs to value intangible goods, which inherently uses people's stated WTP. The influential National Oceanic and Atmospheric Administration (NOAA) panel (Arrow et al, 1993: 4610) have suggested that stated WTP values are "*reliable enough to be the starting point of a judicial process of damage assessment*".

RP and SP methods are expressions of what individuals want or desire at the time of the assessment in relation to different states of the world. So an individual's WTP indicates the strength of their preference for a particular good. Preferences are fundamental to microeconomic theory (Kreps, 1990), and in many, if not all, instances, economic value is ultimately derived from individual preferences (Randall, 1981). Consequently, preferences count in allocation decisions, and as preferences are satisfied, the person's well-being is also increased. There is now, however, an increasing literature that suggests that preferences may not be a good guide to individual well-being and that preferences, both revealed and stated, are not consistent with the standard model of economic behaviour (DellaVigna, 2009). This has implications for non-market valuation.

The standard model of economics stipulates that individuals compare the costs and benefits of certain actions or states of the world based upon their beliefs. Rational decision making entails: having a preference, choosing the actions that satisfy the preference, and having clear expectations about the outcomes of those actions. In this framework, preferences are said to be consistent, independent of other inferior considerations, and unaffected by framing (Kreps, 1990). There is an increasing literature however that suggests that choices are not consistent. It is argued that the way in which an option is presented and framed really matters (Kahneman and Tversky, 2000; Ariely et al, 2003), which makes choices malleable. It has been found that preferences depend on the reference point and as a result individuals are loss averse (Kahneman and Tversky, 1979). There are also issues surrounding how the choice environment impacts upon individuals' choices (Vlaev and Dolan, 2009). These problems have severe implications to non-market valuation from RPs and SPs.

Despite the problems, Hausman (1992) has argued that the satisfaction of preferences surely has something to do with well-being. It could however be argued that unless preferences bear a significant relation to utility or well-being, we many not be able to justify the postulate of normative economics (Sagoff, 1994). Many of the problems briefly mentioned above can be attributed to attention, or the lack of attention. For instance, respondents' attention in a real or hypothetical market is directed towards a limited subset of attributes and is therefore biased by a focusing effect, i.e. "*nothing is life is as important as you think it is while you are thinking about it*" (Schkade and

Kahneman, 1998; Dolan and Kahneman, 2008). Individuals place a great deal of attention on a good when they are willing to pay for it or buy it, but our attention will inevitably turn to many other things, and not just the good itself. It is arguable that we should start to think of alternative ways of measuring welfare that starts to focus on the attention that non-market goods have in our lives. That is, how does the non-market good affect the experience of our lives?

Some economists have turned their own attention to methods developed by psychologists that directly measure people's experiences, also known as subjective well-being (SWB). That is the utility associated with the experience of actual states of the world as opposed to the utility inferred from preferences over those states.

SWB is the umbrella term for the various guises of self-reported well-being, ranging from the ladder of life (Deaton et al, 2009) on the one hand to affect (Diener et al, 1999) on the other hand. The ladder of life asks individuals to place themselves on a ladder on where they rank themselves in society. Affect is based on individuals' moods and emotions at the time of assessment. Within the literature, life satisfaction is the most commonly used measure in the literature and is seen as a middle ground between the life evaluation and affect approaches. It has been found to be a measure with good internal and external validity and reliability (van Praag and Ferrer-i-Carbonell, 2004; Kreuger and Schkade, 2008). The analysis of SWB data as a measure of utility assessment is providing some interesting findings, such as the degree to which the marginal utility of income diminishes (Ferrer-i-Carbonell and Frijters, 2004; Clark et al, 2008) and how expectations affect our well-being (Graham, 2007). Some work has already been conducted to use SWB ratings to value non-market goods (e.g. van Praag and Baarsma, 2005). Nevertheless, the distinction between SWB and preferences and the potential synergies or tensions between the two concepts has yet to be fully explored.

We have little knowledge on how experiences differ from preferences for public policy. This thesis just does this for non-market valuation, which demonstrates the novelty and originality of the research. This thesis represents the first known empirical comparison between preferences and SWB for the same non-market goods from the same set of individuals, in addition to being the first research to use SWB to

causally value non-market goods in the UK. Indeed, little work has been done on using SWB for welfare appraisal in general. Furthermore, little is known of the effect that both the natural and urban environment has on SWB. This thesis proposes studies to address how the urban and natural environment, voting, terrorism, and non-market goods in general, might have an effect on SWB, how we can value such goods using SWB and how these compare to individual preferences. This thesis also represents the first finding of the causal effect of experiences on preferences, and the impact this has on individual valuation. It also shows the focussing effect of surveys on people's SWB, providing important lessons to those in all domains of social science.

The implications of this research are also potentially very important for public policy. Many governments (e.g. UK – HM Treasury, 2003) and international organisations (e.g. OECD, 1995) use preferences to determine whether a policy is efficient or not. We show that policies valued in such a way might generate different values if they were valued using people's SWB. This research is still ultimately based on efficiency in the presence of CBA. While distributional concerns are not of prime importance in this thesis, it is acknowledged that such issues are very important for the appraisal of projects (Little and Mirrlees, 1974). Additionally, procedural justice is important in public policy, but this is also beyond the scope of this research. Since the focus is on the valuation of non-market outcomes using a single metric for use in CBA, the thesis does not discuss other accounts of well-being that are not best suited for this purpose e.g. objective well-being (Sen, 1999) or psychological well-being (Deci and Ryff, 1999).

We also avoid any direct interpersonal comparisons of utility. From the preference perspective, when valuing non-market goods, the marginal utility of income is unity (HM Treasury, 2003). So we implicitly assume that, in the elicitation phase, person A's change in utility is equivalent to person B's change in utility. Only in the evaluation of projects, in a CBA sense, do we adjust for the marginal utility of income (see Pearce and Ulph, 1995). From the SWB perspective, we also assume interpersonal comparability; not only because such SWB ratings have been shown to be interpersonally comparable (van Praag and Ferrer-i-Carbonell, 2004; Dolan and Peasgood, 2008), but because we are interested in the marginal effect of the non-market good on SWB. So the identification strategy would implicitly have the control

and treatment groups compromising of similar individuals reporting similar marginal effects of the non-market good on well-being (Gruber and Mullainathan, 2002). The marginal change in SWB therefore from the treatment group is the same as the marginal change in SWB from the control group. As a result, we do not need to concern ourselves with interpersonal comparisons per se for this thesis on valuation, but we recognise some of the issues with intrapersonal comparisons in chapter 2.

Since non-market goods are a significant part of welfare appraisal, it is crucial to get the values correct, which is why this is such an important area of research. If we have biased estimates of non-market goods from preferences, we may therefore have an inefficient allocation of public resources, hence the motivation for this thesis. It has previously been argued that while there are many flaws with preferences, using such an approach is "*the best game in town*" (Pearce et al, 2006: 34) – suggesting that there was no better alternative. If the effect of non-market goods on experiences can be measured accurately however, then we might have another, and possibly more accurate, valuation method for welfare appraisal.

It is important not to underestimate the implications that such research might have for public policy. For instance, take the example of clean energy generation. When an energy company sets up an energy generation plant near people's homes, the local people may not want it near their homes (for a variety of reasons such as they do not like seeing it in their area or alternatively they predict a decline in house prices because of visual, noise or air pollution). As a result, people may express a large WTP not to have the generation site near their homes. So the energy company should recognise such WTP values and compensate local residents accordingly. Suppose, however, that people actually adapt to the generation site, and that it actually improves the community through some sort of social cohesion or identity that makes themselves and/or the place feel more vibrant. We have to question whether we have compensated the wrong individuals, since before the project they warranted compensation through their preferences but once they experienced living near the project they feel better off. Such questions are of fundamental importance to public economics and relates to normative issues, such as whose preferences count and how adaptive preferences cause some difficulties for public policy (see Elster, 1999; Menzel et al, 2005).

Ultimately, this research might allow us to say something about SWB as a method for valuing non-market goods, than is currently expressed in the HM Treasury (2003) Green Book. Given that SWB can be expressed in both monetary and non-monetary benefits, we can provide an alternative way to express the benefits from public sector interventions in a way that allows comparisons to be made across a wide range of interventions. The framework has many similarities to the quality-adjusted life year (QALY) approach that has proved successful in the health sector (Dolan et al, 2005a). The Q in the QALY is currently valued through people's preferences, but it could be easily valued by using people's SWB. If we can do this across government departments, and that such departments only care about the subjective well-being and life expectancy gains of a policy, then it will provide a much more general basis comparing interventions across different sectors.

Nevertheless, the major innovation of the thesis is that different methods are being used to value exactly the same good for the same people, and hence the valuations are directly comparable with one another. This is the novelty and uniqueness of this research. This research will further explore the feasibility of using measures of SWB to inform policy decisions surrounding non-market goods. The results generated will be of interest to any academic interested in how best to value non-market goods goods. There is little or no work in economics that has compared different methods in such a way, and, to date, the feasibility of using such measures for non-market valuation has not been fully addressed.

Whilst being grounded in economics, this research aims to draw on theoretical developments from psychology and thus contribute to collaboration and debate across the social sciences. Camerer and Loewenstein (2003) have already argued that it would be a remarkable coincidence if the human brain were built to guarantee that all types of well-being were exactly the same. The differences among these different concepts of well-being is important because a deviation between preferences and SWB means that government might provide two different policies, one to satisfying wants, and another to provide the greatest enjoyment/experience.

Structure of the thesis

There are two themes that will be underpinning the whole of the thesis and each chapter addresses at least one of these themes. Firstly, preferences and SWB might not give the same result for policy and welfare appraisal. Secondly, there may be some measurement problems in both preferences and SWB. The thesis starts off by exploring the two methods that can be used to value non-market goods, i.e. preferences and experiences (chapter 2). It provides a summary of the current methods for non-market valuation. It goes beyond just a descriptive literature review by providing up-to-date research on the problems with preferences and experiences, and how these problems translate to non-market valuation. Preferences and experiences are placed side by side so as to easily compare the two methods in the context of non-market valuation. We show that there is a great deal more to learn about the measurement of preferences and SWB, and where future research could help such measurement issues.

We will then present a study (chapter 3) of how we can use people's experiences for non-market valuation and how such values compare to the traditional preference-based approach, in an urban quasi-experiment. The experiences used in this chapter are based on people's life satisfaction and the non-market good is an urban regeneration scheme in a particular local area. This chapter illustrates that changes in utility from preferences do not equate to changes in utility from SWB. We find that the monetary values from SWB are significantly higher than that from preferences (of which stated preferences were significantly higher than revealed preferences). Several explanations for the difference between these three methods of valuation are proposed, namely: (i) stigma in revealed preferences; (ii) loss aversion the presence of mental accounting in stated preferences; and (iii) unspecified or unknown time duration in life satisfaction. Treating the effects of regeneration on life satisfaction as an annuity (i.e. a stream of money over time) moves the monetary values closer toward the preference-based monetary values.

Chapter 4 extends on the preceding chapter by examining preferences and experience in a different context. We examine a quasi-experiment of a newly constructed wind farm, where we obtain life satisfaction data before and after the construction. This

chapter provides further support for the finding that stated preferences do not equate to people's SWB. Using our own unique panel dataset, this research represents the first direct empirical comparison of preferences and SWB over time for a non-market good. We document of how the focussing effect creates a wedge between the monetary values from stated preferences and the monetary values from revealed preferences and life satisfaction. This chapter suggests that stated preferences may be prone to focussing effects.

Chapter 5 uses the same non-market good as chapter 4, i.e. the wind farm, to determine the role of experiences on individuals' stated preferences. Given the quasi-experiment, we present the first study, to our knowledge, that has examined the role of exogenous experience on stated preferences. We show that people's experience of the wind farm significantly changes their preferences, in comparison to those individuals without such experience of the good. So this chapter shows that experience can influence preferences, and this experience, we argue, is distinct from market experience, learning, and other types of information.

This chapter uses ex ante and ex post WTP measures from the same individuals. Whilst this is usually a strength in econometric analysis, there could be an issue here that individuals in the second wave already assume that the wind farm is a bad since we have asked them the WTP question previously (in the first wave). So ideally we would like to determine the role of surveying the same individuals twice or more, and how this impacts on their responses to questions in the survey. Chapter 6 does this with respect to the affective forecasting hypothesis and the longitudinal nature of SWB responses. Much of the research on affective forecasting has used the same respondents over time (e.g. see Wilson and Gilbert, 2003), so too did the previous chapter – i.e. there is an initial focussing effect in that individuals mis-predict the severity and length of time of a state of the world in the future. Although this allows us to control for individual heterogeneity, it does mean that respondents might use their first response as an anchor for their second response, either because they recall what they were asked to think about previously or because they simply recall their previous SWB rating.

There have been no studies however that have tested the degree to which having been asked about something before an event affects the assessments of well-being after the event. To do this, we need an experiment in which respondents are randomised to a standard 'before and after' condition or to an 'after only' condition, thus allowing the 'after' responses of the two groups to be compared with one another. In this chapter, we report on two experiments that were designed to test the well-being forecasts of the impact of two major European football games; the actual well-being impact of the game; and, crucially, whether having previously been asked about the game affects later well-being reports. This is different to panel conditioning since there is nothing to learn for respondents from answering two surveys on well-being (see Wilson and Howell, 2005). We show that being exposed to the initial survey biased the affective responses provided seven days later – this is the second focusing effect. That is, respondents who answered the survey twice base their second rating on their response to the first survey. The fact that respondents focussed their attention on the first survey and responded accordingly to the subtle cue, has large ramifications for using SWB responses for the same individuals within a short space of time.

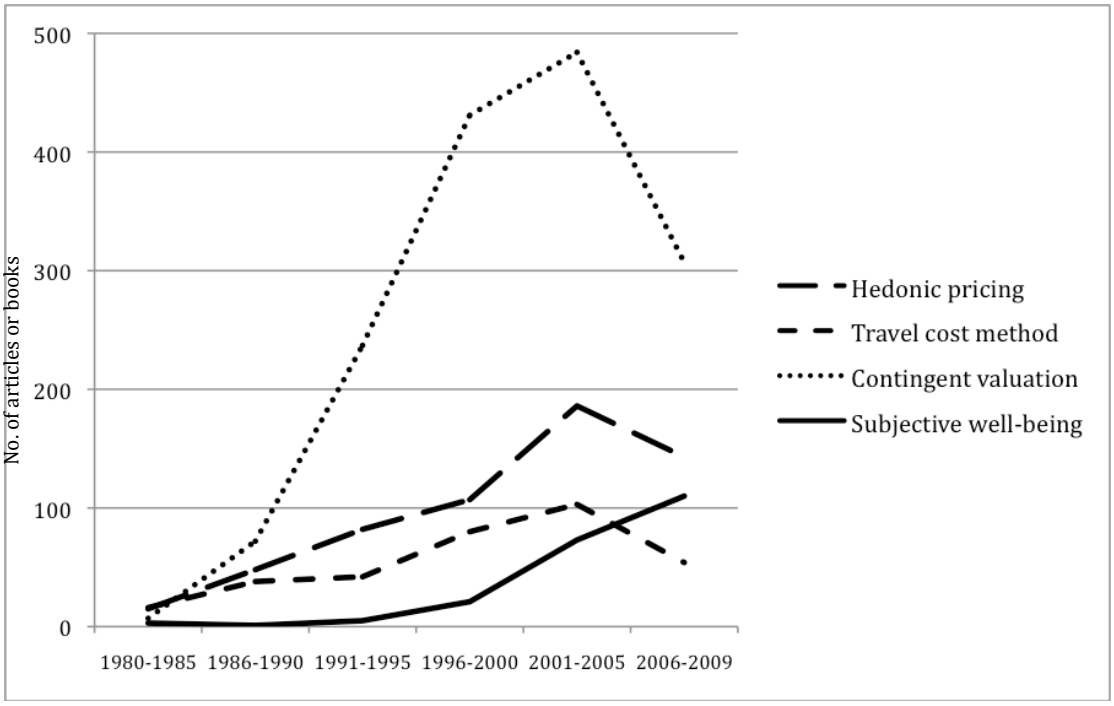
All of the chapters so far have sought to use primary data to determine how preferences differ to experiences, and how robust the methods are to focussing effect, experience and general longitudinal methods. There are secondary datasets however, that gather SWB data in the UK, i.e. the BHPS. So we can use the BHPS to shed further light on the issues already discussed in this thesis. Chapter 7 examines the affective forecasting hypothesis with respect to voting and elections. This chapter shows that voting in itself does not affect life satisfaction, and the outcome of an election does not impact on life satisfaction, even when linked to political party affiliation. This chapter demonstrates how research on SWB can be applied to voting behaviour. It seems that voters in large-scale elections succumb to the affective forecasting effect (Wilson et al, 2000). It has been found that individuals believe that the outcome of the election will affect them over a much longer period of time than it actually does. We find that individuals do not obtain any utility from voting in itself, and that voting for the winning party does not significantly increase utility.

Lastly, in chapter 8 we show that it is possible to value experiences if no life satisfaction data are present and if the non-market good is international in its

consequences. Using the General Health Questionnaire (GHQ), we show how a non-market good affects people's SWB as measured in the British Household Panel Survey, where the interviews were conducted in September 2001. The results show that the September 11th attacks resulted in higher levels of mental distress for those interviewed after September 11th compared to those interviewed before. This natural experiment shows that the magnitude of this increase in mental distress is equivalent to 0.5% of the adult British population suffering with clinical depression, which would cost between £170million and £380million in treatment expenses. This provides one of the first examples of a terrorist attack in one country having spillover effects in another country. This study also shows that it is possible to bypass the income endogeneity problem for valuing non-market goods by people's SWB, and that one can value intangible goods by other measures of SWB (beyond life satisfaction).

Overall, it must be noted that this thesis goes against what is currently being researched in non-market valuation. Economists in the realm of non-market valuation are very embedded in the preference-based approach. This is understandable given the long history that non-market valuation has within CBA and welfare economics. For instance, as can be seen in Figure 1 below, the research using preferences for valuation and public policy has been substantial since the early 1990s. The research in SWB in economics is starting to increase, as is shown in Figure 1, and it has an increasing trend compared to the other areas, which is promising for this research area. It must be noted however, that this general work in SWB is still absolutely less than what is specifically being currently published by the standard non-market valuation methods. Many economists might have traditionally felt that there was no better alternative to value such goods (Pearce et al, 2006). There is now however, at least a promising complement that economists in non-market valuation should acknowledge and potentially use to determine how scarce resources could be allocated. Work on preferences has a twenty to thirty year head start in terms of research, so more research is needed on SWB to determine its potential as a way to value non-market goods and appraise public policy. There is significant potential in examining whether what people want in terms of non-market goods also improve their experiences as denoted by their SWB.

Figure 1: Number of articles, books and working papers on non-market valuation



Note: This EconLit search was conducted on the 30th March 2009. Subjective well-being was also searched with "life satisfaction". We have not illustrated other less used revealed preference techniques such as 'cost of illness' or 'defensive expenditures', and other less used stated preference techniques such as 'choice modelling'. It is important to note that subjective well-being includes all articles in the economics field and not just those using subjective well-being for valuing non-market goods.

To conclude, this thesis demonstrates that preferences do not equate to SWB for urban and rural non-market goods. It has also shown that preferences and SWB are both prone to some biases, so further research on these areas would be fruitful. SWB measures however can be a useful welfare guide for public policy-making and might potentially be a more robust measure of what matters for policy than examining people's preferences is. It has to be noted however that the research on non-market valuation using SWB is still in its infancy and is literally thirty years behind that of generating monetary values from revealed and stated preferences (for instance, see Krutilla, 1967). So, we need more research on using SWB for economic valuation and, in so doing, we will be in a better position in the future to judge just how meaningful and robust this method actually is. We hope that this thesis has made a starting contribution to this area and we hope it will provide momentum for other researchers to conduct innovative research in this area.

2. PREFERENCE-BASED AND EXPERIENCED-BASED APPROACHES FOR VALUING NON-MARKET GOODS

2.1 Preference-based approaches

2.1.1 Introduction

Since the start of the twentieth century, economists have used people's behaviour as a way to infer utility. People's behaviour is related to their wants (Fisher, 1918), so the more they want or desire a good or service, the higher the utility it brings them. This approach became popular after Robbins (1932, 1938) and Kaldor (1939) argued that interpersonal comparisons of mental states were unscientific. Pareto (1909/1971) believed that economics could find its own laws, and make deductions from those laws, without the interference of psychology (Bruni and Sugden, 2007). This work was extended by von Neumann and Morgenstern (1944), who proposed a theory where it is possible to say something about what is 'rational behaviour' (Frechet and Borel, 1953). At the start of their book, von Neumann and Morgenstern (1944: 8) assume that

> "*the aim of all participants in the economic system, consumers as well as entrepreneurs, is money, or equivalently a single monetary commodity. This is supposed to be unrealistically divisible and substitutable, freely transferable and identical, even in the quantitative sense, with whatever "satisfaction" or "utility" is desired by each participant.*"

According to this view, agents in decisions, games, and more generally in the economy, want to maximise their monetary income. This has become a proxy for utility, and as a result, utility is equated with money, as opposed to satisfaction or happiness.

Given that money was their currency of utility, von Neumann and Morgenstern (1944) then devise a set of constraints for the preferences of agents. These constraints, or axioms, are that: (i) preference ordering always exists; (ii) if an individual prefers one option over another, she chooses that option; (iii) preference ordering of the

individual is consistent, i.e. preferences do not contradict one another (i.e. transitivity); (iv) preferences are independent of other considerations, including other options; and (v) preferences will be the same irrespective of how the options are presented (i.e. invariance). See Kreps (1990) and Mas-Colel and Whinston (1995) for a detailed review of these axioms.

The standard economic model that originates from the above work is that individuals attempt to maximise benefits and minimise costs. So individuals compare the costs and benefits of certain actions or states of the world. Rational decision making entails choosing an action given one's preferences, the actions one could take, and expectations about the outcomes of those actions. So the constraints presented by von Neumann and Morgenstern are crucially important for preference consistency and exogeniety.

These axioms result in the statement that an individual's life goes better for her if she gets what she wants, and there are no constraints on what an individual wants, and all that matters for her utility is whether a desire is met. More recent formulations of preference satisfaction require that preferences are informed in the sense that they are based on the considered use of all relevant information and some accounts exclude certain 'anti-social' preferences, such as those related to malice or envy, even when they are informed (Harsanyi, 1996). All else equal, if an individual's income increases, she is able to satisfy more of her preferences. It is not the income per se that makes her better off but, rather, the increase in choice that means she can satisfy more of her desires. With regard to non-market goods, the more income people have, the more able they are to satisfy their preferences for non-market goods, and therefore the more likely they are to pay more for such goods, ceteris paribus.

Based on these fundamentals, the use of preferences for non-market goods has taken two routes, namely revealed preferences (RPs) and stated preferences (SPs). While the early literature used RPs for valuation, a great deal of recent work has used SPs for non-market valuation. Figure 1 in Chapter 1 illustrates the dominance of this approach. Although the two methods are structurally different, their underlying concept is the same; i.e. they are based on what people want.

2.1.2 Revealed preferences

The theory of revealed preference demonstrates how one can construct utility functions from price and choice observations (Samuelson, 1938, 1948; Little, 1949; Houthakker, 1950). In order to value intangibles from revealed preferences, one has to observe real behaviour and choice in the market-place. From the observed price and choice, we can indirectly elicit the values that individuals place on the attributes of each choice. Economists assume that these revealed preferences are normative, so therefore they represent the individual's true interests. There are a number of possible methods to elicit for non-market goods, which are presented in Table 1 below – all of which are in the Treasury Greenbook (HM Treasury, 2003).

Table 1: An overview of revealed preference valuation methods

Method	Revealed behaviour	Conceptual framework	Types of application
Hedonic pricing	Property purchased or choice of employment	Demand for differentiated goods	Property value and wage models
Travel Cost	Participation in recreation activity and site chosen	Household production, weak complementarity	Recreation demand
Cost of illness	Expenditures to treat illness	Treatment costs	Morbidity

Source: Boyle (2003)

The hedonic pricing method (HPM) is the most commonly used method to value non-market goods by RPs. This method is based on decisions to take a new job or buy a house among choices that have different level of attributes. Essentially, jobs and property are heterogeneous goods whose component attributes yield utility (Rosen, 1974). Most work on the HPM has come from housing markets because the choices of housing location, and therefore neighbourhood amenities, are observable. So by observing the choices individuals make over heterogenous property with varying prices, one can estimate the implicit price of one of the component characteristics of the property (Taylor, 2003). These implicit prices produce the WTP for each characteristic.

For instance, assume H represents the differentiated house with the following characteristics $h' = h_1, h_2, h_3, ..., h_n$. The property is assumed to be sold in a perfectly competitive market, and the interactions of many of the producers and consumers together determine an equilibrium price schedule for the differentiated property, $P(h')$ – with the consumer taking the entire price schedule to be exogenous. So in the basic model, consumer utility is defined over two goods, H and x, a composite commodity representing all other goods (i.e. income after H is purchased). Consumer i, with demographic characteristics a^i, has utility defined as:

$$U_i(x, h_1, h_2, h_3, ..., h_n; a^i)$$

The individual seeks to maximise utility by choosing the differentiated property, h', and the amount of x to purchase, subject to the budget constraint, $y_i = x + P(h')$. The individual will choose h' and x such that the following is satisfied for each h_i:

$$\frac{\partial P}{\partial h_i} = \frac{\partial U / \partial h_i}{\partial U / \partial x}$$

which is the marginal rate of substitution of any housing characteristic, h_i, and the composite commodity, x. The HPM is a well-recognised method for valuing intangibles both in the UK (Cheshire and Sheppard, 2002; Gibbons and Machin, 2003) and in the US (Leggett and Bockstael, 2000). The HPM will be used in chapters 3 and 4.

The travel cost method (TCM) values intangibles used for recreational purposes in a particular location (Ward and Beal, 2000). For instance, natural areas are the focus of many recreational trips, but these natural areas do not have a market price. In order to obtain a value, the TCM assumes that travel and the recreational area are weak complements (Maler, 1974), so that the value of the recreational area can be measured with reference to values expressed in the market for trips to the recreational area (Atkinson et al, 2004). Therefore, in order to use the TCM, the researcher needs to obtain the number of trips that an individual/household takes to a particular recreational area over a specific period of time, and how much it costs that individual/household to travel to the recreational area. The latter usually includes obvious costs such as petrol expenses etc, but also the opportunity cost in allocating time to travel (e.g. ascertained by their wage rate). A specific issue or problem of the TCM is that the trip itself is usually a function of many other reasons. For instance,

the standard TCM cannot easily be applied to an international tourist on a cruise around various places, since there are multiple reasons why the tourist might choose the cruise.

These two methods are the used most frequently for non-market valuation, but there is another method that can be used. The cost of illness (COI) method estimates the resource cost associated with a change in an intangible, rather than the WTP. The COI is the sum of the direct and indirect costs associated with illness, injury, or death. The former relate to the diagnoses, treatment, rehabilitation, and support of the ill or injured persons, and the latter relate to the value of output not produced because of morbidity or premature mortality (Dickie, 2003). COI is not an individual decision alone since the decision to incur these expenditures is made by social administrators and taxpayers (Atkinson et al, 2004). We highlight this approach here since it will be used later on in Chapter 8.

2.1.3 Stated preferences

In order to elicit preferences for goods which cannot be inferred from an existing markets, stated preference (SP) techniques are used. Contingent valuation (CV) is an integral part of SP techniques, and research on this area is more than sixty years old, with the first recognised study to value non-market resources by Ciraicy-Wantrup (1947).[1] The use of CV has grown considerably since this seminal contribution, and the subject area that has become most associated with CV is environmental economics, although health and transport economists also use SPs. The main reason that these fields have used such techniques is that no market exists for many environmental and health goods, so the goods cannot be inferred from observed data. Therefore CV seems a natural way to elicit monetary values for non-market goods, which can then be used in CBA.

The impact that CV has had on public policy has been widespread, not least because of the Exxon Valdez oil spill. The Alaskan state sued Exxon for damages to the local

[1] There are also other frameworks with SP, such as choice modelling (CM). However, the dominant approach has been CV, and the arguments presented can also be assumed to also be representative of CM. For a further discussion of CM techniques, see Louviere *et al* (2000).

environment, and the state settled for compensation of $1billion. This damage settlement was supported by a CV study of the area (i.e. Carson et al, 1992). The size of this settlement spurred the academic community to determine whether the CV approach was a valid and robust scientific method. The main paper that spawned from this debate came from the Blue Ribbon Panel (i.e. Arrow et al, 1993), which was assembled by the National Oceanic and Atmospheric Administration (NOAA). Even though the main premise of the Panel was to evaluate CV in terms of eliciting non-use values, the main benefit of this work was deriving rules of best practice for CV studies. Since the Blue Ribbon paper, CV studies have grown at an increasing rate, and the CV approach has been applied to many diverse non-market goods.

CV studies have become popular within the UK, with practical guidelines provided by Bateman et al (2002). The UK HM Treasury (2003) endorses the use of SP techniques for valuing non-market goods, and it is also a popular method for US government policy (Boardman et al, 2008). Since 1993, three basic sections to a CV have evolved. Within the first section, a set of attitudinal and behavioural questions are asked regarding the good to be valued, so as to reveal the most important underlying factors driving respondents' attitudes towards the public good. This is in place to confirm Ajzen's (1991) model of planned behaviour, where our attitudes and beliefs shape our intentions which ultimately leads to our choices. Secondly, the contingent scenario is presented which includes the hypothetical scenario, a description of the constructed market, and the method of payment. Thirdly, the socio-economic and demographic characteristics of the respondent are asked. This is required in order to ascertain the representativeness of the survey sample relative to the population of interest, to examine the similarity of the groups receiving different versions of the questionnaire, and to study how WTP varies according to respondents' characteristics (Pearce *et al*, 2006). This further allows for benefit transfer (see Ready et al, 2004).

The second section of the CV survey is where most debate occurs. Therefore it is worthwhile analysing why this aspect of the CV questionnaire is problematic. The hypothetical scenario presented usually has a description of attributes of the good. Boardman et al (2008) and Pearce et al (2006) argue that good practice for CV involves including both a description of the available substitutes for the good and a description of the proposed policy change and how the attributes of the good will

change accordingly (i.e. textual, photographs, drawings etc), although there is no consensus or rules for doing so. Where there are multiple aspects to the good, one should employ a top-down approach eliciting the value of the whole good first before valuing the subsequent parts.

The description of the hypothetical constructed market is also critical to the design of a CV study. There are three main elements that deserve careful attention, namely the institution providing the good, the conditions for the provision, and the timing of the provision. The institution providing the good can be central government or local company etc, but this might ultimately affect a person's WTP of a good. A similar effect might happen for the conditions of the provision, as well as the timing of the provision – we will return to these issues later on in this chapter.

For the valuation of a non-market good, the researcher has an important choice between using WTP and WTA. To examine this further, it is beneficial to start with the preference function for an individual (in a similar vein to Just et al, 1982; Freeman, 1993; and Haab and McConnell, 2003), i.e. $u(x, q)$, where x is a vector of private goods and q is a vector of public goods. The difference between x and q is that the individual chooses their quantity of x, not q, which is perceived as being exogenous. The individual maximises utility subject to income, y, so the indirect utility function $V(p, q, y)$ becomes:

$$V(p, q, y) = \max_{x} \{u(x, q) \mid p.x \leq y\} \qquad (1)$$

The minimum expenditure function $m(p,q,u)$ is identical to the indirect utility function:

$$m(p, q, u) = \min_{x} \{p.x \mid u(x, q) \geq u\} \qquad (2)$$

The derivative of eq. (2) with respect to price gives the Hicksian or utility-constant demand. Therefore CV is a way of estimating the change in the expenditure function or the change in the indirect utility function.

Within this there are two ways of illustrating monetary measures, namely compensating and equivalent variation. WTP is the maximum amount a person will pay in exchange for an improvement in circumstances, or the maximum amount a person will pay to avoid a decline in circumstances. WTA is the minimum amount a person will accept for a decline in circumstances, or the minimum amount a person will forego an improvement in circumstances. Compensating variation is the amount of income paid or received that leaves the person at the initial level of utility, and equivalent variation is the amount of income paid or received that leaves the person at the final level of utility.

From this, equivalent and compensating variation rely on the initial versus final utility for their distinction. Equivalent variation is the WTA for a situation where utility is improved and the WTP for a situation where utility declines. Compensating variation can be described as when the final utility is worse than the initial utility level, it is WTA, but when the final utility level is better than the initial utility level, it is WTP. Table 1 below illustrates how these ideas are inextricably linked together. We present these here since they are important to Chapter 5.

Table 1. The connections between WTP and WTA, and compensating and equivalent variation.

	Equivalent variation	**Compensating variation**
Utility increases	WTA	WTP
Utility decreases	WTP	WTA

For an individual, the WTP value is the amount of income needed to compensate an increase in the public good:

$$V(p, q_1, y - WTP) = V(p, q_0, y) \qquad (3)$$

where $q_1 \geq q_0$ and increases in q are seem as desirable (i.e. $\partial V / \partial q > 0$). The WTA value is the change in income that makes an individual indifferent to accepting one of two situations, where the original level of the public good (q_0) and income at $y + WTA$

is one situation, and the new level of the public good (q_1) with income at y the other situation:

$$V(p, q_0, y + WTA) = V(p, q_1, y) \quad (4)$$

Therefore, $WTA \geq 0$ when $q_1 \geq q_0$. The definitions of WTP and WTA correspond to the positive parts of the Hicksian measures, i.e. WTP with equivalent variation and WTA with compensating variation.

While there is a theoretical distinction between WTP and WTA, most CV studies use WTP because the NOAA guidelines recommended this estimate since it is a conservative estimate, and it can flexible enough to include negative values (e.g. Atkinson et al, 2004). Whether the researcher chooses WTP or WTA should depend on the actual context of the policy and on who has the property rights to both the initial and new levels of the public good. Pearce *et al* (2006) note that determining property rights is not straightforward in practice, e.g. the EU Water Framework Directive, although table 1 can be adapted to include property rights (table 2). Section 2.3 will discuss the possible differences in WTP and WTP empirically.

Table 2: The connection between property rights and table 1.

Property rights	**Society worse off: quantity decrease**	**Society better off: quantity increase**
Right to the status quo	WTA to tolerate loss	WTP to secure gain
Right to a new situation	WTP to avoid loss	WTA to forego the benefit

Note: this table assumes that a quantity increase is good for welfare. Source: adapted from Pearce et al (2006: 141).

The part of the CV method which has drawn the most attention is which elicitation mechanism is used. There are four main mechanisms that one can use namely: open ended (OE); bidding game (BG); payment cards (PC); and dichotomous choice (DC) (Bateman et al, 2002) The OE CV question asks the individual to give a point estimate of their WTP. The BG CV method is based on an iterative method for an individual's WTP. Within this, the individual is asked whether they would be WTP a certain amount and the WTP amounts are increased or decreased depending on the

previous answer, with the iterative process stopping when the individual converges on an estimate. The PC method asks the individuals to choose a WTP estimate from predetermined list values on a card shown to the individual. The DC method purely requires a binary yes or no answer to whether an individual would be WTP a certain amount for a good.

From these four, it seems that DC has become the preferred choice over the last decade (Pearce et al, 2006). The main reason for this is that the first three seem to be less efficient as they lack incentive compatibility, in that individuals can influence potential outcomes by revealing values other than their true WTP (Haab and McConnell, 2002). Within DC, a researcher could use single bounded DC or multiple bounds. A single bounded DC CV method would ask one question only. A double-bounded DC CV method would ask whether they would be willing to pay £x, and if it is accepted (rejected), the researcher would ask whether they would be willing to pay £y (£z), where $z < x < y$. The double bounded DC is thought to be superior to the single bound since more information is gathered than in the single bound method. There is also a possibility of using triple-bounded DC, which generates even more information. The problem with both double- and triple-bounded DC however is the loss of incentive compatibility due to the fact that the second or third questions may not be viewed by respondents as exogenous to the choice situation (Bateman et al, 2001).

An alternative one-and-a-half bounded DC has been put forward by Cooper and Hanemann (1995) and Cooper et al (2002). Within this, respondents are initially informed that costs of providing the good in question will be between £x and £y (where $x<y$), with the amounts x and y being varied across the sample. Respondents may be asked whether they are prepared to pay the lower amount x. If the response is negative no further questions are asked: if the response is positive then respondents are asked if they would pay amount y. Conversely respondents may be presented with the upper amount y initially and asked about amount x if the former is refused. This method might be more efficient than other bounds and less prone to any bias. Many of the larger studies use a DC approach. It must be recognised that most researchers are uncertain as to whether the PC method as opposed to the DC method should be used.

The former gives cleaner estimates if the sample size is small, which is true of many local provision of non-market goods.

We will use a local and recent example of how WTP from the SP method can be applied in practice. The Summer Olympic Games can be considered a non-market good in terms of the benefit it brings to the local area for hosting the event. An Olympics may potentially provide the impetus for environmental and economic regeneration in the host city, as well as a boost to national pride and so on. Attempts to quantify the benefits of hosting major sporting events have focused exclusively on tangible gains (Lybbert and Thilmany, 2000; Hotchkiss et al., 2003; Rose and Spiegel, 2009) with a clearly identifiable cash-flow. In fact, from these studies however, it is unclear whether hosting the Games is actually worth the cost.

As the tangible benefits are low, we need to consider whether the non-market benefits are any better. Using a CV survey it is possible to establish the size of the non-market benefits of the London 2012 Bid, which might help justify staging the event on cost-benefit grounds. Atkinson et al (2008) asked residents from London, Manchester and Glasgow for their WTP to host the 2012 Olympic Games in London on the basis of the non-market impacts that this event might provide. They found that respondents' WTP in London, Manchester and Glasgow was £22, £12 and £11 respectively in extra council tax payments. So, for the six million Londoners, the non-market benefits can be aggregated as £132 million.

2.2 Experience-based approaches

2.2.1 Introduction

The previous section presents two ways of valuing non-market goods. Both of these methods are ultimately based on well-being as a function of income, or the satisfaction of preferences. There is however another way of documenting well-being, and that is through people's happiness (Parfit, 1984).[2] Before the work of Fischer (1918), Robbins, and van Neumann and Morgenstern (1944), mental states were of

[2] We acknowledge that there is another way of documenting well-being, that is through people's objective well-being, but we do not focus on this in this thesis since it is a method that cannot readily be applied to welfare appraisal.

central importance to economics. The works of both Jeremy Bentham and John Stuart Mill demonstrated this. Bentham believed in the 'greatest happiness principle', and succulently argued that "*(n)ature has placed mankind under the governance of two sovereign masters, pain and pleasure. It is for them alone to point out what we ought to do, as well as to determine what we shall do*" (Bentham, 1823: 1). This influential argument turned into a procedure for estimating the moral status of any action, which was called the 'hedonistic calculus'. Bentham stated that pleasures and pains can be ranked according to their value, such as intensity, duration, certainty of a pleasure or a pain, which results in the aggregation of happiness by pleasure pain (Spiegel, 1991).

Mill (1863) continued Bentham's line of thought but believed that there was a need for a qualitative separation of pleasures. While Bentham treated all forms of happiness as equal, Mill argued that intellectual and moral pleasures are superior to more physical forms of pleasure. Nonetheless, Mill's work was important for not only furthering some of the work in economics using the hedonic calculator, but also for developing an understanding of the issue of negative spill-overs to other people. Mills' (1859) 'harm' principle supports the freedom of the individual, so that each individual has the right to act as he wants, so long as these actions do not harm others. If the action only directly affects the person undertaking the action, then society has no right to intervene, even if it feels as though the actor is harming himself. Society only has the right to intervene if the actions harm the well-being of other individuals. Given that Mill was primarily concerned with happiness as a measure of well-being, we could argue that what Mill had in mind was that harm to people can be calculated by examining their happiness. This therefore provides the first incidence of an economist to argue that negative externalities, such as pollution or other such non-market goods, should be valued in terms of people's happiness.

This was taken further by the English marginalists, such as Jevons (1871/1970), Edgeworth (1881/1967) and Pantaleoni (1889/1898), to reduce Benthamite utility to a one-dimensional measurement scale of the individual perception of pleasure and pain (Cohen, 1997). Such efforts are characterised by their 'hedonimeter'. Edgeworth posited that the development of a hedonimeter that would measure utility directly, which he felt was necessary for the extension of utility analysis to the real world. Hence, this work connected the mathematical complexity of human behaviour with

the utility of utilitarianism. The Paretian turn however seemed to end this close link between economics and psychology, and little dialogue took place between the two disciplines during the 1950s and 1960s – apart from Simon (1955). Nevertheless, this dialogue did re-open between the two disciplines at the start of the 1970s. The meeting of Daniel Kahneman and Amos Tversky was pivotal for this revival.

The contribution of the Kahneman and Tversky work was two fold.[3] Firstly, they placed the psychology of decision theory to real world situations to reflect real uncertainty – this will be documented in section 2.3. Secondly, they made the distinction clearer between the normative solution to a problem and the subjective answer given by real-world individuals. In one of their seminal papers, Kahneman and Tversky (1979) argued that because real world behaviour of individuals deviates from the standard economic model, the academic community should move to a descriptive theory, where utility depends on the individual's subjective perception.

Kahneman and Ritov (1994) and Kahneman et al (1997) have showed that there is a conceptual difference between preferences and experiences. Within the latter concept, we depart from von Neumann and Morgenstern's money equating utility theory, to examine aspects of people's lives that are subjectively perceived by themselves. The situation in economics is succinctly summarised by Wansbeek and Kapteyn (1983: 249):

> "*Utility seems to be to economists what the Lord is to theologians. Economists talk abut utility all the time, but do not seem to have hope of ever observing it this side of heaven. In micro-economic theory, almost every model is built on utility functions of some kind. In empirical work little attempt is made to measure this all-pervasive concept. The concept is considered to be so esoteric as to defy direct measurement by mortals. Still, in a different role, viz., of non-economists, the same mortals are the sole possessors of utility functions and can do incredible things with it".*

Since the 1990s, economists have begun to recognise that measuring experiences (or utility) is one of the key emerging research themes within economics and the work from psychology enabled experiences to be measured. Table 1 in chapter 1 illustrates the increasing popularity with using SWB in economics.

[3] Main contributions were Kahneman and Tversky (1972, 1973, 1979) and Tversky and Kahneman (1971, 1973, 1974).

There seems to be two key reasons as to why economists, especially in the field of non-market valuation and public policy, would want to define utility in terms of experiences. Firstly, preferences and experiences are different concepts of well-being and as a result, what we prefer might not be the same as to what we enjoy best. There is increasing evidence that individuals are unable to choose the greatest amount of well-being for themselves, and individuals might want goods which either decrease their well-being , or does not increase well-being as much as a different choice would have provided – i.e. 'mis-wanting' (Wilson and Gilbert, 2003). Secondly, given the vast amount of research in psychology, experiences can now be measured with a degree of accuracy (Frey and Stutzer, 2002; Graham, 2007). These two reasons are clearly very important for economists in non-market valuation since the government (or other such organisations) might actually allocate scarce public resources to non-market goods that individuals might want but which might reduce the quality of their experiences over time.

2.2.2 Subjective well-being

The most used measure for experiences is the life evaluation account. Within Sumner's (1996) evaluative account, an individual's life goes well for her if she has a positive attitude, in terms of affirmation or endorsement, towards her life, which has both a cognitive and an affective component. The cognitive aspect is an evaluation of the conditions of an individual's life, and the affective component is an evaluation of how enriching or rewarding their life is, and whether they feel satisfied and/or fulfilled. The affective component of the evaluative account is not a measure of utility in the Benthamite hedonist sense, but feelings of contentment and satisfaction. Clearly, pleasure and pain will contribute to both aspects of this account, but it allows the person to decide how much pleasure and pain matter for their overall evaluation of their lives.

A key element of the cognitive aspect is how well we have met our expectations, which suggests that a life evaluation is broader than the utility that Bentham had in mind. Essentially, it allows the individual to choose how important affect is in comparison to them meeting their expectations in the future and how they have met them in the past. So it could be construed as being a less restrictive concept of

subjective well-being than hedonic calculus/utility. How often people make such an evaluation is uncertain, but when individuals are not evaluating their life, the underlying components of their evaluation (e.g. their preferences, attitudes, feelings) exist and therefore a hypothetical evaluation of that person's life may be made (Sumner, 1996). So an individual's well-being is determined by factors they would use if they were to evaluate themselves at that time.

Psychologists have used such an evaluation that provides both a cognitive and affective element. Diener et al (2003) and Diener and Seligman (2004) present well-being as peoples' positive evaluations of their lives, which inherently includes affect, engagement, satisfaction, and meaning. As a result, SWB is an individual's assessment of their life, which encompasses how an individual thinks and feels about their life (Diener et al, 1999). Therefore, the SWB approach reflects the weights attached to their hedonic experience alongside their assessment of how well life is going for them more generally (Dolan et al, 2008). This is clearly different to the hedonic calculus approach (see Kahneman et al, 2004; White and Dolan, 2008). This life evaluation method was initiated in economics by Easterlin (1974), who used a 'Satisfaction with Life Scale', where statements such as "I am satisfied with my life" and "in most ways my life is close to ideal" are rated on a one to seven scale, ranging from "strongly disagree" to "strongly agree". There are various ways of measuring SWB through surveys, but most studies do use the Likert scale, which goes from zero to ten (e.g. Clark and Oswald, 1994; Blanchflower, 2004; and Van Praag and Ferrer-i-Carbonell, 2004).

To sum up, SWB has a both a cognitive element, in that it determines whether a person's aspirations have been met, and an affective element, in that it determines the extent to which one finds one's life to be enriching and feels satisfied and fulfilled by it. SWB allows each individual to freely choose how important each dimension is to her own well-being (Sumner, 1996), which actually suggests that SWB is quite an autonomous measure of welfare. It is not easy to formalise SWB, but economists have started to determine what a SWB function would look like. A standard SWB function could be approximated to:

$$SWB = U(Z,t) + \varepsilon \quad (5)$$

where *SWB* is subjective well-being, which takes on the value from zero to ten, and *U*(…) is the observable utility or well-being, but the exact structure is hidden from the interviewer. The *Z* vector includes a set of determinants of SWB, and *t* represents the relationship between the *Z* vector and well-being over time. The residual ε represents the noise influencing the relationship between actual and reported well-being, such as the cognitive inability to accurately communicate their well-being levels in a survey or in an interview.

In order to use SWB as a measure of utility, there needs to be validity and reliability in the actual measure. Sandvik et al (1993) have showed that individuals are able to recognise and predict the satisfaction of others, suggesting that SWB is observable and comparable between individuals. Furthermore, van Praag (1991) has argued that individuals can easily translate verbal labels, e.g. very good, in to roughly the same numerical values. Sandvik et al (1993) and Shizgal (1999) have demonstrated that there is a strong positive correlation between SWB ratings and emotions such as smiling and frowning. The research on Duchenne smiles (i.e. a type of smiling that involves a muscle near the eye called orbicularis oculi, pars laterali, which can distinguish between true and feigned enjoyment) are correlated with SWB (Ekman et al, 1988; 1990).

There has also been neurological evidence as to the extent of the validity of SWB responses. For example, Urry et al (2004) have shown, using Electro-encephalography, that left-right brain asymmetry is shown to be associated with higher levels of positive affect and SWB.[4] This left-right asymmetry is also associated with quicker recovery from negative affect shocks, and also to cortisol levels, which are related to responses to stress (Clow, 2006). While this brain asymmetry has something to do with genetics, both Davidson (2004) and Huppert (2006) argue that early social experiences have a significant effect, and that it is possible to manipulate

[4] Davidson (2004) argued that in right handed people, positive feelings are associated with more brain wave activity in the left prefrontal cortex, whereas negative feelings are more associated with the right prefrontal cortex; hence the importance of left-right brain asymmetry.

the asymmetry in adults using magnetic fields to stimulate activity in the left frontal portion of the brain.

Another strand of literature suggests that SWB at a particular time can predict future behaviour. Therefore, individuals might know what activities decrease their well-being and these activities are gradually discontinued over time (Kahneman et al, 1993; Frijters, 2000). Freeman (1978) and Clark et al (1998) have shown that satisfaction in an individual's job is a strong predictor of their resignation, and that lower SWB predicts future marital break-up (Gardner and Oswald, 2006). Evidence from Di Tella et al (2003) found that rises and falls in suicide rates move in the opposite direction to changes in SWB, and Bray and Gunnell (2006) showed that there was a negative correlation between SWB and suicide. Guven (2007) has found that higher SWB: increases savings; reduces risky behaviour; impacts on future expectations; reduces the likelihood to smoke; and reduces the likelihood to migrate to another country. The latter is also implied in Blanchflower and Shadforth (2009).

Furthermore, illnesses such as heart disease (Sales and House, 1971), strokes (Huppert, 2006) and hypertension (Steptoe and Wardle, 2005; Blanchflower and Oswald, 2008) are highly correlated with SWB. Cohen et al (2003) found that people with higher SWB were less likely to catch a cold when exposed to the cold virus, and recovered faster if they did catch a cold, than those with lower SWB.

In terms of statistical reliability, single-item SWB measures have been found to have moderate reliabilities, usually between .40 and .66, even when asked twice in the same session one hour apart (Andrews and Whithey, 1976). Kammann and Flett (1983) found that single-item well-being questions under the instructions to consider "the past few weeks" or "these days" had reliabilities of .50 to .55 when asked within the same day. Krueger and Schkade (2008) improved on the earlier studies by analyzing the reliability of a standard self-reported life satisfaction measure and of affect measures collected from a diary method. Over a two-week period, the correlation of net affect (i.e., duration-weighted positive feelings less negative feelings) measured two weeks apart is 0.64, which is slightly higher than the correlation of life satisfaction ($r = 0.59$). They argue that a potential reason for the modest reliability of SWB measures compared with education and income, which

typically have reliability ratios of around 0.90, is the susceptibility of SWB questions to transient mood effects.

In addition to this, Lyubomirsky et al (2005) provide a meta-analysis on the work examining the link between positive affect and other behaviours or skills. They found consistent evidence that positive affect increases: sociability and activity; altruism; liking of self and others; the immune system; and effective conflict resolution strategies. They also found, but to a weaker extent, that positive affect increases original thinking. As a result, higher SWB is an important determinant to become a productive member of society and the economy.

Moving to the other direction of the relationship, from the *Z* vector in equation (3.1), Dolan et al (2008a) argue that there are five major determinants of SWB: (i) income (absolute or relative – social and temporal); (ii) health (especially psychological health); (iii) employment (unemployed; long time sick; carer); (iv) personal relationships (divorced or separated; living alone); and (v) social capital (e.g. talking frequently to neighbours). Regarding how income is a determinant of SWB, there has been much debate in the literature as to whether absolute income is as important as relative income.[5] Easterlin (2001) has found that individual income has a significant effect on SWB, and is at is greatest strength amongst low-income groups (Biswas-Diener and Diener, 2001), and lowest strength for higher income groups (Cummins et al, 2004). However, absolute income does not always increase SWB (e.g. Wildman and Jones, 2002), and there are problems with causality of well-being, i.e. higher initial levels of well-being might ultimately lead to higher levels of income (Graham et al, 2004). This has implications to deriving monetary values for SWB, which we will highlight later on in chapters 3 and 8.

Relative income is perceived as being a key determinant of well-being. Luttmer (2005), Ferrer-i-Carbonell (2005) and Blanchflower and Oswald (2004) find that relative income significantly affects SWB, and Easterlin (2001) believes that this dominates the absolute income effect. McBride (2001) however believes that the relative income effects is less at lower income levels, where absolute income matters

[5] The literature review by Clark et al (2008a) devote the whole paper to just this issue.

much more, but Fafchamps and Shilpi (2006) have found that poor households in Nepal do not care less about relative consumption than high income households. There is a debate not only on the effect of relative income, but also on how relative income should be measured. The Hirschman tunnel effect, i.e. other people's incomes impact on my well-being positively because I expect my income to increase in the future, might be prominent for some individuals or some societies. Clark et al (2009) finds similar effects for pay in the labour market. At this time, we do not have a very clear idea of the causal effects of absolute and relative income on SWB.

If there is a clear causal effect of an event on SWB, we can start to value such events for public policy. So we can place a value on public non-market goods which can feed in to CBA. This would then also allow us to compare such values with people's revealed and stated preferences, which has not been previously done for non-market goods. The way that SWB can be used to elicit monetary values has been provided by Baarsma (2000), Clark and Oswald (2002), Frey et al (2004) and van Praag and Baarsma (2005), essentially using the compensating differential approach from labour economics.

In order to value a non-market good, we need to firstly specify the SWB function within an ordinary least squares, ordered logit or ordered probit regression:[6]

$$SWB_i = \beta_0 + \beta_1 \ln(Y)_i + \beta_2 N_i + \beta_3 X' + \varepsilon_i \qquad (6)$$

where SWB_i is individual i's SWB, $\ln(Y)_i$ is the natural logarithm of household income, N_i is the non-market good, X' are the personal and social characteristics, and ε_i is the standard error term. With the estimated coefficients of the micro-econometric SWB function for the non-market good ($\hat{\beta}_2$) and household income ($\hat{\beta}_1$), we can calculate the shadow price of the non-market good or implicit utility-constant trade-offs between the good and income. We measure the shadow price by the income compensation (IC), i.e. the decrease in income necessary to hold utility constant if the

[6] See Ferrer-i-Carbonell and Frijters (2005) and Van Praag and Ferrer-i-Carbonell (2004) for the costs and benefits of using each framework.

quality/quantity of the non-market increases. In an indirect utility function, this would be given by:

$$v(N_0, y_0 + IC) = v(N_1, y_0) \tag{7}$$

where $v(.)$ is the indirect utility function, y_i is the initial household income, N_0 is the inferior non-market good and N_1 is the superior non-market good. Given the specification of the micro-econometric SWB function expressed in equation (3.2) the position of IC in the indirect utility function, IC can be defined as:

$$IC = e^{\left(\frac{\hat{\beta}_2(N_1 - N_0)}{\hat{\beta}_1} + \ln(y_0)\right)} - \overline{y}_0 \tag{8}$$

where $\overline{y}_i$ is average household income of the sample population. This is derived by using equation (7) from the regression (6) to form:

$$\hat{\beta}_2 N_0 + \hat{\beta}_1 \ln(y_0 + IC) = \hat{\beta}_2 N_1 + \hat{\beta}_1 \ln(y_0) \tag{9}$$

hence:

$$\ln(y_0 + IC) = \frac{\hat{\beta}_2(N_1 - N_0)}{\hat{\beta}_1} + \ln(y_0) \tag{10}$$

Therefore, this IC generates an income compensation of the non-market good that can then be used in CBA.

The seminal work to date that uses SWB to elicit the value of a non-market good is that of van Praag and Baarsma (2005). They examine residents located within an area around Amsterdam airport, and use a life evaluation question, i.e. the ladder of life – which has recently been used by the Gallup world poll (Deaton et al, 2009). They find that actual objective noise is not significant in explaining the SWB for these residents. This is not surprising since one would expect that those who have an indifference to a

certain level of noise would locate themselves near the airport, i.e. some aspect of residential sorting. Within their regressions however, once they included a subjective perception of noise, this term became negative and significant.

We shall assume that the estimates from their study are causal just to demonstrate how they value noise pollution even though they are prone to a reporting bias. Their value of noise was not linear in noise level, since they found that an increase in noise from 20Ku to 30Ku is equivalent to the change from 30 Ku to 45Ku in SWB terms. From their results, an increase of noise from 20Ku to 30Ku should be compensated by around €34 for a monthly net income of €1,500, and from 20Ku to 40Ku should be compensated around €57. Given that they calculated that the monthly value of insulation was €22 (on housing rent), it is therefore more beneficial to pay the compensation to individuals rather than to insulate every house in the vicinity of the airport.

As a result, using people's SWB to value non-market goods seems a promising avenue for research. If the causal estimates are found for the non-market good, we can obtain the marginal rate of substitution of income for the non-market good, and this then can feed into CBA and public policy more generally. There is therefore a large potential in this method in economics, but how this method compares to preferences is a largely untouched area. But before we can start to think about such possibilities, we need to set out some of the issues of valuing non-market goods by using people's preferences and experiences.

2.3 Problems

2.3.1 Consistency

2.3.1.1 Are preferences consistent?

The premise of WTP is based upon the theory that individuals make choices so as to maximize a utility function, using all the information available, and processing this information appropriately. In this rational model, individuals' preferences over all bundles of consumption of private and public goods are assumed to be time-

consistent, affected only by own payoffs, and independent of the framing of the decision. Therefore, once one asks a sample their WTP for a good, it is seen as being logical and correct to derive a demand function for the whole population from this sample. There is a growing list of papers in psychology and behavioral economics however that document aspects of behavior in market settings that also deviate from the forecasts of the standard theory (DellaVigna, 2009).

One of the main problems of WTP is in terms of risk preferences. The study that really demonstrated the problem of risk preferences for the standard model is Kahneman and Tversky (1979). They propose a reference-dependent model of preferences that, unlike the standard model, fit a great deal of the experimental evidence on risky choice. Their 'prospect theory' stipulated that choices consist of two stages, editing and evaluation. In the editing stage, all possible outcomes of the decision are ordered following some heuristic, so individuals decide which outcomes they see as basically identical and they set a reference point and consider lower outcomes as losses and larger as gains. In the evaluation phase, individuals then behave as if they would compute a value based on the potential outcomes and their respective probabilities. Due to the reference point, the theory measures gains and losses and not absolute wealth, so it is the change in wealth that becomes important not the wealth in its absolute form. They formalize this mathematically to argue that there is a bigger impact of losses than of gains for the same absolute value, which has been called 'loss aversion'.

The above work makes an explicit reference to how the reference point is shaped by the endowment effect - which results in an asymmetry between WTP and WTA. In a laboratory experiment, Kahneman et al (1990) randomly allocated mugs to one group of subjects. They used an incentive-compatible procedure to elicit the WTA for subjects that received the mug, and the WTP for subjects that were not allocated the mug. According to the standard theory, the two valuations should on average be the same. The median WTA of $5.75, however, was twice as large as the median WTP of $2.25. They found that wealth effects did not explain this discrepancy.

In practice, it is very difficult to find any studies where $WTP \approx WTA$. Horowitz and McConnell (2002) conduct a meta-analysis of forty-five usable non-market studies

where both WTA and WTP are reported. They argued that the discrepancy between the two values was higher the further away the good being valued is from being an ordinary private good. They conclude that this discrepancy between WTP and WTA is not due to strategic and hypothetical bias, or either incentive incompatibility.

The psychological mechanisms noted above by Kahneman and Tversy have prompted economists to provide three theoretical reasons for this problem. Firstly, an income effect is very important, i.e. real income changes in a different way according to whether the individual has to pay or receives compensation. Willing (1976) however has argued that the discrepancy must be small since it should not matter whether one adopts the Marshallian measure of surplus or a measure based on compensated demand curves. He further elaborates that any error in using the Marshallian measure of surplus is proportional to the income elasticity of demand and consumer surplus as a fraction of income. Therefore, the errors should be small (i.e. a few percentage points), hence the income effect cannot be solely responsible for the disparity between WTP and WTA.

Secondly, substitution effects are believed to be responsible for the disparity (Hanemann, 1991, 1999; Carson et al, 2001). In the environmental context, there are little substitutes, so very large levels of compensation are needed for environmental goods. Carson et al (2001) argue that in this context, both income and substitution effects are present and the ratio of WTP to WTA depends on the ratio of the income effect to the substitution effect, where the latter is very small. But the results from Kahneman and others document that such discrepancies exist for goods with many substitutes.

Thirdly, endowment effects have become very prominent in explaining the disparity. An endowment point, i.e. a good (or bundle of) already owned by an individual, has more psychological salience if it is lost rather than if an equal good is obtained (i.e. loss aversion) (Tversky and Kahneman, 1991). Therefore, losses are valued more than gains, and the valuation function exhibits diminishing marginal valuation the further away from the reference points one gets (Knetsch, 1989). Although this would obviously cause a large discrepancy in the WTP and WTA values, Shogren et al (1994) argued that whether endowment effects are prominent depend upon

substitution effects. They found that WTP and WTA converge when there are close substitutes, so that the endowment effect does not show up regardless of the degree of substitutability. Knetsch and Sinden (1984) and Morrison (1997) both however argued that endowment effects magnify the substitution effect by shifting the indifference curve.

While there is some debate about whether experience can reduce the WTP – WTA disparity in an experimental setting (see List, 2003, 2004; Plott and Zeiler, 2005; Isoni et al, 2009), there is clear field evidence of the disparity in housing markets (Genesove and Mayer, 2001), and the importance of reference points and loss aversion in the supply of labour (Mas, 2006; Fehr and Goette, 2007) and insurance markets (Sydnor, 2006). The asymmetry between WTA and WTP, replicated in a number of studies, has important implications for markets that are characterized by low volume of trades. Non-market valuation is usually based on one-off trades of buying or selling. There are very rarely repeated trades, limiting the experience people have in the real or hypothetical market. All of these reasons provide some evidence as to why $WTP \neq WTA$, but if the third reason is more crucial than the first and second reasons, this would question the underlying theory of demand and WTP (Pearce et al, 2006). While this is an empirical matter, WTA values are theoretically required in valuation, especially where property right permit them – but not many researchers now use such a method. If we omit WTA, can we really elicit consistent preferences for a non-market good?

A reason why this may not be the case is through social preferences or altruism. A large number of laboratory experiments calls into question the assumption of pure self-interest. For instance, a large number of studies using the dictator game (where a subject (the dictator) has an endowment of $10 and chooses how much of the $10 to transfer to an anonymous partner) find that about sixty percent of subjects transfer a positive amount (Forsythe et al., 1994) – although the standard theory of self-interested consumers would suggest a zero amount.

Field evidence of this phenomenon would be charitable giving. When a fund-raiser contacts a person directly, the situation resembles a dictator game, except for the lack of anonymity. Field experiments in fund-raising, including List and Lucking-Reilly

(2002) and Landry et al (2006), estimate the effect on giving of variables such as the seed money (the funds raised early on), the use of a lottery, and the identity of the solicitor. Charitable giving is increasing in the seed money (List and Lucking-Reilly, 2002) presumably because of signaling of quality of the charity, and in the attractiveness of female solicitors for door-to-door fund-raising, especially for male respondents (Landry et al., 2006). The latter result implies that giving in door-to-door fund-raising is not purely the result of altruism, suggesting a more instrumental view of giving. Overall, however, these field experiments do not yet answer the key question of what motivates most giving, genuine social preferences or more instrumental reasons, such as social pressure.

This is crucially important because WTP is usually supposed to be about our own self-interest (excluding any existence value). But if we value a good for own our well-being and someone else's well-being, and then another person values a good for their own well-being and then for the other person's well-being, we are left with a great deal of double counting. This is demonstrated by both Bergstrom (1982) and (Jones-Lee, 1992). Bergstrom (1982) demonstrated that the value of statistical life should be set at the same level as is appropriate under conditions of pure self-interest. Jones-Lee (1992) extends this to shown that for all degrees of pure paternalism (i.e. in which concern for other people, whether weakly or strongly felt, ignores their preference, the implied value of statistical life is also identical to that which emerges under conditions of pure self-interest. Once such concerns enter people's preferences, then the values for non-market goods will vary. Jones-Lee (1992) shows that such concerns can inflate WTP by around 10 to 40 per cent.

2.3.1.2 Are experiences consistent?

As we have seen in this chapter, SWB has good reliability and validity. There has been little research into whether asking individuals their SWB affects their SWB rating. This is highly related to the Heisenberg uncertainty principle in physics, which states that one cannot measure the energy of a particle because the act of measurement affects the particle's energy level. Similarly, one could argue that introspection about happiness may be impossible since introspection affects happiness (Schooler et al, 2003). Ariely (1998) and Ariely and Zauberman (2000) showed that once respondents

created a moment by moment evaluation of an event or aspect of the day, their overall evaluation of the experience as a whole was influenced by the momentary responses they produced.

Wilson and Schooler (1991) used an experiment based on the quality of strawberry jam to see the effect of evaluation. The control group had to give a gut-reaction evaluation to a number of different jams, whereas the intervention group had to analyse their reasons for their answers for the preference. These ratings were then compared to ratings of experts. They found that the control group had a much higher correlation with the experts as opposed to the participant group, suggesting that reflection caused the participants to emphasize the inferences that they made about their experience and 'lose touch' with their actual hedonic feelings. Due to normative issues of the experts' opinions, Wilson et al (1993) did a follow up study without such 'expert ratings' and still found that greater evaluation undermined individuals' ability to decipher the utilities that they had actually experienced. Therefore, when individuals are forced to make quick gut judgements, their hedonic appraisals seem to reflect better their actual hedonic experience.

While measuring SWB might affect SWB itself, it could also be that the act of eliciting their SWB makes them behave in a different way which might affect their future SWB ratings, i.e. the Hawthorne effect. The Hawthorne effect of measurement occurs when individuals that are been observed during a research study temporarily change their behaviour.[7] Over time this has broadened to include the fact that increased attention on the individual makes them change their behaviour (Parsons, 1974; Adair, 1984). So the measurement or evaluation of an event might not only alter how individuals appriase the situation, but also affect the experience itself (Schooler et al, 2003). Firstly, evaluation may detract from the amount of attention that is devoted to the activity itself, which may in turn detract from the experience. Secondly, it is possible that engaging in hedonic evaluation might lead to disappointment and regret. For instance, Schooler et al (2003) conduct an experiemnt to find that monitoring happiness reduced individuals' ability to benefit from apleasurable experience, which suggests that monitoring one's ongoing experience

[7] It is a well established measurement artifact although Levitt and List (2009) question some of the original data at the Hawthorne plant.

can undermine one's ability to gain utility. Furthermore, trying to be happy lowered the mood and led to fustration, which undermined their ability to feel the pleasure that they sought. However, this experiment is unable to truly show whether monitoring reduces percieved happiness because it detracts from the experience, or because it forces individuals to make a more realistic evaluation of their hedonic state. Overall, it must be recognised that the mere act of asking a SWB question might affect experiences. There is a need for greater research in this area and relates to the comparability of experiences over time.

Consistent experiences relates to how comparable the responses are within and between individuals. Robbins clearly stated that experiences were not possible to compare between individuals. This inter-personal comparability is not really a problem when the study design has a clear identification strategy. To use experiences in non-market valuation, we have to use the causal estimate of the effect of the good on experiences. So by assumption we have to have a randomized, experimental or quasi-experimental design that mitigates worries on inter-personal comparability.

Intrapersonal comparability on the other hand is difficult to mitigate. Intrapersonal comparability requires that the same individuals use the scales, whether numerical or verbal, similarly over time. For the end points to be equivalent for life satisfaction questions, the individuals must have equivalent conceptions of the worst and best levels of satisfaction over time. This goes back to Stevens (1958), who argued that adaptation level theory illustrates the judgment relativity of labels and not adaptation in the conventional sense. When asked a life satisfaction question, respondents must usually decide for themselves what the endpoints of the response scale represent. Thus, one person may interpret the ten out of ten as the highest life satisfaction they have thus far experienced. Another person may interpret it as the highest level of life satisfaction they can imagine experiencing on Earth. This is not necessarily a problem unless the non-market good changes the end points over time within individuals that biases the estimates.

For instance, individual A and individual B might have the same endpoints in t_1, and in t_2 only A becomes a victim of a major flood that destroys her property and belongings. This flood in itself could change the endpoints in life satisfaction, as A

might not think that life in t_1 was pretty good in comparison to t_2, so their whole assessment of life satisfaction might be shifted downwards by a factor τ. So assume that the flood normally reduces life satisfaction by one point, but that the flood causes τ=-1 (i.e. a seven in t_1 is equal to a six in t_2), we would therefore have complete adaptation from the flood, although this is being caused by scale re-calibration and not adaptation in the conventional sense.

There is little evidence on this for SWB, but there is some research on 'reference shifts' within assessment of health-related quality of life (HRQoL) (Albrecht and Devlieger, 1999; Allison *et al*., 1997; Breetvelt and van Dam, 1991; Carver and Scheier, 2000). Reference shifts are defined as:

> "*a change in the meaning of one's self-evaluation of a target construct as a result of a) a change in the respondent's internal standards of measurement (scale recalibration, in psychometric terms); b) a change in the respondent's values (i.e. the importance of different domains which make up construct) c) a redefinition of the target construct*" (Sprangers and Schwartz, 1999: 1508).

Many of these health studies could be replicated looking at SWB measures. This would shed light on whether it is appropriate to directly compare SWB responses across changes in circumstances such as changes in health states, and a whole host of other non-market goods. The importance of scale recalibration for public policy becomes a normative issue.

2.3.2 Framing

2.3.2.1 Are preferences unduly affected by framing?

A key tenet of psychology is that the context and the framing of a situation matter. Two equivalent decision problems that are framed differently may elicit different responses. Tversky and Kahneman (1981) presented a classical example, where one group of 152 subjects were asked how they would respond to the outbreak of a disease which is expected to kill 600 people. Two alternative programs to combat the disease are presented: 'if Program A is adopted, 200 people will be saved. If Program B is adopted, there is 1/3 probability that 600 people will be saved, and 2/3

probability that no people will be saved.' When faced with this decision, 72 percent of subjects preferred program A. A second group of 155 subjects were presented with the same scenario, and asked their preference between Programs C and D: 'if Program C is adopted 400 people will die. If Program D is adopted there is 1/3 probability that nobody will die, and 2/3 probability that 600 people will die.' Programs C and A are equivalent, and Programs B and D are equivalent. When asked, however, only 22 percent of subjects preferred Program C, compared to 72 percent preferring Program A.

This framing effect is related to reference-dependent preferences, i.e. individuals evaluate the outcomes relative to a reference point, in the first frame that no one is saved, in the second frame that no one dies. Compared to this reference point, the outcomes are framed as gains in the first case (hence the risk-averse choice) and as losses in the second case (hence the risk-seeking choice). This example illustrates a general feature of human decisions: judgments are comparative, and changes in the comparison point can affect a decision, even if they do not affect the underlying economic trade-offs.

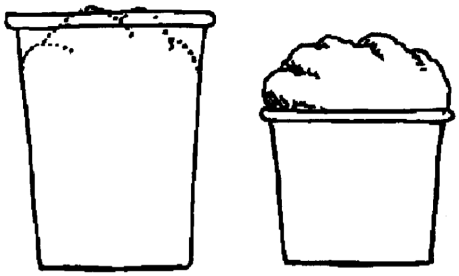
An example of framing is that of anchoring. Ariely et al (2003) (using Tversky and Kahneman's (1974) general approach) presented individuals with six products (computer accessories, wine, luxury chocolates and books) and asked them whether they would buy each good for a dollar figure equal to the last two digits of their social security number (ranging from 00 to 99). After this, they stated their maximum WTP for each product. They found that individuals with above-median social security numbers stated WTP values from 57 to 107 per cent greater than subjects with below-median numbers. For instance, for a bottle of average wine, those in the lowest quintile of social security number distribution were willing to pay on average $9 whereas those in the highest social security number quintile were willing to pay on average $28.

The most compelling evidence that preferences are constructed due to framing is from the preference reversal literature. There is significant evidence that the the price of risky prospects is systematically different from the choice ordering. The reversal of preference was first studied by psychologists, and Lindman (1965; 1971) first

discovered it with subsequent tests by Slovic and Lichtenstein (1968) and Lichtenstein and Slovic (1971). Grether and Plott (1979) brought preference reversals into the mainstream economics literature. Preference reversal has been found outside of experimental settings (see Tversky and Thaler, 1990, and Prelec and Loewenstein, 1991, for discounting reversals, and Clark and Oswald, 1996, for reversals in perceived fairness in the labour market).

One of the most illuminating examples of preference reversal come from Hsee's (1998) work. He presents two hypothetical servings of ice cream, one by Vendor H and one by Vendor L. They were illustrated by drawings as reproduced in Figure 1. It is clear that Vendor H's serving was contained in a larger cup than Vendor L's, but Vendor H's serving was under filled while Vendor L's over filled. Overall, vendor H's serving contained more ice cream, hence more valuable, than Vendor L's.

Figure 1: The two goods taken from Hsee (1998)



Vendor H Vendor L

The questionnaire for this study had three between-subject versions: separate-H, separate-L and joint. In each separate evaluation version, participants read the following: "*Imagine the following scenario: It is summer in Chicago. You are on the beach at Lake Michigan. You find yourself in the mood for some ice cream. There happens to be an ice cream vendor on the beach. She sells Haagen Dazs ice cream by the cup. For each serving, she uses [a 10 oz cup and puts 8 oz of ice cream] [a 5 oz cup and puts 7 oz of ice cream] in it*". These instructions were followed by one of the two drawings reproduced above. Respondents were then asked "*What is the most you are willing to pay for a serving?*". The joint evaluation version was parallel to the separate evaluation versions except that participants were told that there were two vendors on the beach. Participants were presented with the information, including the

drawings, for both vendors. They were then asked "*What is the most you are willing to pay for a serving from each vendor?*" Hsee used 69 college students from a large Midwestern university, and each respondent received one version of the questionnaire and completed it individually. Upon completion every participant received a cash payment.

The theory of preference reversal would predict that Vendor L's serving would be valued more in separate evaluation, and Vendor H's serving would be valued more in joint evaluation. The reasons for these predictions is that in separate evaluation, even though the amount of ice cream was the only value of concern, this attribute was hard to evaluate independently. Lacking a comparison, it was difficult to determine whether a serving with 8 oz ice cream (or with 7 oz ice cream) was good or not. The size of the cup provided a reference point. Presumably, respondents compared the amount of ice cream against that reference. The resulting `relation' attribute (over- versus under-filling) was easy to evaluate: Most consumers would find an over-filled serving appealing, and an under-filled serving unappealing. Thus, the over-filled serving (Vendor L's) would be valued more highly in separate evaluation. In joint evaluation, respondents could compare one option against the other, could recognize that Vendor H's serving contained more ice cream than Vendor L's, and would make their valuations accordingly.

The WTP for Vendor H in the separate evaluation was $1.66 and the WTP for Vendor L in the separate evaluation was $2.26, where the difference is significant at the five per cent level. In the joint evaluation, respondents were WTP $1.85 from Vendor H and $1.56 from Vendor L, where the difference is significant at the one per cent level. As predicted, there was a clear less-is-better effect in separate evaluation, and a clear preference reversal between joint and separate evaluations. In the separate evaluation versions, Vendor L's serving was valued significantly more than for Vendor H's, even though Vendor L's serving contained less ice-cream. In joint evaluation, the effect was reversed: Vendor H's serving was valued significantly more. For a review of such studies, see Hsee et al (1999).

Outside of the preference reversal literature, Benartzi and Thaler (2002) show that the choice of retirement depends substantially on the format of the choices presented. A

field study by Duflo et al (2006) on microcredit in a developing country confirms such framing, and they link such effects with attention. This framing can easily occur in non-market valuation studies, as how one frames the good or scenario, will obviously have an effect on the values elicited. This calls in to question whether giving more information to the respondent is desirable, especially in hypothetical scenarios. For instance, not only are framing effects more prominent with more information (Thaler, 1980; Slovic et al, 1982), but the influence of the level of information on a subject's WTP for a non-market good is potentially significant, particularly when non-use values are being elicited (Munro and Hanley, 1999). Furthermore, placing responsibility on the researcher for the supply of information creates ethical dilemmas. To the extent that subjects view interviews as unbiased sources, the possibility of altering the information set gives the unscrupulous researcher the opportunity to produce results that reflect the experimenter's preferences rather than those of the respondents. To the extent that this occurs, the whole process of using preferences for policy is undermined. Murphy and Clinch (2003) argue that is important for WTP values and the values used in CBA.

2.3.2.2 Are experiences unduly affected by framing?

Due to the fact that SWB contains an affective component, it is liable to be influenced by framing. At the extreme end for example, if the interviewer is in a wheelchair, good health might become significant in their SWB responses. While this can be overcome by good survey design (Frey and Stutzer, 2002), the extent to which it occurs is unknown. On the validity side, biases may sometimes arise since people may not know how happy or satisfied they really are, and the responses could be distorted by ego-defence mechanisms, in that it might be socially desirable to underestimate/overestimate their level of well-being, especially in the presence of other people, and this is something we do not know enough about.

There may also be problems of ordering effects for SWB, similar to that of survey methods. Bertrand and Mullainathan (2001) have argued that subjective data is vulnerable to ordering effects, which can be applied to SWB data. So it is important to examine the ordering or placement of the SWB question within the existing surveys to

examine if there are any discrepancies between these surveys.[8] The European Social Survey (ESS) has an eleven point global or life satisfaction question, which is situated in the politics section, immediately following a question on where you would put yourself on a left/right political scale. Hence, we would argue that how satisfied you are about the political situation of your country is likely to affect your SWB rating and analyses would more likely pick up an effect of political persuasion and inequality on life satisfaction. This is exactly what was found in Pichler (2006).

The Euro-Barometer survey has a four point life satisfaction question within the first section of the survey, meaning that it is less likely to be prone to ordering effects, although it immediately follows a question of whether the individual is able to vote in a political election. This earlier voting question might provide an explanation as to why Alesina et al (2004) find that Europeans' (using the Euro-Barometer survey) SWB is much more correlated with their political persuasion than Americans' SWB (using the GSS survey). It must be noted however that the position of the life satisfaction question within the Euro-Barometer does fluctuate, in that in some years it is positioned immediately after a financial situation question (e.g. Euro-Barometer number 30, 32, 34) and in others it is positioned after a question regarding what material goods the respondent has within the household (e.g. Euro-Barometer number 22). This finding of SWB correlated with political persuasion might contradict to what is found in chapter 7 using the BHPS.

The two main panel data sets analysed in the literature are the British Household Panel Survey (BHPS) and the German Socio-Economic Panel (GSOEP). The BHPS has a seven point life satisfaction question that follows domain satisfaction questions (i.e. health, household income, house, partner, job, social, amount of leisure, quality of leisure). The GSOEP has an eleven point life satisfaction question in section 8.1 which is after a section on education and qualifications, and the life satisfaction question is immediately after questions regarding satisfaction with various domains (i.e. amount of spare time, health, local public transport, environment, job, household and personal income, standard of living and democracy). The life satisfaction ratings

[8] The surveys we will highlight are all examined and analysed in Layard et al (2007).

in both the BHPS and GSOEP therefore might be biased by the domains that are prior to the LS question.

The wording issue has been explored to an extent by Peasgood (2009). She found that those with higher levels of education were less likely to respond at the top end of the life satisfaction scale. So there may be a difference in interpretation of the word 'satisfaction' for some groups. Other concerns relate to whether people use different implicit anchors when responding to these evaluative type questions. Anchors may vary from the 'the best and worst life any human being is capable of experiencing and I am capable of imagining' to 'the best and worse life imaginable given my background, my current personal and socio-economic circumstances'. If an individual's choice of implicit anchors is related to their characteristics, such as education, age or income, this will undermine interpersonal comparisons (Dolan and Peasgood, 2009). Comparisons between different explicit anchors would also help understand the potential role of individual characteristics on how these questions are answered.

Overall, it seems that there is little consistency as to where the SWB question is situated within the existing surveys, and there could be some possible ordering effects within these surveys. At the very least, if domain satisfactions are to be elicited, this should be done after life satisfaction is elicited so as to minimise any bias that the domains may have on the overall life satisfaction rating. There be issues with how the question is frames in terms of language, but this requires much more research.

2.3.4 Affective Forecasting

The standard economic model assumes that decision-makers are on average correct about the distribution of the states of the world. There is however well-documented evidence that people have incorrect beliefs due to projection bias (DellaVigna, 2009). It must be noted however that projection bias has been a source of inquisition since Adam Smith's time. For instance, Smith (2002: 173) argued that "*the great source of both the misery and disorders of human life, seems to arise from over-rating the difference between one permanent situation and another*".

In non-market valuation, consumers are required to project their current preferences onto future periods. In health valuation, they are asked to project their current health preferences onto different health states in the future. In environmental valuation, they are asked to project their current environmental preferences onto different environmental states in the future. The model of projection bias was presented in Loewenstein et al (2003), where they assumed that utility u is a function of consumption c and of a state variable s, that is, $u = u(c, s)$. The current state is s' and the future state is s. When predicting the future utility $\hat{u}(c, s)$, an individual with projection bias expects utility $\hat{u}(c, s) = (1-\alpha)u(c, s) + \alpha u(c, s')$ rather than $u(c, s)$. The parameter $\alpha \in [0,1]$ captures the extent of projection bias, with $\alpha=0$ denoting the standard case and $\alpha=1$ the case of full projection bias. The evidence from the field and lab experiments is that $\alpha>0$.

Empirically, Gilbert et al (1998) asked subjects to forecast their happiness in correspondence of an event, and compare these responses to the responses after the event has occurred. Thirty-three current assistant professors at the University of Texas forecast that getting tenure would significantly improve their happiness (5.9 versus 3.4 on a 1-7 scale). The difference however in rated happiness between 47 assistant professors that were awarded tenure by the same university and 20 that were denied tenure is smaller and not statistically significant (5.2 versus 4.7). Similar results apply for the election of a Democratic of Republican president, kidney dialysis, moving accommodation and a range of other domains. This can be linked to focusing effects, which Schkade and Kahneman (1998) arguing that "nothing in life is quite as important as you think it is, while you are thinking about it". Schkade and Kahenman found that people mis-forecast the effect that weather has on people's subjective well-being. This will clearly have implications for consistency of preferences, but the starting point originates from incorrect beliefs.

Conlin et al (forthcoming) present evidence of projection bias using a data set of two million orders of cold-weather apparel items. They consider the effect of weather at the time of purchase on the probability that an item is returned, conditional on purchase. According to the standard model, colder weather at the time of purchase should not affect the probability of a return, or may affect it negatively (since colder

weather at the time of purchase is correlated with colder weather over the subsequent days). Projection bias, instead, makes the opposite prediction. On colder days, individuals overestimate the use that they will make of a cold-weather item, and hence are ex post more likely to return the item. This prediction holds whether the projection bias regards future utility, ('I expect to like cold-weather items very much'), or future weather ('I expect the coming winter to be very cold'). Conlin et al find that a reduction in the order-date temperature of 30◦ F (for example from 40◦ F to 10◦ F) increases the average return rate of a cold-weather item by four percent, which is consistent with projection bias. A simple structural model of projection bias implies estimates of $\alpha \approx 0.5$, implying that consumers predict future tastes roughly half-way between present tastes and actual future tastes. A caveat to this research might be that on colder days more 'marginal' individuals (which are more likely to return) order cold-weather clothing.

Nonetheless, in a number of domains, people seem to consistently mis-predict their future well-being. This is worrying for using preferences as a guide to well-being and for valuing non-market goods since many of the valuation studies are on states of the world that have not yet been experienced, so they require no or negligible projection bias. Given the lack of experience of non-market goods, it could be that α tends towards one for non-market states of the world.

2.3.5 Income and future/existence utility

If the main objective for a policymaker is to use SWB for the non-market valuation aspect of a CBA, then the monetary valuation of experiences is necessary. The problem to date is that we do not really know how both absolute and relative income translates into SWB, and what, if any, variables can be used as an instrumental variable. In recent times the debate on this has gathered pace and the interested reader should consult Clark et al (2008) and Layard et al (2008). The absolute income coefficient used in papers that have attempted to monetise their SWB results have used the income coefficient for their population, but this could clearly vary across similar populations. So a program in one area could be valued higher than the same program in another area simply because of differing income coefficients. Clearly,

more research on the relationship between income and SWB is needed. There might also be a need for a rough and ready coefficient that can be used across all non-market studies, such as the discount rates that are currently employed in CBA. At the very least, the rough and ready coefficient could be adjusted to account for income levels as already done for the benefit transfer method in CBA.

In the very long run, if all policies were evaluated by SWB, a joined up approach might be to use the weights from the SWB regressions without monetising. So we could have a form of cost-happiness analysis, which will be similar to using QALYs that are currently employed by the National Institute for Health and Clinical Excellence (NICE). So monetising experiences might be a problem for establishing SWB into public policy, in the long-run, it might not necessarily be such a large problem.

Another foreseeable problem with SWB is that it examines the stocks and flows of events on SWB that have been experienced. An individual would have to experience a non-market good sometime during their life for it to contribute toward their SWB. The expectation of a future event can obviously be incorporated into SWB. So the potential loss of a specie in the future, or the loss of a good which we might use in the future, i.e. an option, can clearly affect SWB. Some economists might however argue that for public policy, we might need a monetary value on events that have not already happened, such as a serious flood in London in the future. But individuals in the UK have already experienced serious floods, for instance in Gloucestershire in 2007, so we can value floods in one area and extrapolate it to London. Many disasters or negative externalities have happened to someone in the past.

Given that there is no method to extend well-being into the future, it is clear that SWB might not incorporate non-use value of a good. For example, according to the preference satisfaction account, the total value (T) of a good comprises of its expected use value (N), and its non-use value which is made up by existence value (X) and bequest value (B):

$$T = N + (X + B)$$

This essentially states that an individual might derive value for a non-market good in terms of its pure existence and for its existence in the future, so latter generations can enjoy the wetlands. Krutilla (1967) first analysed this existence value and thought it to be derived from either selfish or altruistic motives. These selfish or altruistic motives, however, are not small. Navrud (1988) analyses a scheme to improve water quality where ($E + B$) accounted for 63% of the total value of the good.

From the perspective of experiences, there cannot be a value given to a good if someone somewhere does not experience it. To predict the value of a good on experiences by a close substitute might give some indication of its value. For preferences, as long as people are willing to pay for the existence, then it surely must matter to their utility. But if they receive no benefit in terms of their experience, at the point of purchase (beyond warm glow) or in the future, then this construes as classic mis-wanting. From an objective well-being account, it could be that such goods, which are never experienced by the individual, provide the possibility of that person becoming a flourishing individual since we cannot appreciate that good today. It is doubtful though that individuals have clear preferences for such goods because they will not be consumed by the good (so they lack experience) and consequently they lack the feedback from the good, and the goods are, mostly, very complex.

Existence value turns the debate into philosophy and whether intrinsic value of something is possible. Both preferences and experiences would not believe that 'things' (and not necessarily goods) have value in themselves because they are placed on Earth, over and beyond the instrumental value they give to humans. The philosophical debate is lively in this area (see Pinchot, 1914; Armstrong and Botzler, 1993). What an experiences argument would look like however is that we cannot value such 'things' for public policy because they do not derive any utility for any human being on Earth.

2.6 Conclusion

Since the late 1960s, psychologists have been arguing that people do not really know what they prefer in a choice setting, so that preferences seem constructed as when the

situation arises. As a result, choices seem to be inconsistent, liable to framing, and affected by a whole host of theoretically irrelevant factors. Economists have been slow to acknowledge such effects however, and when economists make policy recommendations, they are typically made based upon preferences, implicitly assuming that these preferences are identical to their normative preferences (Beshears et al, 2008).

At the start of the 1990s, Diamond and Hausman (1994) argued that SPs do not measure the preferences they attempt to measure, and that improvements in survey methodology are unlikely to change such a view. This argument is primarily due to the fact that SP studies do not derive values of preference but values of attitude. For instance, Kahneman and Ritov (1994) believe that people respond in attitudes, and just because they express a monetary term does not necessarily mean they express a preference. This corresponds to the warm glow effect in expressing support for good causes that are more to do with attitudes and not preferences (Andreoni, 1989). Studies such as Schkade and Payne (1993) support such an argument. They used a think aloud strategy for evaluating CV responses, and found that a significant amount of people stated that they just made up the answer, and others mentioned comparisons given to charities.

As we have seen in section 2.3, revealed or stated preferences could be viewed as being malleable, so that choices and behaviours can be changed, even for private goods. Whether it is RPs or SPs, it has been shown that people's choices are not always consistent for non-market goods. The contribution of this chapter is to show how the general problems with preferences can extend to inconsistency and framing. By documenting and classifying the problems with both RPs and SPs for private goods, where there is plenty of feedback or experience, illustrates the point that these problems would possibly be more severe for preferences for when there is little feedback or experience, i.e. non-market preferences. Some have argued that if both valuation methods provide the same values, which some argue goes some way to show validity of sing SPs (Griffin et al, 1995; Johnston, 2006). But this chapter clearly shows that both types of preferences might not necessarily reveal 'true' values since those values might have been biased in very similar ways.

If we can concede that the problems with RPs and SPs cause valuing non-market goods problematic by these methods, we could use other types of preferences that can be used in the social welfare function. For instance, Harsanyi (1982; see also Scanlon, 1991) suggest that we might want to distinguish between self-regarding and ideal-regarding preferences. The problem with this is that it then becomes difficult in distinguishing between a whole host of preferences that are informed and uniformed, rational and irrational, considered and ill-considered, ephemeral and enduring, spontaneous and adaptive, citizen and consumer etc (Jeffrey, 1974; Griffin, 1986). The issue with this line of argument is that if our RPs and SPs are biased, how do we know that our normative preferences are not biased as well, and how would one mobilise a system of where we determine how one set of preferences are good for us whereas another set are not? These issues are unresolved and they will be argued for some time. So using such normative preferences for non-market valuation might be highly problematic.

Overall, the problems and anomalies of preferences provide some very practical limitations of von Neumann and Morgenstern's (1944) standard theory. The research on preferences in the future might go down two concurrent routes. Behavioural economists are beginning to use data that lie outside the traditional scope of economics, namely questionnaires about mental states and data such as neural imaging on psychological processes inside the brain. On a classical RP view, neither sort of data is of interest, but this data can indeed be of use, provided it is interpreted correctly (Fudenberg, 2006). Camerer and Loewenstein (2003) have argued that this is inevitable as it is unwise and inefficient to do economics without paying some attention to good psychology. On the neurological side, having a model that is better accord with the underlying structure of the brain can be valuable, as it may lead to more accurate out-of-sample predictions (McClure et al, 2004; de Quervain et al, 2004). This might give us a greater understanding of preferences, but this is too early to tell.

The subjective assessments of mental states, i.e. their experiences, might also be an alternative way of overcoming the problems of using people's preferences for valuing non-market goods. This is different to the neurological approach since it does not try to provide a better understanding of the brain and how individuals make choices. It

does however provide us with a different interpretation of well-being that is based on what people enjoy rather than what they want. The link between people's preferences and experiences however has not been fully established, and is an untouched area in non-market valuation. This measure of experiences is normally associated with the measurement of individual subjective well-being.

This chapter documents the approach used by economists to measure SWB. Research in this area has been increasing but there is a lack of research in valuing non-market goods using SWB and applying such values into a CBA. More importantly, there has been no research to date that has determined whether the well-being we get from non-market goods is the same irrespective of whether our welfare measure is preferences or experiences. This chapter has identified a very large gap in the literature and recommends for further research to close this gap, which will make for more effective government policies.

There might be some problems to using SWB though for valuing non-market goods. The issues surrounding the consistency and framing seem particularly pertinent for using such values in CBA. The use of such data can however also be useful in other settings, such as trying to examine policies to change behaviour (Dolan and White, 2007). Moreover, to look at what behaviours SWB can predict in a causal setting would advance the literature somewhat. There has been some tentative experimental work on this from Oswald et al (2009) and Cohen (1997).

It must be noted that many organisations, such as the OECD, and national governments, such as the UK Government, are starting to use SWB as a metric for policy. So knowing how preferences and experiences compare and where we expect to observe some discrepancies is clearly important for welfare economics and public policy. Overall, this chapter contributes to the field by showing how little we know of using SWB for non-market valuation, where the gaps in the literature are, and some of the problems that could arise in the future when using SWB in comparison to preferences.

3. COMPARING WILLINGNESS-TO-PAY AND SUBJECTIVE WELL-BEING IN AN URBAN QUASI-EXPERIMENT

3.1 Introduction

As we have seen in the previous chapter, there has been no comparison of monetary valuations from preferences and experiences. The non-market good we use to compare between WTP and SWB is an urban regeneration scheme in the UK. Regeneration does encompass some private goods (e.g. new house fascias) but when the whole area becomes regenerated, and when the area has a pleasurable aesthetic appeal, urban regeneration becomes a non-market public good. Regeneration is usually targeted at individuals in poor and materially deprived neighborhoods. In the United States, there have been individual-based strategies (i.e. a demand side policy), such as the Moving to Opportunity schemes, where individuals are given vouchers to move from deprived to less deprived neighbourhoods, whereas in the United Kingdom, there have been attempts to physically regenerate the neighborhood where individuals remain *in situ* (i.e. supply side policy).

An illustration of the importance that the UK Government places on urban regeneration is their recent New Deal for Communities (NDCs). The NDC is an area-based initiatives launched in England in 1998. The aim of the NDCs were to reduce the gaps between some 39 deprived neighbourhoods and the rest of the country primarily in relation to five outcome areas: crime, education, health, worklessness, and housing and the physical environment. In the 39 NDC areas, each on average accommodating about 9,800 people, and each NDC has attracted approximately £50m of Government investment. A large proportion of that money is for physical improvements.

Within our own study, our neighbourhoods are perhaps more tightly defined than in other studies, such as the NDCs, Katz et al (2001) and Luttmer (2005), Kling et al (2007), where the analysis of secondary data means the neighbourhood is defined according to reasonably large areas – this point is also made by Clark (2009) and addressed in Clark et al (2009b). In contrast, we gather primary data from two

spatially separated neighbourhoods that are within the same political or census boundary. These two neighborhoods have populations of less than one thousand individuals and are spatially distinct from one another, in that they are separated by a major train line and a school, but one area has recently had urban regeneration and the other has not. This allows us to assume a quasi-experiment of an exogenous change of policy at the local level. The use of quasi-experiments in environmental economics and non-market valuation is increasing, e.g. Greenstone (2002), Chay and Greenstone (2005), Deschenes and Greenstone (2007) and our paper is in a similar spirit to these seminal papers.

It is worth noting that the link between health and the environment goes back centuries, especially in terms of housing and sanitation (Gesler, 1998). Towards the end of the nineteenth century, architects believed that visual contact with nature was beneficial to the emotional and physiological health of individuals (Ulrich and Parsons, 1992). More recently, there has been a great deal of research, especially in the field of environmental psychology, on identifying individuals' relationship with nature along with their aesthetic preferences for such environments (Gullone, 2000). While economists have been long trying to monetise the effect of the environment on RPs and SPs, other social scientists have been trying to tease out the impact of the environment on human health.

From the outset, it is clear that the environment can have a negative effect on human health through: (i) allergies; (ii) poisoning; and (iii) respiratory disease and pesticide exposure. These can cause disease, cancer and possible death. However, most research has gone in to demonstrating how humans can benefit from exposure to an environment. A highly influential paper is Ulrich (1979), who analysed students' reactions to slides of urban and natural environments. He found that those watching the natural environment slides had a significantly higher positive affect than those watching urban environment slides. By positive affect, he means feel elated or pleased. Ulrich (1984) also found that a natural view helps to increase the chances of recovering early from hospital treatment, and Moore (1981) has identified that prison inmates that have natural views from their cell were less likely to use health-care facilities. Tennessen and Cimprich (1995) has found that views to nature through a window can increase attention, providing some support for Kaplan and Kaplan's

(1989) theory that natural environments help maintain or restore the capacity to focus and concentrate.

How the environment effects individuals' SWB can be split in to two parts: both the urban environment and the natural environment. There have been a number of studies that have included a dummy variable within their SWB functions so as to determine whether urban living is negative for SWB. For example, Gerdtham and Johannesson (2001), Frey et al (2004), Graham and Felton (2006), Hudson (2006), and Spellerberg et al (2006) all find that living in a highly populated urban area, compared to either a smaller urban town or a rural village, is significantly bad for SWB. However, Shields and Wheatley Price (2005) do not find a bad effect of urban living for SWB. Using the BHPS cross-sectionally for respondents in the UK during 1996, Ferrer-i-Carbonell and Gowdy (2007) find that living in a house that has pollution, grime or other environmental pollutants reduces SWB. van Praag and Baarsma (2005) found that noise pollution around the Schipol airport in Amsterdam has a significant negative effect on SWB once controlling for everything else.

Propper et al (2004) analyse mental health (i.e. the General Health Questionnaire) and neighbourhood effects from the British Household Panel Survey (BHPS). They define five important aspects of the neighbourhood, i.e. socio-economic disadvantage, mobility, age, ethnicity, and urbanness,[9] and find that these aspects do not significantly affect individuals' mental health. These neighbourhood characteristics however might not necessarily be important to individuals' mental health and they do not consider the quality of the urban environment. Sirgy and Cornwell (2003) suggest that the neighbourhood effects SWB through community satisfaction, but they do not control for individual effects. Using panel data from the United States, Luttmer (2005) find that there is a strong negative association between neighbours' earnings and SWB.

An issue with each study is selection bias and reverse causality. On selection effects, it might be that those who are more likely to move to a rural area are those who are more likely to enjoy living in a rural location and therefore have a higher SWB. This

[9] Urbanness is mostly made up of the % of people working in agriculture which is not so different to the SWB studies that include dummy variables for 'urbanness'.

is not a generalisable result since not everybody would obtain greater SWB from moving to a rural area – so it is in essence a local treatment effect. On reverse causality, it might be that happier people move to rural areas. Overall, there are little to no studies to date that have examined an actual neighbourhood or local area and determined whether place or aesthetics matter for SWB.

While there has been a link shown between urban environments and SWB, there has been less evidence on the natural environment and SWB. At the macroeconomic level, Welsch (2006) has found that air pollution across European nations has a significant negative effect on SWB. For example, he found that nitrogen oxide induces a negative and significant effect on people's SWB. However, Welsch's study has the problem of omitted variables bias since it does not account for the majority of determinants of SWB. Brereton et al (2006), using Irish cross-sectional data, have shown that factors such as population density, access to and quality of facilities and services, the environment and climate are important determinants of SWB, and can be just as important as the most critical socio-economic and demographic factors. Consideration of these variables, which are generally ignored in the SWB literature, is according to Brereton et al (2006) crucial in understanding what increases SWB. However, this study also does not account for a great deal of determinants of SWB, e.g. relative income, so its shortcoming is the very problem in which they argue to be problematic in other studies. Redhanz and Maddison (2003) have shown that extreme weather conditions are detrimental to SWB.

The problem for all of these studies on the environment is reverse causality and/or selection effects. It is well-known that urban environments have 'residential sorting', so that deprived, ill-health, poorly educated individuals tend to be place in poor living environments by social construction. Therefore, it is unsurprising that these areas, which are characterised by high pollution or high population density, have lower SWB since those individuals already living there are more likely to be unhappy or have lower life satisfaction. The only causal study to date using life satisfaction is Luechinger (2009). He uses an exogenous change in air filters of major power plants in Germany, to show that air pollution does indeed affect life satisfaction in a harmful way. A vast amount of research on the urban and natural environment is needed to

determine what parts of the environment is more susceptible to affect different individuals' SWB.

3.2 Valuation methods

3.2.1 Revealed preferences

As shown in chapter 2, considerable research has been conducted on hedonic house price models, especially in the valuation of air quality (Smith, 1995). There has been relatively little research into the using the hedonic pricing method to value the effect that urban regeneration has on house prices. Taking our example of where one area has had urban regeneration and the other area has not, we would expect the difference in house prices to reflect the willingness to pay for the regeneration holding all other attributes constant. Typically, we would have a vector of housing attributes $z = (z_1,\ldots,z_N)$ and an amalgamated good, x, which includes private goods except housing. We assume individuals to have the following utility function, $u(x, z)$, and that individuals maximise their utility subject to the budget constraint $x + P(z) = Y$, where $P(z)$ is the price of a house with attributes z, and Y is household income. In this case, the individual maximises utility by choosing:

$$\frac{\partial P}{\partial z_R} = \frac{\partial u / \partial z_R}{\partial u / \partial x} = WTP_{RP} \qquad (1)$$

where the marginal price of a regenerated house, z_R, equals the marginal rate of substitution between z_R and x; that is, the marginal WTP for regeneration. Once the identification of the hedonic function is stated, we can estimate the WTP_{RP} by ordinary least squares (OLS).

3.2.2 Stated preferences

The CV survey elicits an individual's maximum WTP for a given good. It is assumed that an individual wishes to maximise utility subject to income, y, where the indirect utility function, in this case for the regeneration (R), is: $u(R, y)$. The regeneration

helps to change the z's discussed in 3.2.1. An individual's stated preference willingness to pay (WTP_{SP}) is the income loss equivalent to the regeneration:

$$u(R_0, y_0) = u(R_1, y_0 - WTP_{SP}) \quad (2)$$

where $R_1 \geq R_0$ and that an increase in R is seem as desirable (i.e. $\partial u / \partial R > 0$). We are using WTP rather than willingness-to-accept (WTA) since we are investigating an increase in utility from an initial lower utility level. Moreover, residents do not have property rights over government sponsored regeneration (see Knetsch and Sinden (1984) for greater discussion on compensation measures).

3.2.3 SWB-based valuation

Following the work on SWB in economics to date outlined in chapter 2, our notion of SWB is based on how satisfied an individual is with their life. To place a monetary value on a non-market good, we use the standard compensating differential approach as outlined in Chapter 2. We specify a utility function, which for the sake of exposition includes only income and regeneration:

$$v = h[u(y, R)] + \varepsilon \quad (3)$$

where v denotes some SWB i.e. life satisfaction. The $u(y, R)$ function is the respondents' true utility which is only observable by the individual. Therefore, $h[.]$ is a non-continuous non-differentiable function which maps actual utility to subjective well-being. The error term, ε, captures the fact that individuals cannot accurately map underlying true utility (u) on to SWB (v).

In order to estimate a function such as (3), one can use ordinary least squares (OLS), ordered logit or ordered probit regression. There is, however, some evidence that it makes little difference to the estimated coefficients if we were to assume cardinality and estimate the model using OLS (Ferrer-i-Carbonell and Frijters, 2004). The SWB function will therefore be:

$$SWB_i = \beta_0 + \beta_1 \ln(y_i) + \beta_2 R_i + \beta' X + \varepsilon_i \quad (4)$$

where $\ln(y_i)$ is the natural logarithm of household income, R_i is the regeneration as in (2), X are the personal and social characteristics, and ε_i is the standard error term. By using the estimated coefficients for the regeneration ($\hat{\beta}_2$) and household income ($\hat{\beta}_1$), we can calculate the income compensation (IC) for the regeneration or alternatively the implicit utility-constant trade-offs between regeneration and income. The IC is defined as the increase in income necessary to hold utility constant if the house and area are not regenerated. In an indirect utility function, this would be given by:

$$v(R_0, y_0 + IC) = v(R_1, y_0) \quad (5)$$

where $v(.)$ is the indirect utility function, y_0 is the initial household income, R_0 is the condition of the area prior to regeneration and R_1 is the condition after the regeneration. Given the specification of the micro-econometric SWB function expressed in equation (4) and the position of the IC in the indirect utility function, the *IC* (at mean income level) can be defined as:

$$IC = e^{\left(\frac{\hat{\beta}_2 (R_1 - R_0)}{\hat{\beta}_1} + \ln(y_0)\right)} - \overline{y}_0 \quad (6)$$

where $\overline{y}_i$ is average household income of the sample population.

3.2.4 Comparing the methods

If preferences and experiences are theoretically equivalent, then equating (1), (2) and (5) gives:

$$\frac{\partial u / \partial z_R}{\partial u / \partial x} \equiv u(R_0, y_0) = u(R_1, y_0 - WTP_{SP}) \equiv v(R_0, y_0 + IC) = v(R_1, y_0) \quad (7)$$

<u>*Hypothesis*</u>:

If: (i) R_0 and R_1 are identical, i.e. the change in the regeneration is the same for revealed and stated preferences, and for SWB; and (ii) the initial income level, y_0, is identical in both the $u(.)$ and $v(.)$ functions; then:

$$WTP_{RP} = WTP_{SP} = IC \tag{8}$$

In order for this equality to hold, the marginal rates of substitution in preferences and experiences must be identical. This is the hypothesis that we test.

3.3 Methodology

3.3.1 Background to the quasi-experiment

The urban regeneration programme we use to begin comparing WTP and SWB was targeted at the Hafod area of Swansea, Wales, UK. The specific details of the regeneration programme are not especially relevant to this paper – it is the comparison of WTP and SWB in the context of a quasi-experiment are the important methodological features – but it consisted of four main elements: renewal of fascias, gutters and roofs of houses; renewing property front boundary walls and paths/paved areas; road resurfacing; and provision of new improved feature street lighting. The Hafod area has roughly 950 residential/commercial properties and, by the end of 2007, over 500 properties had been renewed since 2001. This renewal to date has cost around £10million and is expected to cost £20million by completion in 2011. An adjacent neighbourhood, Landore, was chosen as the control area as it has very similar characteristics to Hafod apart from the fact that it has not had the regeneration – see Table 1 for a comparison of the two population groups. These two neighbourhoods were almost identical in terms of deprivation indices before the regeneration and so the urban regeneration can be treated as having been approximately randomised between Hafod and Landore. The Swansea Local Authority obviously had to choose one area to regenerate and did not have the resources to regenerate both areas together (this regeneration was the first of its kind in the Swansea Local Authority area). As a result, the Hafod area was chosen to have the regeneration funding over Landore.

3.3.2 Revealed preference data

The most robust comparison of revealed preference across the two areas will come from house price data obtained from market transactions. House price data are available online from the Land Registry (i.e. www.houseprices.co.uk), which also contains data on type of the house (flat, terraced, semi-detached or detached) as well as whether it is leasehold or freehold. Several dummy variables were also used to account for whether each individual house is on a one-way street and whether it is overlooking a park.

Furthermore, we use subjective assessments of crime from our survey and average the value across each individual street. We do not have data available on floor area (both internally and externally) and the number of rooms or bedrooms each house has. While these factors have been found to account for a large degree of variation in other samples (Leggett and Bockstael, 2000), the majority of houses in this area were built at the same time under similar specifications, so there is a great deal of homogeneity between the houses. As a result, we do not need to control for internal floor area in our linear and semi-log functional forms.

3.3.3 Stated preference and SWB data

Apart from the SWB questions, the survey took on the same format of a traditional CV survey. The survey comprised four sections. Section 1 contained a global life satisfaction question followed by a number of domain satisfaction questions. Section 2 included attitudinal questions, including those relating to the local area. Section 3 included the WTP section, and only households who had not had the regeneration had this part of the survey. Section 4 elicited demographic information.

The initial life satisfaction question used the International Wellbeing Group (2006) question: "*Thinking about your own life and personal circumstances, how satisfied are you with your life as a whole?*". Possible answers range from zero (completely dissatisfied) to ten (completely satisfied). Bertrand and Mullainathan (2001) have argued that subjective data is vulnerable to ordering effects. This is indeed a problem for many surveys since the life satisfaction question is normally situated in the middle

of the survey (as in the British Household Panel Survey), usually coming after domain satisfaction questions, which may bias the ratings given. Therefore, in this study, the life satisfaction question was the very first question on the survey and questions about domain satisfactions follow life satisfaction.

In developing the WTP question, it would have been a very complex task to ask respondents to value all aspects of the regeneration programme, so the main features were stated within a top-down approach, i.e. participants had to place a value on the whole bundle of goods together and then subsequently value each good individually. Respondents were initially asked for their overall WTP for: (i) resurfaced exterior walls of their house; (ii) new front garden walls and paths of their house; (iii) new improved feature street lighting; and (iv) resurfaced roads and pavements. The first two are quasi-private goods whereas the latter two are public goods. A follow-up question asked how this overall amount was broken down into values for (i) and (ii) together and (iii) and (iv) together. The top-down method is the accepted approach within the literature (Pearce et al, 2006),

We use the payment card (PC) method for our WTP estimates. We could have used the DC method, but this requires larger samples than available through our quasi-experiment (Bateman et al, 2002). Respondents were asked to circle one value from a possible sixteen values, ranging from zero to one hundred pounds sterling (about $200). The question was worded as follows: "*Taking all these improvements together, what is the highest amount, if anything, that you would be willing to pay on behalf of your household per month for the next 3 years for these improvements*?"

The final section included a number of background variables that have been shown to be associated with SWB and WTP responses, such as: gender; age; marital status; employment status; social capital; health; and gross household income. The questionnaire was posted by mail in March 2007 to 950 households in Hafod and 675 households in Landore. SWB responses may be best elicited in private where there will be limited bias from the presence of an interviewer.

3.4 Results

We received 364 (22.4%) completed questionnaires. Given the relative complexity of the survey and the fact that response rates are lower in more deprived samples, this seems acceptable and it is broadly comparable to some other published studies which have multiple valuation methods in the survey (e.g. Bala et al, 1998). In any event, the response rate is less problematic given the representativeness of the sample. Table 1 shows that the sample is comparable to the National Census which took place in 2001. Within the sample, 61% reside in the renewal area (although 19% of the 61% live on roads which are not regenerated) and 39% reside in the control area. Of those who have had their home renewed, the average time since renewal was 2.2 years. Importantly, all of the individuals living in the regeneration area have been living in their house since the regeneration took place, which mitigates any residential sorting problems.

3.4.1 Revealed preferences

Between April 2000 and May 2007, 511 properties were sold within the whole area. Figure 1 shows the average house price every six months for the two areas (the official announcement of the regeneration area took place in 2001). It is clear that there are no significant differences in house prices between the two areas in 2000. Regression (1) in Table 2 gives the baseline regression where the time period is after the regeneration (i.e. 2002 to 2007). The time trend variable takes on the value of 1 in April 2000 and up until 86 in May 2007. The marginal effect of time on house prices is £1,033 per month – this is reflected in the increasing trend of both areas in Figure 1. Being in a semi-detached home (the majority of houses are terraced) or living on a one-way street will have an effect of increasing house prices by £9,520 and £6,413 respectively. Having a house overlooking a park increases house prices by £14,689. However, being in the regenerated area does not significantly increase house prices although it does have a positive effect. The regeneration variable here encompasses houses that are in the regenerated *area* or not and is not based on whether the *house* has actually been regenerated or not. Note that the adjusted R^2 is quite high for a hedonic regression despite the fact that floor space, the number of bedrooms, and the quality of interior are not controlled for, supporting the notion that there is a great deal of homogeneity in the housing stock in this area.

Regression (2) in Table 2 has the same specification as regression (1) apart from the fact that the functional form has slightly changed in that we have assumed a non-linear relationship between house prices and the right hand side variables. Again, as in regression (1), the coefficients which drive the variation in house prices are: being a semi-detached property; being on a one-way street; overlooking a park; and the time trend. Comparing (1) and (2), it seems that (1) has the better fit despite the fact that we have not used the log likelihood test since our variables are binary.

Table 3 takes the same specification as Table 2 although different time periods are analysed to determine how the house prices have evolved over time since the regeneration. Row 1 illustrates the baseline regression in Table 2. Row 2 gives the data pre 2002 (i.e. 2000 and 2001) and it is clear that being in the regenerated area has no significant effect on house prices within our sample. Rows 3, 4, 5, 6 and 7 illustrate the hedonic function for the following years: 2002, 2003, 2004, 2005 and 2006-2007 respectively. It is clear that living in the regenerated area does not significantly increase house prices for any particular year. Row 8 changes the independent variable so it becomes one when the exact road the house is situated on has been regenerated and zero otherwise, as opposed to being a dummy variable for the area. This regression shows that for the whole sample, even when we are more specific about the regeneration time periods, being in a regenerated house and street is not significant in increasing house prices. A difference-in-difference model was also estimated using the announcement of the regeneration area in 2001 as the policy change and we also found a non-significant result as above.

3.4.2 Stated preferences

The distribution of the overall WTP estimates is given in Figure 2. The positive skew on the data is comparable to many other WTP payment card studies. We use the parametric approach to estimating the WTP values from the payment card. This has the benefit of accounting for interpolations between monetary amounts stated on the payment card. The two parametric approaches analysed here are OLS (WTP_{OLS}) and

interval data (WTP_{INT}) regressions.[10] Rows 1 and 2 in Table 4 show the parametric WTP values based on OLS (WTP_{OLS}), and interval data (WTP_{INT}) respectively based on estimates in Table 5. Regressions (1) and (2) in Table 5 present the WTP_{OLS} and WTP_{INT} for regeneration, respectively. Outliers are omitted using the Belsley et al (1980) procedure. Both regressions use individual characteristics as independent variables, which allow us to establish whether the determinants of stated preferences are similar to those of life satisfaction. From regressions (1) and (2), we obtain WTP values of £228 and £245, respectively. Given that household income is significantly related to higher WTP values in regression (2), this provides some validity to our estimates. Other variables, such as age and marital status, are also important in explaining WTP values.

3.4.3 SWB responses

Figure 3 illustrates the distribution of life satisfaction ratings (which had a 100% completion rate) and breaks down the data for those living in the regenerated area as compared to those not living in the regenerated area. The mean life satisfaction ratings for the regenerated area and non-regenerated area are 6.60 and 6.32, respectively, and this difference is not significant.

It is important to note that within our sample we have three different population groups: A – those who live in a house and on a street which has been regenerated; B – those who live in the regenerated area but not in a house and on a street which has been regenerated; and C – those who live in the adjacent area which is not to be regenerated. Our two main analyses are: (1) comparing A and B with C; and (2) comparing A with B and C. For (1) we are interested in the well-being effect of living

[10] We can obtain a parametric WTP of the regeneration by regressing relevant independent variables on the WTP, and by using the coefficients to obtain the WTP value. The mean WTP value for the payment card from OLS therefore is:

$$WTP_{OLS} = \beta_0 + \sum_{j=1}^{n} \beta_j \bar{x}_j$$

where β_0 is the intercept, β_j is the coefficient on the j^{th} variable with the mean of that value given by $\bar{x}_j$. However, if the intervals are too coarse, OLS will be biased and it is preferable to use interval regressions (Whitehead et al, 1995). For these interval data regressions, the mean WTP value is (see Cameron and Huppert, 1989):

$$Ln(WTP_{INT}) = \exp(\beta_0 + \sum_{j=1}^{n} \beta_j \bar{x}_j) \exp(\sigma^2/2)$$

in the regenerated area as opposed to not living in a regenerated area. For (2) we are interested in the effect of living in a regenerated house and street as opposed to not living in a regenerated house or street irrespective of whether one is in the regeneration area or not.

The problem for (2) however, is that population B is expecting the regeneration in the future which might actually make them feel better and increase their well-being (for instance, Loewenstein, 1987, has found that individuals derive some utility in expecting a positive future outcome – see also Graham and Pettinato (2002) regarding positive expectations of upward mobility). Furthermore, there might be endogenous neighbourhood effects (Manski, 1993) from the regeneration on to the life satisfaction of individuals who have not yet had the regeneration, which might further complicate the analysis. That is, individuals who have had their house and road regenerated might feel better and therefore might be more likely to have social contact with neighbours that have not had their house or road regenerated, which might make those neighbours feel better. This is consistent with Topa (2000) who finds that local spillovers are higher in neighbourhoods with less educated workers. So we can then provide an additional analysis: (3) comparing A with C and omitting B. Our variable of interest becomes therefore the marginal effect of being in a regenerated house and on a regenerated road as compared to not being in a regenerated house and street. These are reflected in the regressions in Table 6.

Table 6 provides these three analyses where life satisfaction is regressed on key variables using OLS and omitting outliers using the approach suggested by Belsley et al (1980). Regression (1) in Table 6 relates to (1) above, which has the standard SWB function as seen in other studies. It is clear that being in the regenerated area significantly increases life satisfaction by roughly 0.5 points at the 5% level – in our data this is equivalent to roughly a third of the effect of being unemployed and looking for work. In keeping with existing evidence (see Dolan et al, 2008), the variables that are significantly associated with SWB are age, marital status, and unemployment. What is interesting here is that household income does not increase life satisfaction for this population group.

Regression (2) places all non-regenerated households in the control group, and it is clear that regeneration to house and street does not significantly improve life satisfaction. This result is complicated however by the fact that people who have expectations about future regeneration, and possibly gaining some life satisfaction as a result, are in the control group. Regression (3) omits this group, so we have a straight comparison between those who have had regeneration and those who will never have it in the foreseeable future. Now, the coefficient on regeneration is positive and the coefficient is larger than in regression (1).

As well as examining the sample as a whole, we have also restricted the sample to persons of working age (18 years of age to state pension age, which is 65 for males and 60 for females) on the grounds that the economic concerns of retired individuals are likely to be different to those of working age. Evidence from the life cycle hypothesis illustrates that wealth in old age is largely allocated to bequests (Menchik and David, 1983; Modigliani, 1986), indicating that the income received by older individuals will not be overly used for current consumption. This illustrates the problem of examining the income of older individuals in such datasets. Indeed, older individuals seem to care primarily about or place greater importance on their superannuation assets and pension income, and not about income *per se* (Heady and Wooden, 2004; Brown et al, 2005) in comparison to working age individuals.

In Table 7, the population in regressions (1), (2), and (3) are only those under state pension age. From regression (1), regeneration significantly improves life satisfaction by around 0.6 points. The logarithm of household income is also positive and significant, which means that we can calculate the IC from equation (7). For this sample population, the IC for urban regeneration is roughly £24,900. Regression (2) compares sample A with B and C, and given that B have future expectations, the IC from this function is lower at £17,400. Regression (3) compares A with C and values urban regeneration at £19,000. This value assumes that household income has the natural logarithmic form. If we assume a linear relationship between household income and life satisfaction, the IC here becomes £14,000.

It is also important to note that the urban regeneration might influence individuals' life satisfaction indirectly through other key variables. Such variables might be social

capital and neighbourhood negative externalities. For instance, regression 1 in Table 8 includes how often each individual speaks to family, friends and neighbours. It is clear that speaking to friends is important to life satisfaction although this association does not undermine the positive and significant regeneration result. However, by controlling for these factors reduces the income coefficient which generates higher ICs. Regression 2 includes the local negative externalities which might be reduced with urban regeneration i.e. levels of crime and noise from neighbours. Both variables are negatively associated with life satisfaction although significant at the ten per cent level. Overall, the effect of urban regeneration on SWB is largely independent of indirect effects, and the aesthetic appearance of the house and road directly improves an individuals' SWB.

One additional important variable could be relative income, which has shown to be important not only at the national level (Easterlin, 2001; Clark et al, 2008a) but also at the regional level (Ferrer-i-Carbonell, 2005: Luttmer, 2005). If some of the income effect is relative in our population group, controlling for the income of others would be expected to lead to an increase in the size of the income coefficient. The relative income variable in Table 8 is the natural logarithm of average annual income in both neighbourhoods (i.e. Hafod and Landore) with respect to age (i.e. <25 years old, 25-34, 35-44, 45-65, and 65>) and gender, giving ten reference groups. Regression 3 in Table 8 shows that by including relative income, the absolute income level effect slightly increases and the coefficient on relative income is negative (as in Ferrer-i-Carbonell, 2005; and Luttmer, 2005) although not significant. Nevertheless, the coefficient on regeneration remains roughly the same magnitude.

A potential problem with the life satisfaction equations above is that household income could be endogenous i.e. if life satisfaction depends on household income, and household income is itself a function of life satisfaction, then the parameter estimates are biased and inconsistent. Within our data, we have two possible instrumental variables that can be used; namely, whether or not your partner is in employment and whether or not you are in rented accommodation. Neither is a perfect measure and instrumental variables are notoriously difficult to find in happiness research (Knight et al, 2007; Oswald and Powdthavee, 2008a), but both can be used to give some indication of the problems with endogeneity

In Table 9, regression (1) uses regression (3) in Table 7 – i.e. the baseline regression – and regression (2) uses regression (5) in Table 8 – the full specification. For both regressions, an over-identification test suggests that the instruments are valid. The instruments are not weak in regression 1 although they might be in regression 2. Nevertheless, it is important to note that the coefficients on regeneration are roughly the same size (or slightly higher) but the instrumented estimates produce higher coefficients on household income – increasing the size of the estimated effect by between two-fold and three-fold. This increase in the magnitude of the income coefficient, which is also found in Luttmer (2005) and Oswald and Powdthavee (2008), suggests that the bias under OLS is negative, i.e. more satisfied individuals tend to work less to earn income. This has implications for our previously estimated income compensations. For the baseline case, our IC is £6,400 per year, while it is £7,600 for the full specification although the instruments may be weak for the full specification.

3.5 Discussion

From revealed preferences, it seems that the urban regeneration is not positively valued through house sales. These may be the least robust of all of our estimates. First, given that the regeneration is still occurring in some places within the regeneration area, and the fact that people may be reluctant to move from their regenerated house due to lack of mobility, it is unlikely that the housing market in this area would have fully cleared. There have been houses sold after regeneration has taken place so this mutes any self-selection effects. However, it is known that all houses and roads in the regenerated area will eventually be regenerated so the benefit of this should already be capitalised into house prices. It is important to note that the local council has stated that only those houses in the Hafod area are to be regenerated and not those in other surrounding areas like Landore – so there should be not be any expectation effects in house prices elsewhere.

Second, the regenerated and non-regenerated areas are within roughly the same housing market, so that people who would buy a property in one area would also consider buying in the other area since the areas and housing types are very similar.

Therefore, residents selling their homes in the non-regenerated area are likely to be aware that the housing stock is of better quality in the regenerated area, which imposes a negative externality on those living in the non-regenerated area in the form of private costs of improving the quality of their own homes.

Third, and possibly most plausibly, regenerated areas are known by locals as poor areas, and by naming these areas as 'renewal areas' provides a signal to society that there is a need for government intervention. So, it is very much probable that this signal creates a stigma effect. The effect of stigma on house prices has already been shown in other hedonic price studies of adverse environmental consequences (e.g. Kohlhase, 1991; Messer et al, 2006). There has been little research on how urban regeneration might create a stigma, although anecdotal evidence suggests that this is indeed the case (Robertson et al, 2008).

From stated preferences elicited through a CV survey, it seems that urban regeneration generates a positive benefit and is a non-market public good which individuals do want, with willingness to pay values at around £230 to £240 per year for three years. It is entirely possible that, in generating their WTP per month, respondents did not pay attention to the duration over which they would make this payment. Indeed, other studies have shown that the responses are insensitive to the payment period – i.e. temporal embedding (Stevens et al, 1997) – and so it likely that higher values would have been elicited from using a longer time frame over which payments would be made. If we assume that temporal embedding occurs and that people would be willing to pay each year for the average length of the time they live in one house (12 years according to the Department for Communities and Local Government, 2006), then the total WTP for the regeneration would be roughly £2,800.

The stated WTP values may be below their true values a result of loss aversion in the presence of mental accounting. Essentially, loss aversion can be applied to all negative departures away from the status quo (Bateman et al, 1997), hence individuals may recognise the benefits from the urban regeneration but they may not be willing to sacrifice a large proportion of their disposable income (i.e. the negative departure) for the regeneration. It has already been found that loss aversion can explain sub-optimal transactions in a marketplace (Knetsch, 1989) and a reluctance to upgrade durable

items (Okada, 2001). As a result, the benefit of the urban regeneration might be much higher than their unanticipated consumption budget (i.e. their mental account), and beyond this budget, individuals are far more motivated to avoid losing their income than they are to gaining the benefit from the regeneration (Thaler, 1999). Indeed, Bateman et al (2005) state that if an individual faces an unanticipated buying opportunity (i.e. the WTP choice) which they can finance only by foregoing some specific consumption plan, the act of buying the non-market good involves a definite loss, as distinct from the possible gain from the non-market good (e.g. urban regenration). This problem is highlighted in chapter 2 for preference-based valution.

The value of the regeneration estimated from SWB responses is around £6,400 (instrumenting for household income) to £19,000 per year (not instrumenting for household income). Assumptions about duration are also important for estimates based on the SWB ratings. It is possible that the life satisfaction ratings might incorporate individuals' past experiences and future expectations of the urban regeneration, which means the monetary value of £6,400 estimated from them should be treated not as a per year value but as a value weighted over a finite time horizon. If we assume an equal weighting over the average duration of occupancy, the annual IC would be £533. If we assume that the occupancy time is higher (which is not unreasonable since properties in these areas have a relatively low turnover rate), the IC value would decrease further. However, the occupancy duration would have to be twenty-seven years in order to equate the IC and stated preference WTP values.

The time frame over which gains in SWB are expected to last has not been addressed in any of the papers we are aware of, e.g. Blanchflower and Oswald (2004) and van Praag and Baarsma (2005) both assume that the ICs are annual and do not extend beyond the last or current year. However, it is unknown whether life satisfaction ratings incorporate the benefits or costs of a good or a circumstance from past experiences and/or future expectations. This assumption is crucial when applied to welfare appraisal, since the annual ICs would inherently double-count the benefits or costs of a non-market good and therefore would bias the cost-benefit analysis. Therefore, there would seem to be good grounds for viewing the ICs as a total value over a finite horizon. Clearly, the actual assumption made on how life satisfaction

incorporates future expectations is crucial to the methodology of the value of the non-market good by experiences, and merits further investigation.

For a robust CBA, there needs to be clearly specified benefits and costs for each year that the policy or program is present. From RPs and SPs, a significant value today counts as the benefit or cost today that can be discounted into the future, providing the values do not change over time. The question that needs to be asked about experiences is, do we see the SWB benefits as a one off benefit that can be discounted into the future, or do SWB responses already include the future through expectations? To take an example, if a non-market good made individuals' life satisfaction increase, is it because the good has made them feel good over the last ten years or is it because they feel that the non-market good will make them feel better next year? While we originally argued that the flexibility and autonomy of using a life evaluation measure make it appealing, the downside is that it might be too free an instrument for cost-benefit analysis.

An evaluative account is not specific over the time scale to which it applies, both in the language of the questions (some do not even give a specific time scale) and in the likely time scale which subjects would use when reporting about the general state of their life (even when a time scale is specified within the question there is little evidence to suggest that this is the time scale used by respondents) (Peasgood, 2008). This has rarely been discussed (notable exception is Levy-Garboua et al, 2007) as shown in chapter 2, but it is a very important feasibility issue for using SWB in a CBA. This area requires further debate and is similar to the argument of using SWB for health states. The current problem there is that it is very difficult to value life using SWB, whereas it is straightforward to do using preferences, although the nature of the number might not be accurate in itself. So there might be a requirement to change the end-point of the scale so as to make the very dis-satisfying life as being equal to a dead state.

A further consideration is the possibility that we have not controlled for a factor within our regression that is important to the SWB of the intervention group but not for the control group. A difference-in-difference estimate would correct this but given that these two neighborhoods are in a similar geographic location and that they are

both materially deprived neighborhoods, it is unlikely that we have omitted an important third factor, suggesting that our results are not spurious. A more likely explanation is that we have not correctly specified the well-being function with respect to income. If we have not fully captured the effect of income on SWB, and the true effect is much larger, the value from life satisfaction would be lower and would tend toward the value derived from the stated preference method over the duration of time spent living in the house. Furthermore, it must be noted that we started our SWB approach on the premise that life satisfaction is a reliable proxy for experiences. This is one of a number of ways however of measuring SWB, so we would need to know more about how such an intervention affects other measures of SWB.

Overall, and notwithstanding the relatively small-scale exploratory nature of this study, it seems that equation (8) does not hold in our quasi-experiment of urban regeneration. Nonetheless, the results, especially if replicated on larger samples and in other areas, have major implications for welfare economics and cost-benefit analysis. Within our urban regeneration context, if we assume that all the benefits from regeneration are captured by the WTP values from house prices, we could argue that this intervention, at least in the short to medium term, has no affect on well-being and is therefore an inefficient allocation of resources. If we assume that all the benefits from regeneration are captured by the stated WTP responses, the total benefit of urban regeneration for the households in the Hafod area would be £240,000. Given that the scheme to date has cost £10 million, this scheme has been a net cost and has not been an efficient allocation of resources.

Assuming that all the benefits are reflected in life satisfaction ICs (i.e. between £6,400 and £19,000), the total benefit of urban regeneration for the households of the Hafod area would be between £6.1 million and £18.1million. However, if we included longer term tangible benefits, such as employment and increased investment in the area, urban regeneration might prove to be worthwhile in the Kaldor-Hicks sense. We need more large-scale studies to suggest whether urban regeneration is efficient.

These results support those found by Metcalfe (forthcoming), who used another urban regeneration scheme in South Wales, where the intervention was randomised between home repairs (similar to this chapter) and house safety measures (such as more secure

doors and windows etc). Metcalfe found that the valuation of home repair through SWB is £8,100 (0.45 point increase in life satisfaction on an eleven point scale), and the valuation of home safety through SWB is £19,800 (0.74 point increase in life satisfaction). So the value of the safety measures was more than double of that from the home repair measures, but the safety measures cost about 30 times less. Our point estimates in this chapter are closer to the house repair marginal effect (it is slightly higher) than the house safety marginal effect. This makes sense as our intervention here is similar to the house repairs apart from the fact that more is spent on the exterior, e.g. roads and pavements, so the marginal effect should be higher.

To sum up, by using an urban regeneration intervention in a quasi-experiment context, we find that (revealed and stated) preferences and SWB do not equal one another. Stigma in revealed preferences, loss aversion the presence of mental accounting in stated preferences, and unspecified or unknown time duration in life satisfaction might explain some of the difference. We need much more research into the extent and the sources of the differences between these valuation methods and in different contexts.

Table 1: Percentage of resident population in our sample and that obtained from the 2001 National Statistics Census

	Hafod		Landore		Swansea
	Sample	Census	Sample	Census	Census
Regenerated area	61	62	39	38	N/A
Aged over 65	26	20	16	16	23
Employed full-time	33	31	39	41	35
Employed part-time	19	13	16	13	12
Self-employed	2	5	6	4	4
Unemployed – looking for work	6	5	4	4	4
Unemployed – not looking for work	13	11	10	12	10
Student	3	6	4	4	3
Retired	30	29	22	22	15
Single (including cohab)	30	30	33	32	30
Married	43	46	44	47	50
Separated	4	3	3	2	2
Divorced	11	12	12	11	8
Widowed	13	10	9	8	10
Owner occupied house	78	70	82	76	69

Table 2: Baseline hedonic regressions

	(1)	House price	(2)	Ln(House price)
	Coeff.	S.E.	Coeff.	S.E.
Regenerated area	1470.95	1374.35	0.028	0.036
Time trend	1033.08***	34.77	0.018***	0.001
Semi-detached	9520.10***	2374.70	0.122***	0.045
Freehold	3695.73	5412.58	0.042	0.103
One way road	6413.72***	2167.28	0.088**	0.039
Over looking a park	14689.57***	2366.37	0.215***	0.045
N	511		511	
Adjusted R^2	0.67		0.63	

Notes: ***,**,* represents significance at the 1,5 and 10% levels respectively.

Table 3: Robustness of hedonic regressions*

Dependent: House prices	Regenerated area			
Specification:	Coeff.	S.E.	Adj. R^2	N
(1) Baseline	1470.95	1374.35	0.67	511
(2) Pre regeneration	-30.04	1865.68	0.02	139
(3) 2002	-459.22	2046.64	0.05	116
(4) 2003	4707.93	2869.97	0.28	92
(5) 2004	4892.28	3253.02	0.27	97
(6) 2005	-86.65	2939.96	0.31	82
(7) 2006 & 2007	-1993.44	3083.54	0.12	124
(8) Post regeneration - Only comparing regenerated sales not areas	771.93	1890.71	0.67	511

Notes: ***,**,* represents significance at the 1,5 and 10% levels respectively. Each regression has the same controls as Table 2.

Table 4: Mean willingness to pay values (per year) (n=126)

Specification:	Mean	95% CI
WTP_{OLS}	£228	£192-£264
WTP_{INT}	£245	£209-£281

Table 5: Determinants of WTP values

Dependent:	(1) OLS WTP Coeff.	S.E.	(2) Interval Ln(WTP) Coeff.	S.E.
Regenerated area	-1.487	4.182	-0.203	0.242
Ln(Household income)	4.771	3.987	0.473**	0.228
Gender	4.685	4.288	0.80*	0.246
Age	0.763	0.709	0.100**	0.042
Age^2	-0.010	0.008	-0.001***	0.000
Married	-0.091	6.063	-0.289	0.347
Cohabiting	-12.701*	7.216	-0.505	0.408
Divorced	-11.384*	6.852	-0.807**	0.400
Separated	-15.817	9.705	-1.185**	0.572
Widowed	-4.056	8.509	-0.399	0.502
Employed part-time	-5.205	6.580	-0.077	0.375
Self-employed	7.924	9.430	0.363	0.529
Unemployed – looking for work	-8.723	9.189	-0.188	0.529
Unemployed – not looking for work	-9.022	7.464	-0.591	0.434
Student	-6.046	13.866	-0.063	0.795
Retired	-7.206	7.951	-0.233	0.467
Constant	-31.538	43.340	-3.684	2.497
Σ			1.187	
WTP	£228		£245	
N	133		133	
LogL			-313.439	
Adjusted R^2	0.16			

Notes: ***,**,* represents significance at the 1,5 and 10% levels respectively.

Table 6: SWB regressions – whole sample

Dependent: Life satisfaction	(1)		(2)		(3)	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Regeneration	0.477**	0.235	0.225	0.234	0.573**	0.243
Ln(Household income)	0.142	0.253	0.073	0.251	0.217	0.273
Gender	0.252	0.240	0.241	0.241	0.071	0.259
Age	-0.153***	0.041	-0.138***	0.040	-0.200***	0.041
Age^2	0.002***	0.000	0.001***	0.000	0.002***	0.001
Married	1.190***	0.336	1.192***	0.338	0.895***	0.346
Cohabiting	0.791	0.480	0.768	0.484	-0.012	0.533
Divorced	0.524	0.432	0.360	0.440	0.470	0.458
Separated	-1.604*	0.839	-1.503*	0.845	-2.783***	0.981
Widowed	0.025	0.513	0.378	0.502	-1.484***	0.563
Employed part-time	-0.085	0.182	-0.093	0.183	-0.098	0.177
Self-employed	0.527	0.690	0.698	0.663	1.767**	0.715
Unemployed – looking for work	-1.399**	0.610	-1.267**	0.588	-1.461**	0.643
Unemployed – not looking for work	-1.187***	0.425	1.316***	0.428	-0.680	0.450
Student	0.783	0.724	0.759	0.730	1.139	0.700
Retired	0.416	0.440	0.360	0.431	0.600	0.447
Constant	7.550***	2.690	8.072***	2.663	8.057***	2.876
N	305		308		244	
Adjusted R^2	0.18		0.18		0.25	

Notes: ***,**,* represents significance at the 1,5 and 10% levels respectively. Reference groups are Single and Employed full-time. Regression (1) compares populations A (living on a regeneration road) and B (living in the regeneration area but not on a regeneration road) versus C (living in control area). Regression (2) compares population A versus B and C, and regression (3) compares population A versus C.

Table 7: SWB regressions – working age

Dependent: Life satisfaction	(1)		(2)		(3)	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Regeneration	0.558**	0.259	0.556**	0.254	0.646**	0.276
Ln(Household income)	0.652**	0.276	0.841***	0.279	0.928***	0.307
Gender	0.178	0.292	0.246	0.284	0.163	0.314
Age	-0.127*	0.075	-0.096	0.075	-0.148*	0.080
Age^2	0.001	0.001	0.001	0.001	0.001	0.001
Married	1.082***	0.346	0.993***	0.338	0.653*	0.371
Cohabiting	-0.139	0.480	0.108	0.477	-0.915*	0.546
Divorced	0.941*	0.488	0.951**	0.476	0.640	0.518
Separated	-1.927**	0.759	-1.407*	0.783	-2.723***	0.974
Widowed	-0.160	0.786	-0.431	0.817	-0.730	1.122
Employed part-time	-0.020	0.182	0.065	0.381	0.236	0.418
Self-employed	1.906***	0.710	1.449**	0.653	2.468***	0.759
Unemployed – looking for work	-1.034*	0.612	-0.727	0.603	-0.773	0.667
Unemployed – not looking for work	-1.034**	0.446	-0.703	0.439	-0.243	0.483
Student	1.231*	0.676	1.346**	0.667	1.122	0.700
Retired	0.653	0.601	0.980	0.594	1.053	0.715
Constant	2.291	3.194	-0.231	3.256	0.105	3.563
N	229		225		187	
Adjusted R^2	0.24		0.23		0.23	
Average household income	£18,378		£18,578		£18,848	
IC for regeneration	£24,900		£17,400		£19,000	

Notes: ***,**,* represents significance at the 1,5 and 10% levels respectively. Reference groups are Single and Employed full-time. Regression (1) compares populations A (living on a regeneration road) and B (living in the regeneration area but not on a regeneration road) versus C (living in control area). Regression (2) compares population A versus B and C, and regression (3) compares population A versus C.

Table 8: Robustness checks for the SWB equations

Dependent: Life satisfaction	(1)		(2)		(3)	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Regeneration	0.738***	0.283	0.623**	0.283	0.652**	0.327
Ln(Household income)	0.704**	0.305	0.629**	0.304	0.729***	0.197
Gender	0.036	0.318	0.073	0.313	0.271	0.273
Age	-0.150*	0.080	-0.140*	0.079	-0.116	0.083
Age^2	0.001	0.001	0.001	0.001	0.001	0.001
Married	0.609	0.383	0.602	0.377	0.571	0.214
Cohabiting	-0.776	0.568	-0.691	0.560	-0.764	0.514
Divorced	0.128	0.499	-0.109	0.499	-0.118	0.641
Separated	-1.818	1.139	-1.361	1.134	-1.193	0.723
Widowed	-0.596	1.125	-0.503	1.107	-0.569	0.379
Employed part-time	0.020	0.424	0.025	0.417	0.059	0.515
Self-employed	2.651***	0.720	2.291***	0.720	2.289***	0.466
Unemployed – looking for work	-1.314*	0.687	-1.394**	0.676	-1.415*	0.616
Unemployed – not looking for work	-0.182	0.491	-0.305	0.486	-0.246	0.572
Student	0.916	0.682	0.877	0.671	0.991**	0.405
Retired	0.334	0.707	0.492	0.697	0.579	0.425
Speaking to family	0.126	0.197	0.157	0.196	0.165	0.218
Speaking to friends	0.421**	0.196	0.447**	0.196	0.467**	0.184
Speaking to neighbours	0.096	0.148	0.068	0.146	0.059	0.176
Crime			-0.241*	0.132	-0.239***	0.059
Noise from neighbours			-0.210*	0.115	-0.219**	0.080
Ln(Reference income)					-1.755	1.204
Constant	0.169	3.625	1.160	3.593	16.904	11.182
N	185		185		185	
Adjusted R^2	0.33		0.36		0.37	
Average household income	£18,986		£18,986		£18,986	
IC for regeneration	£35,200		£32,100		£27,500	

Notes: ***,**,* represents significance at the 1,5 and 10% levels respectively. Reference groups are Single and Employed full-time. Robust standard errors are adjusted for clustering at the cell level for reference income in regression (3).

Table 9: Instrumented regressions

Dependent: Life satisfaction	(1)		(2)	
	Coeff.	S.E.	Coeff.	S.E.
Regeneration	0.708**	0.290	0.811	0.531
Ln(Household income)	2.449***	0.891	2.418***	0.839
First stage *F* statistic	12.20		6.05	
First stage partial R^2	0.13		0.10	
Over-identification test	0.707	(p=0.401)	0.444	(p=0.505)
Average household income	£18,943		£18,986	
Income compensation	£6,350		£7,600	

Figure 1: Average house price every six months by area

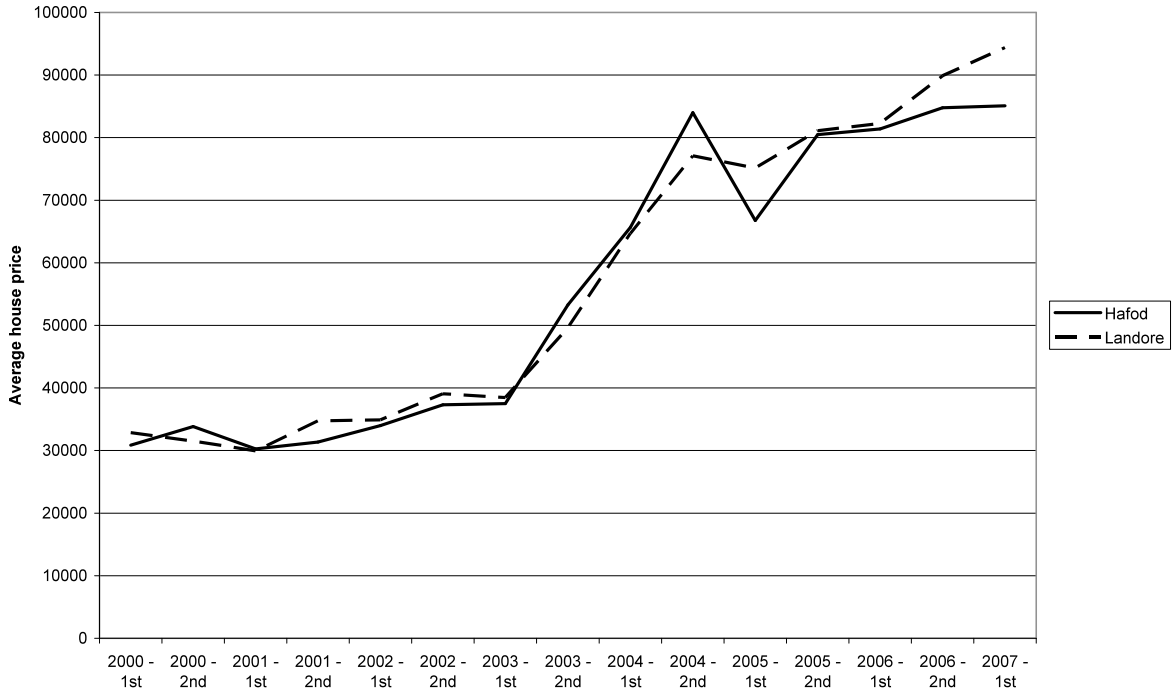


Figure 2: Distribution of WTP values

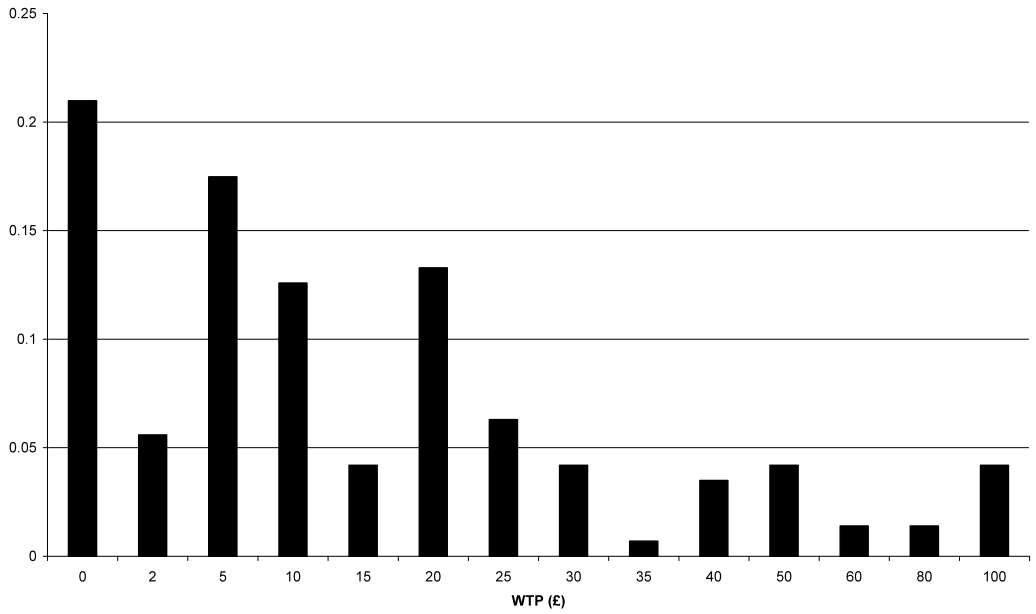
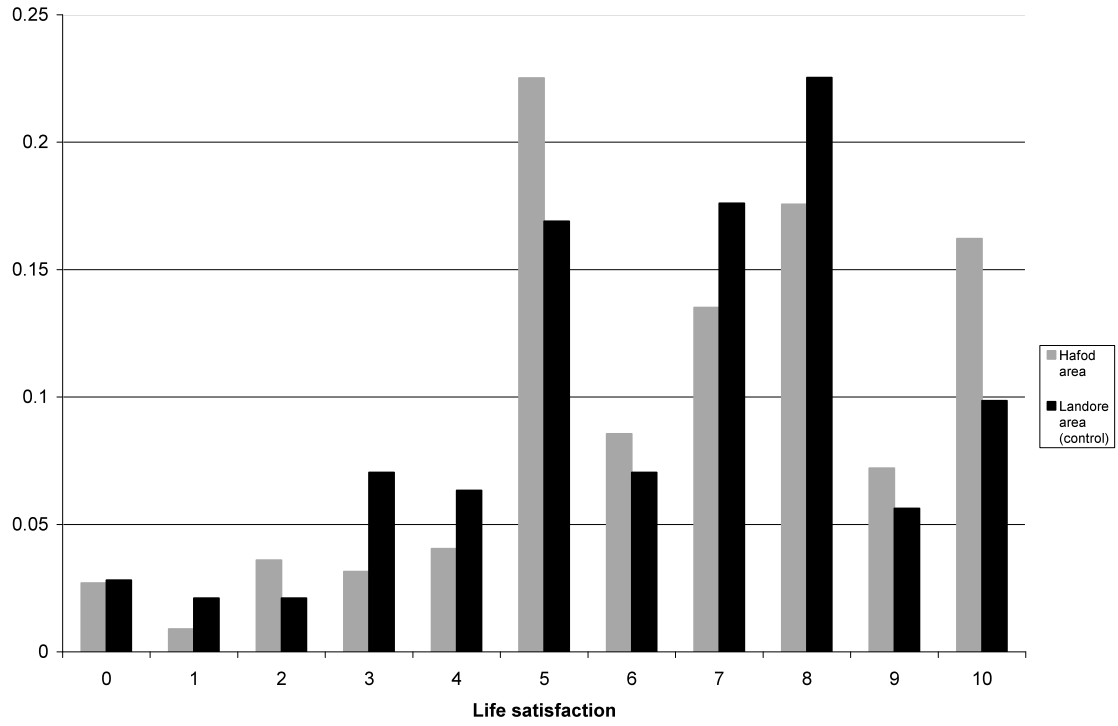


Figure 3: Distribution of general satisfaction by area



4. BLOWIN' IN THE WIND: THE VALUE OF A WIND FARM

4.1 Introduction

The previous chapter has shown that WTP and SWB might give different results in an urban setting although we require further research. That chapter was an ex post study only, so it assumes that the SWB levels pre-regeneration are the same. In order to develop that chapter further, we need to examine SWB levels before and after an exogenous shock. In addition, the previous chapter was based on an urban intervention, so examining a rural intervention would be complementary to the analysis. In this chapter, we examine the well-being consequences of a wind farm, by comparing preferences and experiences, where the experiences include both ex ante and ex post.

Wind farms have been recognised as having a major role to play in reducing greenhouse gas (GHG) emissions (IPCC, 2006), but the take up of wind power across the developed world has not been widespread. The majority of planning proposals for on-shore wind farms receive much opposition from local residents and lobby groups, which has been categorised as NIMBYism (Mitchell and Carson, 1986; Portney, 1991; Hamilton, 1993; O'Sullivan, 1993). In this context, the definition of NIMBYs are those individuals who are in favour of wind energy in general but do not want wind energy development in their local area. The NIMBYism is attributed to the fact that wind farms might cause local negative externalities, in that individuals living near wind turbines report negative visual aspect (Warren et al, 2005). NIMBYism is perceived to be rational since most people are homeowners, and individuals cannot insure assets like their homes against devaluation by the local development (Fischel, 2001).

The NIMBYism syndrome however leads to an inefficient allocation of resources unless those that receive the benefits (i.e. society) could compensate the neighbourhood around the wind farm site for bearing the external costs (Kunreuther et al, 1987). The problem here however is determining the correct level of compensation required by local residents to return their utility levels

back to the same level as before the introduction of the wind farm. SP studies have found that people do not want wind farm developments near their local area (e.g. Alvarez-Farizo and Hanley, 2002; Groothuis et al, 2008). Alvarez-Farizo and Hanley (2002) found that Spanish individuals are willing to pay between £15 and £35 not to have an onshore wind farm near their home and Bergmann et al (2006) found a willingness to pay of £19 amongst Scottish residents not to have an on-shore wind farm in the area (see also Ladenberg and Dubgaard, 2007, for Danish preferences).

The message from these studies is that people do not want wind turbines sited near their home, so they are not prepared to sacrifice their self-interest for the social good (assuming that wind farms are a social good). To follow on from the last chapter, we want to determine whether the value of this non-market good is the same when measured using a WTP measure and an IC measure derived from SWB. So our hypothesis will be:

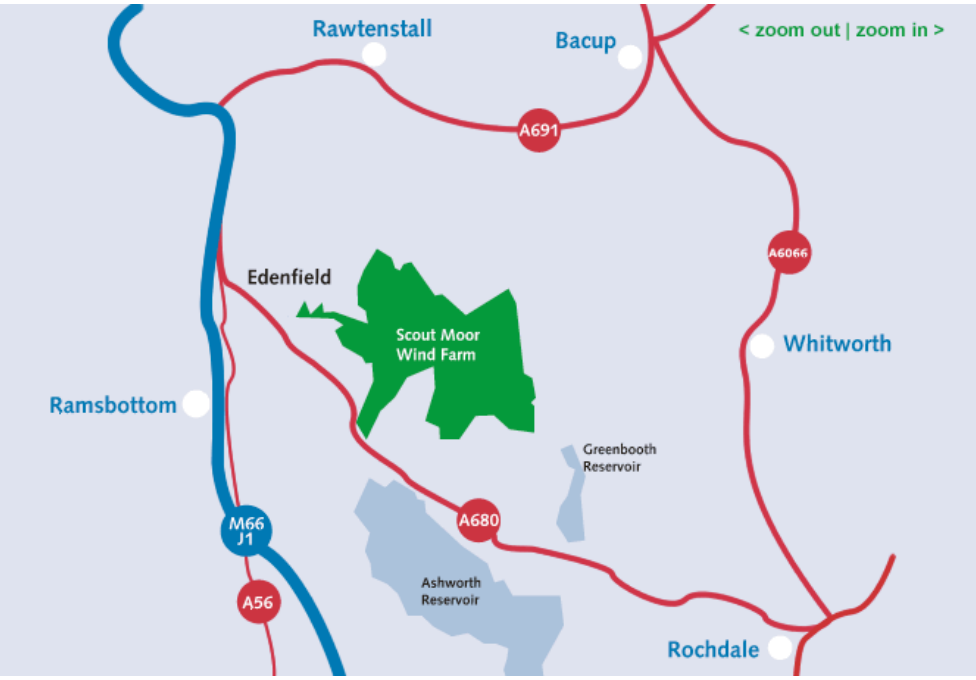
$$WTP_{RP} = WTP_{SP} = IC_{SWB} \quad (1)$$

4.2 Methodology

4.2.1 The non-market good

The non-market good we value is a local wind farm in the UK, named Scout Moor. The Scout Moor wind farm site is located in North West England, approximately 15 kilometers north of Manchester, on land between Edenfield and Whitworth, near Rochdale – see Map 1 below. The elevated moorland site is located in the Edenfield area, approximately 1.5km to the east of Edenfield and East Ramsbottom, 5.5 kilometers to the west of Whitworth, 3 kilometers to the south of Rossendale, and 11.5 kilometers to the north west of Rochdale. The wind farm can be seen from at most 35 kilometers away.

Map 1: The wind farm area



The Scout Moor wind farm consists of twenty-six wind turbines, each with a generating capacity of 2.5 Mega Watts (MW). The wind farm therefore has a total capacity of 65MW of electricity, which is equivalent to supplying energy to 40,000 households annually. This is equivalent to saving 160,000 tonnes of carbon dioxide emissions per year into the atmosphere. Each turbine tower is 60 meters high, and the blade length is 40 meters, therefore the total height is 100 meters. The wind turbines are distributed over a 545 hectare site, although the wind farm actually takes up 8.3 hectares of land within the area.[11] The local residents were fully informed by the private company of the exact details of the wind farm, and the information was freely available on the wind farm website and on the submitted planning proposal. As a result, there was little or no ambiguity about the provision of the good.

4.2.2 Identification

The identification of wind farms is not straightforward since it is difficult to randomise such goods across different places and people. Once a private firm

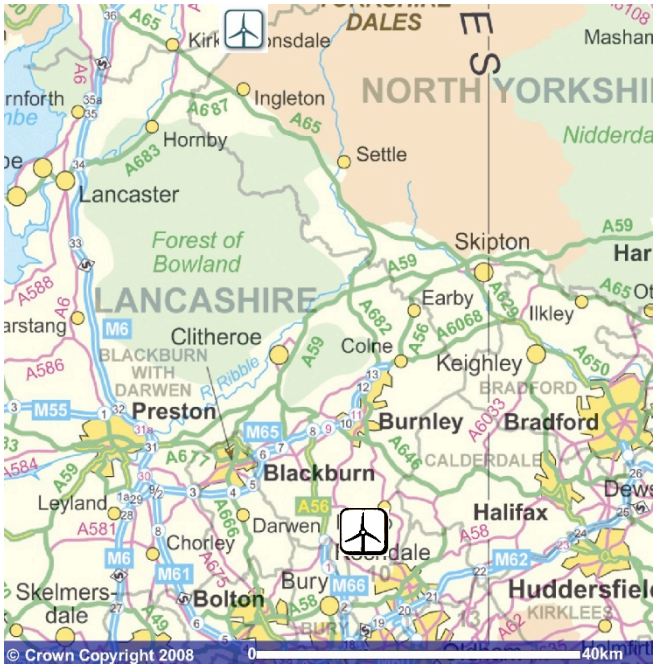
[11] More about the wind farm development can be found at www.scoutmoorwindfarm.co.uk/ and a short news documentary at http://news.bbc.co.uk/1/hi/england/7636696.stm

proposes a wind farm plan in the UK, the local planning authority/inspectorate has to either reject or accept the plan and then the Secretary of State for Energy has to make a decision based upon the planning authority's decision. So wind farm locations are chosen by private firms to increase the possibility of having their plan accepted, which effectively means that they aim to maximise potential energy captured (based on topography and wind speed) and therefore profit and minimise negative externalities. So wind farms might be allocated on where there is less local resistance to the wind farm, although there is no current evidence to support this claim. For Scout Moor wind farm, it is important to acknowledge that from the outset that the three local councils adjacent to the wind farm (i.e. Lancashire County Council, Rochdale Country Council and Rossendale County Council) did not accept the plan and were opposed to the wind farm being built. Given that these bodies represent the views of the local people, and because a local group set-up in opposition to the wind farm, we would argue that the majority of local residents were also against the proposal.

Since wind farm plans are assessed by local planning authorities, they cannot be randomized. But it is possible to say what localities nearby would have been likely candidates for a wind farm based on a certain criteria, thereby providing a matched 'control' area. Having a control group, which is similar to the treatment group apart from the fact that it does not have the treatment, allows us to have a quasi-experiment. The control group was selected based on the following factors: (i) it was more than 35 kilometers away so that the wind farm cannot be seen from the control area; (ii) it was in the same region and was a similar size town so to control for regional or locational effects; (iii) it was not in the vicinity of an existing wind farm so individuals were not affected by such an existing policy; (iv) it had the same topography so the hypothetical wind farm is a realistic proposal; and (v) it had the same wind speed so again the hypothetical wind farm was a realistic proposal. Based on these five criteria, we chose Kirkby Lonsdale as the matched control area, since there were not many areas in the North-West of England that satisfy even two or three of these criteria. Kirkby Lonsdale however satisfied all five criteria. Map 2 below shows the location of the two study areas. So to summarize, we have Edenfield as the intervention group and Kirkby Lonsdale as the control group. We also have a quasi-

intervention group, which is Rossendale (Rawtenstall) and Whitworth – this group cannot see the wind farm from their area due to the local topography, but they live very close to the wind farm and will be able to see it when travelling on main roads around the wind farm – see Map 1.

Map 2: North-West England and the two areas (intervention = Edenfield; control = Kirkby Lonsdale)



= hypothetical wind farm – Kirkby Lonsdale.
= actual wind farm - Scout Moor.

The timeline of the Scout Moor wind farm is as follows. The initial proposal by the private electric company was submitted mid-2003. Following a public inquiry held in 2004, planning consent was granted by the Secretary of State for Energy on the 25th May 2005 for the Scout Moor wind farm (BERR, 2006). Consent from the Secretary of State was required since to build and operate power stations with a capacity greater than 50 MW, private firms need consent under Section 36 of the Electricity Act 1989. The first turbine was delivered in November 2007, and all turbines were erected by June 2008. Full turbine generation started by September 2008.

To identify the well-being effects of wind farms, we need to identify the difference-in-difference (D-i-D) econometric specification for revealed preferences and SWB. Stated preferences do not need such a specification since the values are elicited ex ante before the wind farm has been experienced – this is one of the reasons why many non-market economists use such preferences (Mitchell and Carson, 1989). So for revealed preferences, we use OLS:

$$\ln(P_i) = \beta_0 + \beta_1 I_i + \beta_2 T_i + \beta_3 (I_i \times T_i) + \beta_4 x_i + \varepsilon_i \qquad (2)$$

where $\ln(P_i)$ is the natural logarithm of house prices, I_i is the intervention group, T_i is the intervention time period, (I_i x T_i) is the D-i-D estimator, and x_i is a vector of variables that are related to house prices, such as size of property, type of contract etc. If the wind farm has a detrimental effect on house prices, then $\beta_3<0$. For SWB, we have a two period model, so again we will have an OLS specification:

$$SWB_{it} = \beta_0 + \beta_1 I_{it} + \beta_2 T_i + \beta_3 (I_{it} \times T_i) + \beta_4 x_{it} + \varepsilon_{it} \qquad (3)$$

Given the panel data structure of (3), we can also estimate it by using a random effects model.

4.2.3 Measures of preferences and experiences

Given the strength of the opposition to the proposed wind farm, we assume that the wind farm will represent a negative change in utility. This is consistent with the previous research that suggests that people place significant positive WTP not to have a wind farm. Although some people maybe willing to pay to have the wind farm in their local area (and such values might be important for cost-benefit analysis – see Clinch and Murphy, 2001), the evidence suggests that it is mostly a negative externality in the local area. As in the last chapter, we ask a WTP measure based on equivalent variation. Initially, in both areas, individuals do not have property rights over the land and there is the possibility of a reduction in the quality of the local area. Individuals start off with private

consumption y_0 and w_1 of the public good, so we can ask them what loss of private consumption would be just as preferable as a decrease in the quality of the public good to w_2 (where $w_1 > w_2$).

Consistent with the last chapter and chapter 2, we used the payment card (PC) method for stated preferences. Respondents were asked to circle one value from a possible sixteen values, ranging from zero to one hundred pounds sterling. The question was: "*What is the maximum you would be willing to pay in increased electricity bills per year for 3 years for the Scout Moor wind farm not to be built?*". The context was obviously changed for the control group but the exact same question and values were used in this group.

For SWB, we asked individuals the following in both groups and in both time periods: "*Thinking about your own life and personal circumstances, how satisfied are you with your life as a whole?*", ranging from zero (completely dissatisfied) to ten (completely satisfied). This is consistent with the question used in the last chapter and of course consistent with the recent research in the area (Dolan and Kahneman, 2008; Luechinger, 2009), which was summarised in chapter 2. As with the last chapter, focussing effects were avoided by not asking any questions about wind energy. While there may be some framing issues with using SWB (Schwarz and Strack, 1999), it is certainly possible to reduce such framing by asking the SWB question first in the survey – again this methodology is consistent with that used in the previous chapter. We also asked a local area domain satisfaction question after the life satisfaction question. We conducted the first mail survey in September 2007 and the second mail survey in October 2008.

For revealed preferences, we use house price data obtained from market transactions. House price data are available online from the UK Land Registry (i.e. www.houseprices.co.uk), which also contains data on the type of house (flat, terraced, semi-detached or detached) as well as whether it is leasehold or freehold. We do not have data available on floor area (both internally and externally) and the number of rooms or bedrooms each house has. But both areas have very similar housing markets so there is a great deal of homogeneity

between the houses. Due to the fact that this is a quasi-experimental approach, we do not expect, a priori, a certain type of house size to be correlated with the sale of a property after the wind farm is constructed.

4.3 Results

4.3.1 Revealed preferences

In the time period from January 2000 to January 2009, there were 797 dwellings sold in the areas we sampled. Figure 1 shows the bi-annual average house price for each group. We can clearly see that the trend of each group is similar whether we examine the post-acceptance time period or the post-construction time period.

Table 1 presents the hedonic regressions using OLS. Regressions (I) and (II) assume that the time of intervention is when the wind farm proposal was initially accepted – i.e. June 2005. Regression (I) only includes the intervention group and the control group, whereas regression (II) also includes the quasi-intervention group into the intervention group. It is clear from these two regressions that the wind farm has no significant effect on house prices. Regressions (III) and (IV) follow the same format as (I) and (II), but here we change the intervention date to be when the wind turbines were erected – i.e. December 2007. Again, there is no effect of the wind farm on house prices. Regressions (V) and (VI) keep the same format as (III) and (IV) but here we reduce the time period, so we do not include any observations before it is known that the wind farm was accepted in the local area – i.e. before June 2005. Again, there are no significant effects, but these regressions provide us with a larger positive coefficient and a better determined standard error, although the t-statistic is around one.

Table 2 follows the same samples and specifications as Table 1, but here we include property characteristics in each regression. It is clear that by doing this, the D-i-D coefficients in each regressions are smaller, but we still end up with a t-statistic on our final sample of around one. It is clear however from both of

these tables is that the wind farm does not have a significant detrimental effect on house prices.

4.3.2 Stated preferences

In the first wave of surveys, we obtained 580 responses out of a possible 1600 individuals (36% response rate). The payment card for the annual WTP not to have the wind farm ranged from £0 to £100 per quarter. Table 3 shows the non-parametric WTP values, which only includes zeros that are genuine. The non-genuine zeros are those who stated a zero because of reasons beyond support and income, such as protest zeros like "the local council should pay" or "I pay enough electricity bills at the moment". For instance, there were 8% non-genuine zeros in the first wave.

For this analysis, we used annual WTP. Before the wind farm was constructed, the respondents in the wind farm area were willing to pay slightly less than the control group. Both of these areas were willing to pay more than the quasi-intervention respondents. So the range of WTP values were between £12 and £55, depending on what type of sample were asked. The intervention group consist of those who should be compensated in the traditional project appraisal sense, so their stated preference of £29 would be what is used for cost-benefit analysis. These values are similar to those already found in the literature. The parametric values provide very similar results to the non-parametric values stated here, and are available upon request.

4.3.3 Subjective well-being

While the first wave response rate was 36%, the response rates varied by area, with the intervention area obtaining a 39% response rate, the quasi-intervention area obtaining a 29% response rate and the control group obtaining a 44% response rate. Of these 580 who responded in the first wave, 416 responded in

the second wave (i.e. 72%) – which results in a panel sample of 26% of the possible 1600 residents – see Table 4.[12]

Table 5 provides the basic SWB regressions for the balanced panel. Regression (I) states the basic specification and shows that the wind farm does not impact on life satisfaction. Regressions (II) and (III) control for household income and age respectively, which increase the D-i-D coefficient although the standard error is not that well determined. Regressions (IV) and (V) control for employment status and marital status respectively, and these again have little effect on the D-i-D estimate. Regression (VI) is estimated by a random effects model that reduces the D-i-D coefficient. We also check these regressions by using fixed effects (except for regression (VI)) and the results do not change (and is supported by Hausman tests), suggesting that the unobservable variables are uncorrelated with the explanatory variables. These results are available upon request.

We next examine those individuals who were against the wind farm at the outset. Table 6 only includes those individuals who, in the first wave, stated that they were against the wind farm being built. Regressions (I) to (IV) examine the intervention group and control group only, whereas regressions (V) to (VIII) examine the intervention group and quasi-intervention group together against the control group. In seven out of eight regressions, the D-i-D coefficient is positive. Regression (I) is the only specification to show a negative coefficient, although it is not significant at the ten per cent level.

Even though the results suggest that the wind farm does not reduce life satisfaction, it is possible that it impacts on local area satisfaction. So we asked an area domain satisfaction in our survey after the life satisfaction question. Using the same scale, we asked how satisfied they are with their local area from zero to ten. Table 7 presents these regressions. As with the previous table, regressions (I) to (IV) examine just the intervention area and regressions (V) and (VIII) examine the intervention and quasi-intervention groups together. In each

[12] To note, there are no significant differences between those who did and did not respond to the second wave survey.

regression, the D-i-D coefficient is positive. In regression (IV) where we use random effects, the coefficient is positive and significant at the five per cent level.

We also asked and analysed psychological measures of well-being, such as autonomy, and more affective measures of well-being, such as worry. We found that the D-i-D estimate is positive yet insignificant. We also obtained behaviour of walking over the moor, to determine whether the wind farm affected the number of times people walked over the moor. Again, we find the exact same result as above in that the coefficient is positive although not significant at the ten per cent level, and these regressions are available upon request.

4.4 Discussion

This chapter illustrates that what individuals want through their revealed preferences and what they enjoy through their experiences are similar. It seems that wind farms do not have a negative value through the housing market or through people's life satisfaction. These results contradict what is found through people's stated preferences, where a significant negative value of a wind farm is found.

Therefore, on the one hand we have no effect of the wind farm through revealed preferences and life satisfaction, and on the other hand, we have a negative effect of the wind farm through stated preferences. These two differing values might have something to do with having two differing types of elicitation methods. The effect of the non-market good on revealed preferences and life satisfaction is found indirectly through regression analysis. For stated preferences however, the values are elicited directly, in that the residents are asked to think about the good independently (in contingent valuation). So their full attention is drawn to the good (and its relevant attributes) at that point in the survey. This attention towards the non-market good is not the same type of attention as when someone chooses to buy a house or when they evaluate their life.

It must also be noted here that revealed preferences require some sort of market experience and life satisfaction is clearly based on experience. Ex ante stated preferences however do not require any type of experience. For instance, the HM Treasury (2003) Greenbook makes no explicit reference to ex ante or ex post preferences, only WTP versus WTA. It is possible that there could be some differences to stated preferences over time, i.e. when the experience of a good directly impacts upon the value of the good. This is something that further research should focus on. For instance, if individuals were given the contingent valuation survey after the wind farm was built, would their stated preferences be updated by the experience of the exposure to the good? Is the experience somehow different to the standard information given in the description to the non-market good?

Beyond pursuing this further research, there are clear limitations to this research that require further investigation. For instance, the usual problems associated with RPs might be biasing our estimates, e.g. segmentation, in that it could be argued that these two areas are too far apart to be in the same housing market, which will therefore not allow us to gather good estimates on house prices. In addition, if the life satisfaction question was asked on the day that the first turbine was erected, would the results remain intact? Furthermore, would a more moment-to-moment measure of experienced utility provide any different result? These questions are of importance to future research in this area but are beyond the scope of this thesis.

Table 1: Hedonic regressions

	(I)	(II)	(III)	(IV)	(V)	(VI)
Dependent variable: Ln(House prices)						
Intervention group	-0.377***	-0.582***	-0.358***	-0.517***	-0.487***	-0.566***
	(0.055)	(0.057)	(0.051)	(0.054)	(0.070)	(0.076)
T_2	0.560***	0.560***	0.357**	0.357**	-0.084	-0.084
	(0.079)	(0.091)	(0.153)	(0.177)	(0.119)	(0.139)
Intervention group x T_2	-0.076	0.046	0.019	0.083	0.149	0.132
	(0.096)	(0.102)	(0.186)	(0.199)	(0.144)	(0.156)
Quasi-intervention group included	No	Yes	No	Yes	No	Yes
Intervention date – June 2005	Yes	Yes	No	No	No	No
Intervention date – November 2007	No	No	Yes	Yes	Yes	Yes
Time period	2000-2009	2000-2009	2000-2009	2000-2009	2005-2009	2005-2009
Household variables	No	No	No	No	No	No
R^2	0.28	0.29	0.12	0.13	0.24	0.18
N	498	797	498	797	178	297

Notes: Standard errors in brackets. ***, ** and * represent 1, 5 and 10% significance levels respectively.

Table 2: Hedonic regressions with household variables included

	(I)	(II)	(III)	(IV)	(V)	(VI)
Dependent variable: Ln(House prices)						
Intervention group	-0.365***	-0.603***	-0.328***	-0.552***	-0.478***	-0.667***
	(0.048)	(0.049)	(0.049)	(0.049)	(0.061)	(0.065)
T_2	0.583***	0.593***	0.428***	0.440***	-0.024	-0.023
	(0.065)	(0.076)	(0.133)	(0.153)	(0.093)	(0.112)
Intervention group x T_2	-0.042	0.005	-0.030	0.048	0.082	0.122
	(0.079)	(0.086)	(0.162)	(0.172)	(0.113)	(0.125)
Quasi-intervention group included	No	Yes	No	Yes	No	Yes
Intervention date – June 2005	Yes	Yes	No	No	No	No
Intervention date – November 2007	No	No	Yes	Yes	Yes	Yes
Time period	2000-2009	2000-2009	2000-2009	2000-2009	2005-2009	2005-2009
Household variables	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.52	0.51	0.34	0.36	0.56	0.49
N	498	797	498	797	178	297

Notes: Standard errors in brackets. ***, ** and * represent 1, 5 and 10% significance levels respectively. Household characteristics are: terrace, semi-detached, flat (the reference group being detached); freehold; and new build.

Table 3: Average WTP in each group

		WTP
Intervention group	Mean	£28.73
	s.e.	(5.13)
	95% CI	(18.60 – 38.86)
	N	187
Quasi-intervention group	Mean	£12.42
	s.e.	(3.14)
	95% CI	(6.22 – 18.61)
	N	183
Control group	Mean	£54.87
	s.e.	(8.26)
	95% CI	(43.55 – 76.19)
	N	158

Table 4: Response rates by group

	Pre-construction	Post-construction	Panel sample
Intervention group	39%	74%	29%
Quasi-intervention group	29%	64%	35%
Control group	44%	80%	19%
Overall	36%	72%	26%

Table 5: OLS life satisfaction regressions

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
Dependent variable: Life satisfaction (0-10)							
Intervention group	-0.478**	-0.522**	-0.359*	-0.270	-0.240	-0.224	-0.383*
	(0.214)	(0.222)	(0.212)	(0.212)	(0.212)	(0.214)	(0.202)
T_2	-0.024	-0.061	-0.100	-0.106	-0.094	-0.101	-0.103
	(0.222)	(0.232)	(0.218)	(0.217)	(0.216)	(0.131)	(0.233)
Intervention group x T_2	0.068	0.106	0.116	0.108	0.092	0.046	0.121
	(0.304)	(0.315)	(0.298)	(0.295)	(0.294)	(0.179)	(0.279)
Ln(household income)		0.144	0.426***	0.501***	0.340**	0.326*	0.487***
		(0.117)	(0.128)	(0.136)	(0.152)	(0.172)	(0.132)
Age			-0.068*	-0.076**	-0.091**	-0.069	-0.046
			(0.038)	(0.039)	(0.0402)	(0.050)	(0.028)
Age^2			0.001***	0.001***	0.001***	0.001*	0.001**
			(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employment status	No	No	No	Yes	Yes	Yes	Yes
Marital status	No	No	No	No	Yes	Yes	Yes
Quasi-interv group	No	No	No	No	No	No	Yes
Random effects	No	No	No	No	No	Yes	No
R^2	0.02	0.02	0.08	0.12	0.14	0.13	0.20
N	544	518	514	512	512	512	780

Notes: Standard errors in brackets. ***, ** and * represent 1, 5 and 10% significance levels respectively.

Table 6: OLS life satisfaction regressions – only including those who were against it at the outset

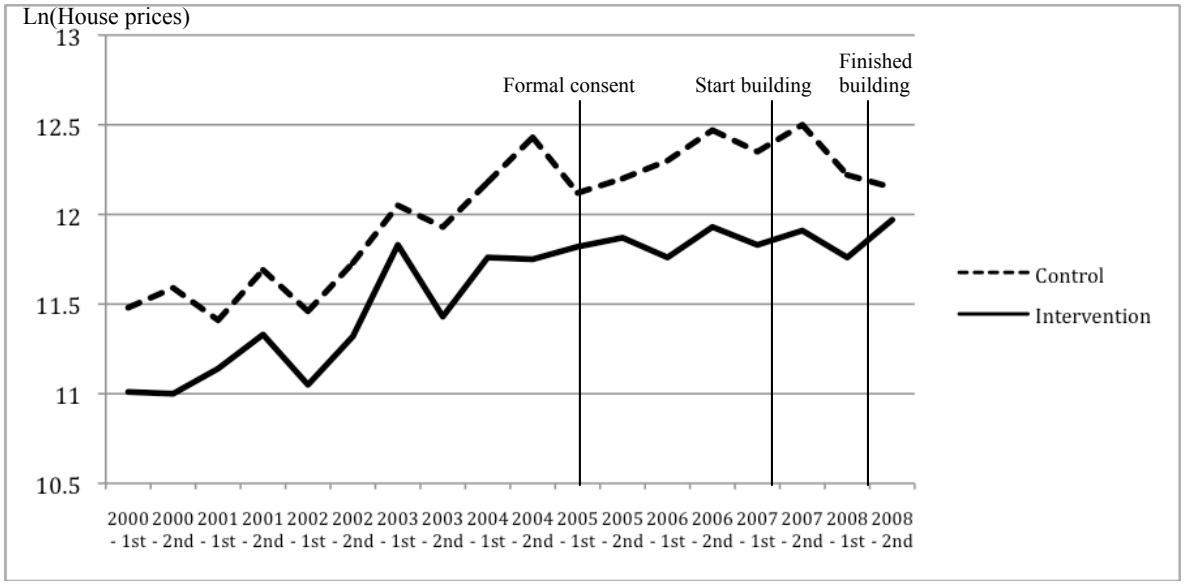
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Dependent variable: Life satisfaction (0-10)								
Intervention group x T_2	-0.064	0.042	0.036	0.039	0.035	0.165	0.158	0.149
	(0.361)	(0.350)	(0.349)	(0.349)	(0.313)	(0.324)	(0.320)	(0.320)
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income, age and age^2	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Employment status	No	No	Yes	Yes	No	No	Yes	Yes
Marital status	No	No	No	Yes	No	No	No	Yes
Quasi-interv group incl.	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.05	0.14	0.16	0.17	0.05	0.16	0.20	0.21
N	408	384	382	382	655	520	518	518

Table 7: OLS local area satisfaction regressions

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Dependent variable: Area satisfaction (0-10)								
Intervention group x T_2	0.271	0.286	0.297	0.313**	0.132	0.110	0.132	0.135
	(0.246)	(0.239)	(0.241)	(0.156)	(0.271)	(0.261)	(0.262)	(0.165)
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income, age & age^2	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Employment status	No	No	Yes	Yes	No	No	Yes	Yes
Marital status	No	No	Yes	Yes	No	No	Yes	Yes
Random effects	No	No	No	Yes	No	No	No	Yes
Quasi-interv group incl.	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.06	0.13	0.15	0.17	0.09	0.19	0.20	0.23
N	545	515	513	513	827	784	782	512

The full regressions are available upon request.

Figure 1: Natural logarithm of house prices by area (x-axis is bi-annual time period)



5. Does experience matter for non-market preferences?

5.1 Introduction

As was discussed in the last chapter, little is known about how stated preferences are affected by experiences over time. While there are many problems of using preferences (as presented in chapter two), such as inconsistency and framing issues, it could be that many of these problems result from the lack of experiencing the good.

List (2001) has found that market experience can reduce, but not totally diminish, the hypothetical bias of stated preferences, which suggests that experienced preferences should be used for welfare appraisal. This argument seems persuasive and would possibly have an impact on non-market preferences. The studies examining experiences to date however are usually cross-sectional and unable to show causality due to their between-individual approach (see Hoeffler and Ariely, 1999; Jansen et al, 2001; and Salkeld et al, 2000). So it might be that differences in experience might actually be underlying selection effects, in that those who are more likely to experience the good are those who are more likely to value the good in a particular way. So there could be some genetic or personality reasons for valuing a good (Roe and Haab, 2007). Essentially, experience is usually not exogenous, but endogenous.

While the non-market good literature is large, we are unaware of any studies that attempt to examine people's WTP for the same good before and after exogenously experiencing the non-market good. This is surprising given the amount of research on SPs. There is some evidence of combining stated preferences with revealed preferences to produce a panel dataset (Englin and Cameron, 1996), but this does not show whether WTP is affected a change in the experience of the non-market good. There is some evidence examining WTP versus willingness to accept (WTA) for the same good (Shogren et al, 1994; Morrison, 1997; and see chapter 2), but it is usually not based on how experience changes the value. It is more about how the endowment effect (i.e.

the reference point) affects preferences. The mere experience of the good in its exposure is distinct from reference points and endowment theory.

It has already been argued by List (2002) that more attention should be paid to the development of consistent approaches for estimating non-market goods, and if preferences are constructed during the elicitation process, then this might call for a re-evaluation of the fundamental building blocks of utility theory. A natural pre-cursor to this however is to determine how the experience of the good affects preferences. This is crucially important because policy-makers use such values for resource allocation decisions (e.g. HM Treasury, 2003) and assume these values to be valid and consistent. In this chapter we use the same non-market good as the previous chapter (i.e. the wind farm) to examine the role of experiences on preferences.

5.2 Background

Going back to the seminal piece by Arrow et al (1993), hypothetical choices in contingent valuation surveys must provide useful information about exactly what they are being asked to value if stated WTP is to mean anything. Familiarity with the non-market good may lead to less biased stated preference values (Cameron and Englin, 1997) although it does not eliminate the hypothetical bias (List and Shogren, 1998). It is important to note that in many applications individuals will have little experience of the good, suggesting the possibility that data from unfamiliar goods will always have significant noise. It has been found that information provision can influence WTP estimates (Hoehn and Randall, 2002), and information given in stated preference surveys may be significantly different to information that is given in real market transactions. For instance, when valuing clean air through SP surveys, respondents are usually given information about the effect that poor air quality will have on their health and the local environment. When purchasing a house or working in a building (i.e. RPs) with poor air quality, the agent is not informed of the effects of the poor air quality on their health or the environment. So the type of information that an agent has in ex ante SPs and ex post RPs would be different.

There have already been attempts to determine how ex post factors can shape choices. One such approach is that of regret theory (Loomes and Sugden, 1982; 1985), which postulates that individuals take into account the chance of experiencing disappointment and regret when making choices, so the imagination of the future experience of the good or decision might affect elicited ex ante values. So if SP values change over time as a result of experience, the expected regret is not borne out and as a result regret is distinct to experience. This regret theory is therefore different to pure experience since the latter determines the role of experiencing the good on valuation, whereas the former determines the ex ante imagination and expectation of the experience.

Another similar approach to experience is that of learning. Standard theory would suggest that a WTP value will reflect the consumption value and the information value of learning how the good can be incorporated into her preference set (Grossman et al, 1977). Experience is however distinct from preference learning since here individuals are not repeatedly bidding on a good to determine how much of this value is due to novelty or market experience. Shogren et al (2000) showed how individuals learn about buying the novel goods at multiple time periods, which affects their WTP values. Learning is something that does not necessarily arise with experience of the good. Learning can take place ex ante with more descriptive information about the good, and assumes that we cognitively process the information. The role of exogenous experience on the good does not make such an assumption and is different to that of the discovered preference hypothesis suggested by Plott (1996), where individuals play rounds of bids and through trial and error they discover what their preferences are. This is the role of market experience and not the role of exposure from direct experience of the good.

The effect of experience has empirical similarities with the test-retest literature, in that individuals are asked the same WTP question over a number of time periods. For instance, Brouwer and Bateman (2005) examine WTP over a five-year interval and find that there are significant differences between WTP values obtained at both time periods (as well as different determinants of WTP). There is no experience of the good however that could have affected these values.

It might be that experience has something to do with the hypothetical bias found in many stated preference studies (see Bishop and Heberlin, 1979; Carson et al, 1996; List, 2001), where the bias is upward in that it significantly inflates the WTP values. Due to the overall evidence in favour of the hypothetical bias, the National Oceanic and Atmospheric Administration's (NOAA) blue-ribbon panel recommend that hypothetical SP values should be deflated using a 'divide by two' rule (i.e. divide the WTP estimate by two) unless the hypothetical values bear some resemblance to values elicited from revealed preferences (NOAA, 1996).

More generally, lack of experience might be a reason as to why individuals might not have pre-existing preferences for such goods (Diamond and Hausman, 1994; DellaVigna, 2009). The possibility of the lack of consistent or experienced preferences may make individuals 'mis-want' non-market goods. It has been shown in chapter 2 that humans are not that good at predicting how future events or goods will affect their utility in the future (Gilbert et al, 1998; Gilbert and Wilson, 2007), so humans instinctively mis-want goods. Camerer and Loewenstein (2003), Kahneman and Sugden (2005) and Dolan and Kahneman (2008) have argued that revealed or stated preferences might not necessarily increase happiness and that systematic forecasting errors show that people often make mistakes in trying to predict how various actions will make them feel, and moreover these mistakes lead people to take the wrong actions (Loewenstein et al, 2003; Fudenberg, 2006). This affective forecasting is empirically tested in the chapters 6 and 7 of this thesis.

While the importance of experience has been stressed, most research in this area is cross-sectional which doe not always allow causality to be determined. This might lead to the possibility that a third factor might be driving an association between experience and value, such as genetics or personality. There will always be self-selection into preferred treatments as demonstrated by Roy (1951), and the exogenous role of experience on preferences is an area that has rarely been discussed. The seminal work of List (2003) has showed how market experience can reduce the hypothetical bias, but this work does not show how simply

experiencing the good affects individuals' WTP. In fact, there is a lack of research examining the within-individual effect of a non-market good on utility and a lack of research on the role of experience and learning in micro-level panel data (Agarwal et al, 2008).

Due to the possibility of a lack of experienced preferences and therefore mis-wanting (i.e. we want goods that do not increase our SWB), List (2002) has argued that much more attention should be paid to the development of consistent approaches for estimating the non-market benefits and costs of public programs. It would be first important to examine how experiences can shape stated preferences to establish whether experience should be taken into account for non-market valuation.

5.3 Methodology

5.3.1. Hypothesis

The non-market good we value is a wind farm (as described in the previous chapter). There are two time periods, t_1, before the wind farm is built, and t_2, after the wind farm is built. The first time period is a state of the world where residents have not experienced living near a wind farm. Hence their information set $I(a,e_1)$ is a function of a, which is a vector of the elements that determine how individuals gather information regarding the wind farm (such as attitudes, information shared by peers, risk perceptions etc.) and e_1, which is their actual experience of living near a wind farm.

The second time period is a state of the world where residents have experienced living near a wind farm, so their information set becomes $I(a,e_2)$, where e_2 is their actual experience of living near a wind farm. In this state of the world, everyone in the intervention area has experienced living near a wind farm. So essentially, we are assuming here that a change in the experience corresponds to a change in the information set (and further assume that a and e are orthogonal). Individuals' WTP not to have w, the wind farm, in stated preferences, will be

conditional on the information set. Therefore individuals' WTP, through their stated preferences in the first time period, WTP_{t_1}, would be different to their WTP through stated preferences in the second time period $WTP_{t_2}^{SP}$, with the change in the information set.

So we would like to test our null hypothesis that: H_1: $WTP_{t_1} \neq WTP_{t_2}$, i.e. WTP in both time periods are significantly different given the new information. This hypothesis has not been tested previously. Clearly, for our hypothesis and in accordance with the identification strategy, we will use a difference-in-difference (D-i-D) econometric approach. So to test our hypothesis, we would require the D-i-D estimator:

$$\ln(WTP_{it}) = \beta_0 + \beta_1 w_{it} + \beta_2 T_i + \beta_3 (w_{it} \times T_i) + \beta_4 x_{it} + \eta_{it} \qquad (1)$$

From equation (1), the estimated coefficient $\hat{\beta}_3$ will enable us to test our hypothesis. If $\hat{\beta}_3 = 0$, then we can reject our null hypothesis.

5.3.2 Measure of preference

As already demonstrated in the previous chapter, we use the PC method, asking individuals in the first wave: "*What is the maximum you would be willing to pay in increased electricity bills per year for 3 years for the Scout Moor wind farm not to be built?*". As before, the context was changed for the control group but the exact same question and values were used in this group.

In the second wave, the intervention area had the reduction in the public good without property rights over the land (that is w_2), but the control group did not have the same reduction in the public good. So the control area continued to be asked their WTP in the equivalent loss sense. So the WTP for the change in quality for the intervention group represents the compensating variation, since the loss in private consumption exactly compensates for that increase in quality. For this time period, the contingent question was: "*As you might be aware, the Scout Moor wind farm has been fully built. However, what is the maximum you*

would be willing to pay in increased electricity bills per quarter for 3 years for the Scout Moor wind farm to be removed?". Using WTP from equivalent variation and compensating variation should give very similar values (Willing, 1976). The exact same values in the payment card were used as in the first time period. We conducted the first mail survey in September 2007 and the second mail survey in October 2008.

5.4 Results

The sample is the same as that used in the previous chapter – highlighted in Table 1. The payment card for the annual WTP not to have the wind farm ranged from £0 to £400 per year (but this was asked quarterly). Table 2 shows the non-parametric WTP values for the unbalanced panel, which only includes genuine zeros. Before the wind farm was constructed, the respondents in the wind farm area were willing to pay slightly less than the control group. Both of these areas were willing to pay more than the quasi-intervention respondents. So the range of WTP values are between £12 and £60, depending on what type of sample are asked in period 1. The intervention group would be those who should be compensated in the traditional project appraisal sense, so their stated preference of £29 would be what is used for cost-benefit analysis.

In the second time period, on average all areas were willing to pay less to not have the wind farm than in this first time period. The differences here are non-parametrically insignificant. The parametric approach will rarely give significantly different values to the non-parametric values for the PC method – and indeed we find similar values for the parametric method (these results are not reported here although they are available on request). In order to determine whether the values from stated preferences change, we would like to use the balanced panel (i.e. those who responded to the survey before and after the building of the wind farm), and control for the factors that change over time that are related to the WTP responses. Table 3 does just this examining whether the natural logarithm of WTP (and keeping the legitimate zeros as zero and not as missing values) changes after the construction of the wind farm, i.e. the D-i-D

estimator ($w_{it} \times T_2$). In every specification and sample, the D-i-D coefficient is negative but insignificant.

The problem here is that there are a great deal of zeros which will bias the OLS estimation – some of which are individuals who were in support of the wind farm. So there might have been a change in stated preferences at the top end of the distribution but because of the frequency of individuals at the lower bound of zero, we do not observe a true effect of a changing SP value. We can address this in two ways. Firstly, through splitting our sample by concentrating only on those who were against the wind farm at the outset. This will exclude many zeros and focus our attention on those who had a strong preference not to have the wind farm. Secondly, through using a Tobit regression. We do both starting with the sample segmentation.

Table 4 analyses those who did not want the wind farm as represented by those who stated that they did not want it in their local area – a non-monetary preference. We use the exact same specifications and samples as that of Table 3. Once we control for income and age, WTP does change in the intervention (and quasi-intervention area) after the wind farm is built. So how much does their WTP change as a result of the experience? This depends on the actual specification that is used. Using only the control group and the full specification – i.e. regression (III) – we observe a decrease in WTP of around £48. This is found by multiplying the D-i-D coefficient by the average WTP in this sample (i.e. £60). Using the intervention and quasi-intervention groups together and using the full specification – i.e. regression (VII) – we observe a change in average WTP by around £38. So including the quasi-intervention group into the intervention group reduced the stated preference change but the result still remained significant. We also check all these regressions, as in the previous chapter, by using fixed effects (except for regression (VI)) and results do not change, and these results are available upon request.

These results however might still be biased by too many zeros, i.e. those who say they are against the wind farm but are not willing to pay anything for the change – so this might be just 'cheap talk'. We can further segment the sample

into those who were willing to pay something not to have a wind farm in the first wave – Table 5. The effect of preference change is even stronger here with WTP change being in the region of £89-£96.

We next examine the Tobit model, which allows us to control for the corner solution of zero in our data. The Tobit regressions are shown in Table 6, which include the whole sample, i.e. those for and against the wind farm. The D-i-D estimate for the intervention group only are negative but not significant at the ten per cent level. We found however that by including the quasi-intervention group, respondents' WTP decreases by between £42 and £60. Table 7 presents the Tobit regressions for those who stated a preference not to have the wind farm. It is clear that the D-i-D coefficients increase substantially here, which ultimately means that the WTP decrease will be greater. In every specification, the D-i-D estimate is significant at the ten per cent level at the very least. The decrease in WTP from these regressions ranges from £82 to £122.

Table 8 summarizes the changes of stated preference values. The OLS regressions seem to give lower values than that of Tobit regressions. It is also clear that when the intervention group is compared to the control group, i.e. omitting the quasi-intervention group, the value of change is higher. The intervention and quasi-intervention areas both reduced their WTP values, although the intervention group changed their values to a much greater extent. Clearly, our $\hat{\beta}_3$ coefficient from equation (1) is not equal to zero, so we cannot reject our null hypothesis and conclude that experience does matter to non-market valuation.

5.5 Discussion

The main result from this chapter is that stated preferences change after experiencing the good. We show that this is a robust result for different specifications and samples. This result is extremely important to economics and public policy since many governments use such preferences for public policy and believe that, to use Diamond and Hausman's (1993) title, 'some number is

better than no number at all'. The problem here is that the initial SP number is different to the SP number elicited after experiencing the good.

It has been recognised that projects or policies are capable of changing people's preferences in an endogenous sense (Elster, 1983; Sunstein, 1993; Bowles, 1998). A great deal of preference endogeniety is due to adaptive preferences, i.e. people adapt to the status quo. Clearly adaptation is a big part of behaviour, especially in the realm of behavioural economics (Madrian and Shea, 2001; Dar et al, 2004). Adaptive preferences raise thorny issues about whose preferences count and when do they count (Dolan et al, 2003). The research area of how perspective shapes individual WTP has gained little attention previously. If those who have experienced the good however, give a significantly different value to those who have not experienced the good, then this should clearly affect the allocation of resources. In the traditional sense, the experience of the non-market good is extra information that individuals would need to update their preferences in the Bayesian sense, so it would make sense to use ex post preferences in conjunction with ex ante preferences at the very least.

The problem with treating experience as information in the non-market sense is that this information can only be retrieved and used once the good is experienced on a day-to-day basis. One can provide individuals with as much information as possible, but it still might be insufficient due to the lack of day-to-day exposure. This is consistent with the recent research from psychologists, such as Barron and Erev (2003), Hertwig et al (2004) and Hau et al (2008), who argue that there is an increasing amount of research that suggests that cognitive processes and behaviour depend on whether information is acquired through personal experience or by descriptive summaries of events. The reasons range from notions of psychological distance (Trope and Liberman, 2003) to the fact that experienced information evokes the explicit learning system, as opposed to the implicit learning system (Koehler, 1996). Whatever the exact reasons, the role of experience on decision not only has large implications to the findings of prospect theory (as individuals tend to underweight low-probability events – see Hertwig et al, 2004), but also to the role of information provision in preferences for welfare appraisal. As a result, experience is a type of information that is

distinct to descriptive information, and it can only be found from ex post welfare measures.

The effect of experience here might be confused with hypothetical bias (Murphy et al, 2005), which may have arisen for strategic reasons. For instance, ex ante, people may state a large WTP so as to make a strategic move to make sure that the wind farm is not built (Throsby and Withers, 1986). This strategic bias is different to their monetary value from their underlying preferences of the wind farm. What is interesting in our example is that in the treatment area, the residents knew the wind farm was going to be built, so there would be little strategic reasons for stating a higher value. The control group however did not have the expectation of the wind farm, so this might be a major reason as to why their ex ante WTP was so much higher than the treatment area (see Table 2). The strategic bias however would not predict a difference in the WTP for the treatment group here since the residents knew the wind farm was taking place.

The current view in public policy is that ex ante SPs are placed on a pedestal to value non-market goods. From our research, however, this might not be the most efficient estimator of the value of the non-market good in question. Given the problems of valuing unfamiliar goods with preferences, trying to establish the effect of the non-market good on experiences to complement preferences seems a sensible avenue for academic research and public policy. It may also be important to determine whether asking the same question twice to the same individual over a period of time impacts upon their responses in the second time wave. For instance, in our example, if we asked individuals their preferences or experiences ex post without being exposed to the ex ante survey, would their answers remain unbiased? Do individuals remember the last time they completed a survey and anchor their response from that survey? Or does the mere act of remembering what the survey was regarding impact upon their stated preference or other such subjective data? These questions would be fruitful areas of research.

Table 1: Response rates by group

	Pre-construction	Post-construction	Panel sample
Intervention group	39%	74%	29%
Quasi-intervention group	29%	64%	35%
Control group	44%	80%	19%
Overall	36%	72%	26%

Table 2: Annual WTP not to have the wind farm: non-parametric

		Pre-construction	Post-construction	Difference
Intervention group	Mean	£28.73	£22.53	-£6.20
	s.e.	(5.13)	(5.91)	(7.88)
	95% CI	(18.60 – 38.86)	(10.84 – 34.22)	(-9.30 – 21.70)
	N	187	131	
Quasi-intervention group	Mean	£12.42	£8.86	-£3.56
	s.e.	(3.14)	(3.64)	(4.81)
	95% CI	(6.22 – 18.61)	(1.65 – 16.09)	(-5.91-13.03)
	N	183	135	
Control group	Mean	£59.87	£61.31	£1.45
	s.e.	(8.26)	(9.70)	(6.28)
	95% CI	(43.55 – 76.19)	(42.10 – 80.54)	(-26.59 – 23.69)
	N	158	112	

Table 3: OLS WTP regressions

	(I) Ln(WTP)	(II) Ln(WTP)	(III) Ln(WTP)	(IV) Ln(WTP)	(V) Ln(WTP)	(VI) Ln(WTP)	(VII) Ln(WTP)	(VIII) Ln(WTP)
Intervention group	-1.103***	-0.951***	-1.102***	-1.033***	-1.315***	-1.116***	-1.153***	-1.147***
	(0.263)	(0.266)	(0.269)	(0.269)	(0.208)	(0.211)	(0.215)	(0.216)
T_2	-0.098	-0.114	-0.150	-0.119	-0.117	-0.111	-0.135	-0.110
	(0.276)	(0.279)	(0.278)	(0.180)	(0.247)	(0.248)	(0.249)	(0.163)
Intervention group x T_2	-0.318	-0.366	-0.249	-0.206	-0.279	-0.337	-0.281	-0.283
	(0.374)	(0.376)	(0.375)	(0.243)	(0.294)	(0.295)	(0.296)	(0.192)
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income, age and age^2	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Employment and marital status	No	No	Yes	Yes	No	No	Yes	Yes
Random effects	No	No	No	Yes	No	No	No	Yes
Quasi-interv group incl.	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.09	0.15	0.19	0.18	0.12	0.18	0.20	0.20
N	493	465	463	463	757	717	715	715

Notes: Standard errors in brackets. ***, ** and * represent 1, 5 and 10% significance levels respectively.
This sample excludes non-genuine zeros.

Table 4: OLS WTP regressions

	(I) Ln(WTP)	(II) Ln(WTP)	(III) Ln(WTP)	(IV) Ln(WTP)	(V) Ln(WTP)	(VI) Ln(WTP)	(VII) Ln(WTP)	(VIII) Ln(WTP)
Intervention group	0.074	0.358	0.277	0.187	-0.335	-0.029	-0.075	-0.107
	(0.347)	(0.345)	(0.345)	(0.343)	(0.276)	(0.276)	(0.280)	(0.278)
T_2	-0.098	-0.117	-0.175	-0.150	-0.098	-0.110	-0.152	-0.133
	(0.303)	(0.299)	(0.295)	(0.209)	(0.287)	(0.282)	(0.282)	(0.199)
Intervention group x T_2	-0.802	-0.924*	-0.792*	-0.681**	-0.759*	-0.860**	-0.773**	-0.753***
	(0.496)	(0.487)	(0.480)	(0.341)	(0.391)	(0.384)	(0.382)	(0.271)
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income, age and age^2	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Employment and marital status	No	No	Yes	Yes	No	No	Yes	Yes
Random effects	No	No	No	Yes	No	No	No	Yes
Quasi-interv group incl.	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.02	0.12	0.18	0.18	0.05	0.14	0.18	0.18
N	357	335	333	333	485	456	454	454
Average WTP in t_1 & t_2	£58.75	£59.92	£60.28	£60.28	£48.26	£48.78	£49	£49
(95% CI)	(£48-£69)	(£49-£71)	(£49-£71)	(£49-£71)	(£40-£56)	(£40-£57)	(£40-£57)	(£40-£57)
ΔWTP		£55.37	£47.74	£41.05	£36.63	£41.95	£37.88	£36.90

Notes: Standard errors in brackets. ***, ** and * represent 1, 5 and 10% significance levels respectively. This sample excludes non-genuine zeros. Those who did not want it are those in the intervention area that stated that they did not want the wind farm built in their area – it is a stated preference not based on stating a monetary value.

Table 5: OLS WTP regressions

	(I) Ln(WTP)	(II) Ln(WTP)	(III) Ln(WTP)	(IV) Ln(WTP)	(V) Ln(WTP)	(VI) Ln(WTP)	(VII) Ln(WTP)	(VIII) Ln(WTP)
Intervention group	1.756***	1.767***	1.626***	1.637***	1.193***	1.317***	1.216***	1.249***
	(0.416)	(0.407)	(0.406)	(0.409)	(0.330)	(0.324)	(0.326)	(0.324)
T_2	-0.098	-0.123	-0.177	-0.144	-0.098	-0.116	-0.161	-0.135
	(0.296)	(0.292)	(0.287)	(0.205)	(0.289)	(0.284)	(0.283)	(0.203)
Intervention group x T_2	-1.339**	-1.359**	-1.248**	-1.278***	-1.445***	-1.506***	-1.411***	-1.478***
	(0.612)	(0.591)	(0.582)	(0.414)	(0.475)	(0.460)	(0.458)	(0.328)
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income, age and age^2	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Employment and marital status	No	No	Yes	Yes	No	No	Yes	Yes
Random effects	No	No	No	Yes	No	No	No	Yes
Quasi-interv group incl.	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.07	0.16	0.23	0.23	0.06	0.15	0.20	0.20
N	293	277	275	275	357	338	337	337
Average WTP in t_1 & t_2	£70.08	£70.87	£71.39	£71.39	£61.18	£63.91	£64.28	£64.28
(95% CI)	(£58-£82)	(£58-£83)	(£59-£84)	(£59-£84)	(£54-£75)	(£53-£75)	(£53-£75)	(£53-£75)
ΔWTP	£93.84	£96.31	£89.09	£91.24	£92.74	£96.25	£90.70	£95.01

Notes: Standard errors in brackets. ***, ** and * represent 1, 5 and 10% significance levels respectively. This sample excludes non-genuine zeros. Only included are those who stated a positive value in the first wave (i.e. pre-construction) not to have the wind farm built in their area – it is a stated preference based on stating a monetary value.

Table 6: Tobit WTP regressions

	(I) Ln(WTP)	(II) Ln(WTP)	(III) Ln(WTP)	(IV) Ln(WTP)	(V) Ln(WTP)	(VI) Ln(WTP)	(VII) Ln(WTP)	(VIII) Ln(WTP)
Intervention group	-2.689***	-2.182***	-2.217***	-1.813***	-3.249***	-2.511 ***	-2.504***	-1.988***
	(0.704)	(0.688)	(0.680)	(0.493)	(0.641)	(0.629)	(0.629)	(0.442)
T_2	-0.195	-0.259	-0.356	-0.252	-0.198	-0.273	-0.330	-0.250
	(0.694)	(0.679)	(0.661)	(0.377)	(0.716)	(0.699)	(0.689)	(0.402)
Intervention group x T_2	-1.470	-1.472	-1.229	-0.837	-1.884**	-1.914**	-1.793**	-1.337**
	(1.027)	(0.996)	(0.971)	(0.566)	(0.927)	(0.902)	(0.891)	(0.539)
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income, age and age^2	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Employment and marital status	No	No	Yes	Yes	No	No	Yes	Yes
Random effects	No	No	No	Yes	No	No	No	Yes
Quasi-interv group incl.	No	No	No	No	Yes	Yes	Yes	Yes
Log likelihood	-698.691	-656.908	-642.305	-622.533	-908.819	-850.793	-839.841	-825.966
N	493	465	463	463	756	717	715	715
Average WTP in t_1 & t_2	£42.76	£43.40	£43.59	£43.59	£31.16	£31.24	£31.33	£31.33
(95% CI)	(£35-£51)	(£35-£52)	(£35-£52)	(£35-£52)	(£26-£37)	(£26-£37)	(£26-£37)	(£26-£37)
ΔWTP					£58.71	£59.79	£56.17	£41.89

Notes: Standard errors in brackets. ***, ** and * represent 1, 5 and 10% significance levels respectively.
This sample excludes non-genuine zeros.

Table 7: Tobit WTP regressions

	(I) Ln(WTP)	(II) Ln(WTP)	(III) Ln(WTP)	(IV) Ln(WTP)	(V) Ln(WTP)	(VI) Ln(WTP)	(VII) Ln(WTP)	(VIII) Ln(WTP)
Intervention group	0.300	0.814	0.741	0.600	-0.321	0.293	0.259	0.221
	(0.682)	(0.655)	(0.639)	(0.549)	(0.586)	(0.568)	(0.561)	(0.470)
T_2	-0.181	-0.233	-0.332	-0.275	-0.182	-0.228	-0.299	-0.256
	(0.603)	(0.574)	(0.552)	(0.368)	(0.609)	(0.580)	(0.566)	(0.379)
Intervention group x T_2	-1.978*	-2.037**	-1.358*	-1.358**	-2.355***	-2.434***	-2.282***	-1.894***
	(1.013)	(0.956)	(0.919)	(0.971)	(0.866)	(0.902)	(0.803)	(0.539)
Balanced panel	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income, age and age^2	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Employment and marital status	No	No	Yes	Yes	No	No	Yes	Yes
Random effects	No	No	No	Yes	No	No	No	Yes
Quasi-interv group incl.	No	No	No	No	Yes	Yes	Yes	Yes
Log likelihood	-629.384	-583.645	-567.544	-549.735	-795.161	-735.580	-720.970	-702.296
N	357	335	333	333	485	456	454	454
Average WTP in t_1 & t_2	£58.75	£59.92	£60.28	£60.28	£48.26	£48.78	£49.00	£49.00
(95% CI)	(£48-£69)	(£49-£71)	(£49-£71)	(£49-£71)	(£40-£56)	(£40-£57)	(£40-£57)	(£40-£57)
ΔWTP	£116.21	£122.06	£81.86	£81.86	£113.65	£118.73	£111.82	£92.81

Notes: Standard errors in brackets. ***, ** and * represent 1, 5 and 10% significance levels respectively. This sample excludes non-genuine zeros. Those who did not want it are those in the intervention area that stated that they did not want the wind farm built in their area – it is a stated preference not based on stating a monetary value.

Table 8: Summary of preference change values

	Overall sample		Do not want it in t_1		Willing to pay something not to have it in t_1	
	Intervention group	Intervention & quasi-intervention group	Intervention group	Intervention & quasi-intervention group	Intervention group	Intervention & quasi-intervention group
OLS	N/A	N/A	£41-£55	£36-£42	£89-£96	£91-£96
Tobit	N/A	£42-£60	£82-£122	£93-£119	N/A	N/A

6. 'OOPS…I DID IT AGAIN': THE PERVASIVENESS OF THE FOCUSSING EFFECT IN WELL-BEING REPORTS AND ITS IMPLICATIONS FOR LONGITUDINAL STUDIES

6.1 Introduction

The previous chapter has shown that individuals' ex post preferences differ to their ex ante preferences in light of their experiences. We found this result using the same individuals over time, so they are exposed to two identical surveys. The problem here is that we do not know if the first survey may have shaped their ex post preferences, in that if they were only given the question ex post, would their WTP values be the same? This is something that has not been examined before, for either preferences or experiences. We try to show whether a second survey induces a focussing effect from the first survey, and we use the context of projection bias to determine this effect.

Chapter 2 has already shown the problem of projection bias resulting from incorrect beliefs for the usual economic model. That chapter demonstrated that contrary to what much of standard economic theory has to say, psychologists have shown that we are generally not very good at affective forecasting, which might more accurately be labeled defective forecasting. A number of studies have documented this well known result – ranging from kidney dialysis (Riis et al, 2005) to housing assignments (Dunn et al, 2003).

Much of the research on affective forecasting has used the same respondents over time. This allows us to control for individual heterogeneity but it does mean that respondents might use their first response as an anchor for their second response, either because they recall what they were asked to think about previously or because they simply recall their previous response.

To our knowledge, however, there have been no studies that have tested the degree to which having been asked about something before an event affects the

assessments of well-being after the event. To do this, we need an experiment in which respondents are randomised to the standard 'before and after' condition or to an 'after only' condition, thus allowing the 'after' responses of the two groups to be compared with one another. In this chapter, we report on two experiments that were designed to test the well-being forecasts of the impact of two major football games, the actual impact of the games and, crucially, whether having previously been asked about the games affects later well-being reports.

Whilst the context may appear somewhat specific, the implications of the results are important and much more general. If there is a difference in well-being between those who were asked twice and those who were asked only once, then continually asking people to state their well-being may induce focusing effects which renders well-being reports biased. This has major ramifications for not only clinical trials, but also panel data and non-market valuation. If respondents remember the topic of the survey they completed last year or remember what value they gave previously, a survey this year could bias those well-being reports. If this is found to be true, then a great deal of analysis on the British Household Panel Survey or the German Socio-Economic Panel datasets could be compromised.

Formally, we can define a person's predicted utility by:

$$U_i = U(u_1(g_i), u_2(z_i)) \tag{1}$$

where U_i is the predicted happiness of individual i, g_i is the outcome of the football game, and z_i is the other background variables which have an effect on happiness. If people place too much of a weight (i.e. focus heavily) on the outcome of the game in the prediction, and less on other things that happen in life (i.e. z_i), then we would have the following utility function:

$$U_i = U(u_1(g_i)^{\alpha_1}, u_2(z_i)) \tag{2}$$

where the weight α_1 determines the magnitude of the first focusing effect in the prediction.

At a later time period, we can observe the relationships of this utility function, which will be:

$$U_i = U(u_1(g_i)^{\alpha_2}, u_2(z_i)) \quad (3)$$

where α_2 determines the weight placed on the outcome of the game on the happiness of people seven days later. The affective forecasting literature would suggest that α_2 would be zero and there would be no weight given to the outcome of the game, so that $\alpha_1 > \alpha_2 = 0$. If the first survey has an effect on the happiness rating at a later time period, then we might find that $\alpha_2 > 0$ for group A only, i.e. a second pervasive focusing effect. Therefore, if the first focusing effect (affective forecasting) makes people mis-predict their utility, then $\alpha_1 > \alpha_2$, and if the second focusing effect (pervasive focussing) provides a subtle cue for individuals' answers, then $\alpha_2 > 0$ for group A but $\alpha_2 = 0$ for group B.

6.2 Experiment 1

Methods

We used the 2008 UEFA Champions League Final as our event, and supporters of the teams in the final as respondents. The context is important because football supporters are familiar with football games so they should be in a good position to predict how they would feel after the game. The Champions League is a competition involving the top football teams from all the domestic leagues around Europe. It is the biggest annual football tournament in the world and the 2008 final was the first final to be contested by two English teams, Chelsea and Manchester United football clubs. The final took place in Moscow on May 21 and was won by Manchester United on penalties.

To obtain a sample of Chelsea and Manchester United supporters, we conducted a randomised experiment on the social networking site Facebook (www.facebook.com). Using Facebook for our study meant that we could easily identify and select football supporters of both teams. Being an internet-based survey allowed us to could control the presentation of the questions to respondents (e.g. they could not 'flick through' the questions so they would not know what the survey is about). Only one internet protocol address was allowed

onto the survey and the survey could only be accessed from the link on the Facebook message meaning that individuals could only complete it once.

The process of randomisation was crucial for this study. On the Facebook website, there are various group pages that individuals can join so that they can signal to their peers that they support that group. So there are dozens of both Chelsea and Manchester United groups. We used the five most popular group pages for each team, and then randomly assigned individuals to be in either group A (where they completed the 'before' and 'after' survey) or group B (where they completed the 'after' only survey).

The difference between group A's predicted well-being and actual well-being after the event reflects any affective forecasting errors. The difference between group A's well-being before and after the event, and between group A's well-being before and group B's well-being after the event, shows whether the outcome of the event has an effect on well-being, i.e. a double focussing effect. So, the difference between group A's and B's well-being after the event shows the degree to which having previously been asked about something may affect later well-being reports.

Before the event we asked supporters, '*overall, how happy do you feel today?*' on a zero (not happy at all) to ten (very happy) scale. We then asked them to predict their well-being seven days after the final if their team had won and if they had lost. Given that it has been shown that the mis-prediction can be corrected to a certain extent by asking people to think carefully about the many other events that demand their attention in the future (Wilson et al, 2000), we further stated that "*It is very important to note that we are asking you how you will feel overall seven days after the final, rather than when you are specifically thinking about the result*".

We randomly selected 1400 supporters (50% from each team) and randomly assigned them to group A and group B. Group A were assigned 60% supporters, since we wanted to control for any attrition in this group, and consequently group B were assigned 40% supporters. For group A, from a possible 840

respondents on Facebook, we received 344 responses (159 Chelsea supporters). This represents a 41 per cent response rate, which is high for an internet survey (Sheehan, 2001). Of those 344 respondents, 240 stated their email address enabling us to contact them again for the follow up. Of those 240 who left their email addresses, we received 104 completed responses seven days after the final, and these 104 respondents (51 Chelsea supporters) therefore made up Group A. The timeline for this experiment is illustrated in Figure 1.

Results

From Graph 1, we can observe that Manchester United fans were happier on average than Chelsea fans before the final, although the difference is not significant. This is true of both the initial overall sample and our Group A sample (i.e. the 344 responses altogether). As a result, we will focus on the results of Group A as it is not statistically different to the overall sample and therefore eases description.

The predictions for a Manchester United win (which is what eventually happened) are also shown in Graph 1 for both Chelsea and Manchester United supporters. It is clear that Chelsea supporters predicted that if Manchester United won, their happiness would fall from 6.86 to 2.73, and Manchester United supporters predicted that if their team won, their happiness would rise from 7.09 to 8.68. Both happiness predictions are significantly different to the happiness ratings before the final at the one per cent level.

Seven days later we contacted the respondents again to take part in a general survey conducted by the University – there was no mention of football per se in the survey (so limiting any obvious focussing effects), only that they took part in a survey by the University seven days ago. We only elicited their happiness score, which for Chelsea supporters was 5.53 and for Manchester United supporters was 6.77. The ratings for each set of supporters had fallen from seven days ago, although the decrease for Manchester United supporters is not significant. This slight decrease may be a result of a great deal of anticipatory utility before the game, and then the loss of expectation after the game

(Loewenstein, 1987). The Chelsea difference of 1.33 is significant however at the one per cent level. So losing in the Champions league final seemed to have a detrimental affect on the happiness of Chelsea supporters seven days later.

Given these happiness ratings seven days later, we can clearly observe the affective forecasting result that many researchers have already shown. The difference between the predicted happiness and actual happiness seven days later for both sets of supporters was large and significantly different at the one per cent level. The difference for Manchester United supporters was around two happiness points and for Chelsea supporters it was just less than three happiness points (both on an eleven point scale).

While affective forecasting has been shown in many contexts, although not for football in Europe, no study has shown whether priming people about the study in the first instance affects the second rating. Group B (i.e. 'after new sample' in Graph 1) is our randomised control group who did not get exposed to or primed from the first survey, which allows us to test this hypothesis. We sent out 550 Facebook emails seven days after the final to group B and obtained 219 completed responses (108 Chelsea supporters) (i.e. a 40 per cent response rate).

As we can see from the last column of Graph 1, Group B Chelsea supporters gave a 6.67 happiness rating and Manchester United supporters gave a 7.00 happiness rating. Group B's happiness rating is not statistically different to that of Group A's ratings both before the final and seven days later. The happiness of Chelsea supporters in Group B was no different to that of Chelsea supporters in Group A before the final. The happiness between Chelsea supporters in Group A and Group B seven days later however was significantly different at the one per cent level. If there was no priming effect, given randomisation, then we would not expect a difference. This is not the case, and it seems that Chelsea fans in Group A systematically lowered their happiness ratings, more so than what would have otherwise occurred.

Overall, we found that both Chelsea and Manchester United supporters mis-predict their affective state seven days after the final, but that Chelsea

supporters' affective state did decrease seven days later – there was not complete adaptation. We found that the second group who had not been asked their affective state previously however had a different rating for their happiness. This suggests that people in the first group, who answered the survey twice, remembered what the survey was about first time around and focused their attention on this event. As a result, supporters did not only succumb to the focusing illusion at the prediction stage, but they also focused on the game seven days after by the subtle cue of re-taking the survey – i.e. a double focusing effect.

6.3 Experiment 2

Methods

The second experiment used the 2009 UEFA Champions League final between Manchester United and Barcelona on the 27th May 2009. Barcelona beat Manchester United two nil. For this experiment we analysed data from Manchester United supporters only. This experiment differs from experiment 1 by: (i) increasing the time when we provided the second survey; and (ii) controlling for any background variables that may be correlated with randomisation. For group A, we asked respondents their 'happiness today', which was one week before the Champions League final. Two weeks after the final, we asked respondents their 'happiness today', and then asked them to remember their happiness a week before the final took place. The survey then elicited respondents' gender, age, employment status and location, so as to control for any spurious differences in the background variables that resulted from randomisation that might drive such a difference found in experiment 1.

600 respondents were allocated into Group A (i.e. asked before and after) whereas 300 were allocated into Group B (i.e. after only). From Group A, we received 276 completed surveys, of which 224 left an email address so we could contact them again. Once we emailed them again to take part in a follow-up study, 105 from the 224 responded. From Group B, we received 103 completed responses.

Results

Table 1 presents the regressions for happiness two weeks after the Champions League final. The variable 'Group A' is whether the respondent was asked the survey before the final took place (i.e. before and after). The other variables are self-explanatory. It seems from regression (I) that being in the panel sample had no direct effect on happiness. Regressions (II) and (III) include the background variables of male age and age^2 and employment status respectively. Including these variables increases the size of the coefficient of the panel sample but does not significantly change the standard error. In these regressions age and unemployment explain a great deal of variance in happiness, as expected from the previous literature (van Praag and Ferrer-i-Carbonell, 2005; Dolan et al, 2008). Regression (IV) introduces regional variables and in doing so, the coefficient of the panel sample variable increases and is significant at the five per cent level. So by controlling for regional effects, along with other personal variables, we can show causally that those who were asked the happiness question previously were 0.66 points less happy than those who did not take the initial survey.

Table 2 is in the same format as Table 1 but here the dependent variable is remembered happiness one week before the final of the Champions League. Similar to the previous results in Table 1, the respondents who are unemployed, young and from Wales remember themselves as being less happy than those who are employed, older and from England. What is crucial here is the fact that the 'Group A' variable in every regression is significant. If we take regression (IV) as our best estimate of remembered happiness, we can causally show that those who were in the panel sample remember themselves as being less happy by 0.850 points than those who have not taken the survey previously. This coefficient is larger than that of the first table and it demonstrates that the double focussing effect is very prevalent when individuals are asked to think about a time period in which a negative event (i.e. losing the Champions League final) took place.

6.4 Discussion

In two randomized experiments, this chapter has clearly demonstrated that affective forecasting is prevalent in experienced situations (i.e. football matches) and that being exposed to an ex ante survey can shape ex post experiences. On the first finding, football supporters innately focused on and attended to the outcome of the game and did not think about the other things which might have been important in their life seven days later. The role of attention is crucial in this process (Kahneman et al, 2006). It helps to explain why many people believe the result will be important in their life after the final and why the longer-term effect of the Champions League final become relatively small (i.e. because attention eventually shifts away to other (more) important aspects of life). It has already been argued that finding ways to increase the accuracy of affective forecasts would be a valuable although complex task (Ubel et al, 2001). The results show that it is not enough to directly tell people not to think about an event when predicting their happiness scores. This study has shown that people still think about the game – it seems very difficult for them not to do so.

The second result clearly shows that being exposed to the initial survey biased the affective responses seven days later – this is the second focusing effect. Respondents who answered the survey twice (even though the second survey did not mention the Champions League final or even football) seemed to remember what the first survey was about and then base their second rating on this. Hence, they focused their attention on the first survey and responded accordingly to the subtle cue. This happened for the supporters of the losing team and not the winning team. So it might be that the losing team supporters are more likely to be affected by subtle cues or perhaps losing is more salient in the memory than winning (based on loss aversion – Kahneman and Tversky, 1979).

This is especially interesting since while loss aversion and the characteristics of the value function have been tested extensively for preferences and psychological perceptions (see Köbberling and Wakker, 2005; for an overview), "*the extent to which loss aversion is also found in experience is not yet known*" (Kahneman, 1999: 19). It might be that losses remain in the brain (or in the

subconscious) even after two weeks and the survey instrument primes the individual to think about the loss causing it to become salient again. On the other hand, it might be the case that gains do not remain in the brain since this higher level of SWB becomes a new reference point (or a very quick reference point). Perhaps priming individuals about the gain does not make the gain more salient and it might be that focusing attention on it may not matter.

If such a double focusing effect is replicated elsewhere, it is important that research on affective forecasting becomes more innovative in order to mitigate against the double focusing. Using different scales might not necessarily overcome this problem since it is not that respondents remember what they have given seven days ago and respond accordingly, but that people attend to the event seven days later and feel unhappy as a result. While the theory behind affective forecasting is clearly important, one cannot escape the fact that the methodology of deriving the answers to test the theory is just as important. By using two randomized experiments, we have shown that affective forecasting is more important than would have been the case without the randomization.

This research supports the evidence that subtle cues can have an affect on subjective assessments of life (Schwarz et al, 1987), and throws up interesting questions which merit further consideration and investigation. For instance, we have shown that the second focusing effect lasts seven days, but does it last longer? Do people focus on a survey that they responded to as long as one year ago and bias their current subjective well-being rating? Indeed, are life satisfaction ratings prone to long-term focusing effects in the same way as affective ratings might be? Finally, does the pervasiveness of focusing effects help to explain why people adapt to some life events and not to others?

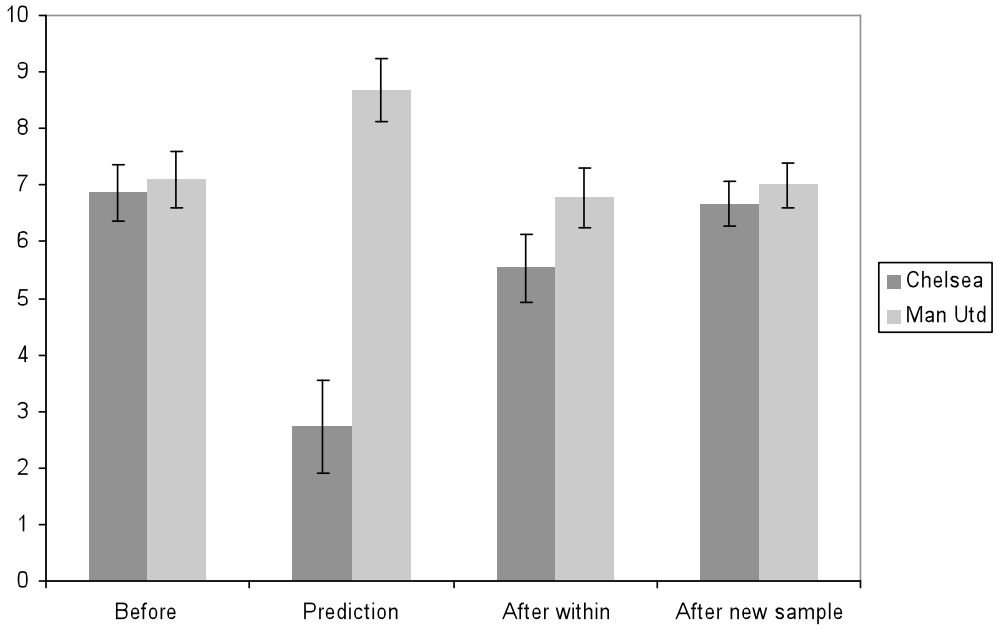
This chapter has shown that the focussing illusion is important for prediction, but that a backward focusing effect is also present which further supports the affective forecasting findings. As a result, this chapter is the first to show a double focusing effect which has implications for those conducting research on subjective well-being and self-reporting more generally. More research is

needed on both of these focussing effects in economics in order for us to understand economic behaviour to a greater extent.

Figure 1: Our methodology & timeline for experiment 1 (picture courtesy of guardian.co.uk)



Graph 1: Happiness before final, happiness predicted seven days after final, and happiness after final



Notes: Each individual error line represents the 95% confidence interval. The 'Before,' 'Prediction' and 'After within' groups contain the same 104 respondents – i.e. Group A. The 'After new sample' group – i.e. Group B – contains 172 randomized respondents.

Table 1: OLS happiness regressions

	(I) Happiness - two weeks after final	(II) Happiness - two weeks after final	(III) Happiness - two weeks after final	(IV) Happiness - two weeks after final
Group A	-0.298	-0.448	-0.494	-0.657**
	[0.303]	[0.313]	[0.307]	[0.327]
Male		0.043	-0.196	-0.262
		[0.397]	[0.391]	[0.390]
Age		-0.160**	-0.157**	-0.143**
		[0.072]	[0.071]	[0.072]
Age^2		0.002***	0.002**	0.002**
		[0.001]	[0.001]	[0.001]
Unemployed			-2.086***	-2.084***
			[0.524]	[0.523]
Student			-0.360	-0.392
			[0.403]	[0.403]
Retired			0.251	0.370
			[1.315]	[1.310]
Wales				-2.148*
				[1.223]
Scotland				0.859
				[0.684]
Ireland				0.567
				[0.639]
Other				0.431
				[0.349]
Obs	208	208	208	208
R^2	0.01	0.05	0.12	0.15

Notes: Standard errors in brackets. ***, ** and * represent 1, 5 and 10% significance levels respectively. Happiness is an eleven-point happiness today scale. Reference groups are ‘Employed’ and ‘England’. Other refers to countries outside of the UK and Ireland.

Table 2: OLS remembered happiness regressions

	(I) Remembered happiness a week before the final	(II) Remembered happiness a week before the final	(II) Remembered happiness a week before the final	(II) Remembered happiness a week before the final
Group A	-0.677*	-0.871**	-1.006***	-0.850**
	[0.344]	[0.358]	[0.353]	[0.373]
Male		-0.128	-0.307	-0.358
		[0.452]	[0.448]	[0.445]
Age		-0.122*	-0.115	-0.127
		[0.074]	[0.081]	[0.082]
Age^2		0.002**	0.002*	0.002**
		[0.001]	[0.001]	[0.001]
Unemployed			-1.862***	-2.000***
			[0.600]	[0.597]
Student			0.185	0.105
			[0.463]	[0.460]
Retired			-2.447	-2.583
			[1.509]	[1.494]
Wales				-3.845***
				[1.395]
Scotland				-0.16
				[0.780]
Ireland				-0.027
				[0.728]
Other				-0.532
				[0.400]
Obs	207	207	207	207
R^2	0.02	0.05	0.11	0.15

Notes: Standard errors in brackets. ***, ** and * represent 1, 5 and 10% significance levels respectively. Happiness is an eleven-point happiness today scale. Reference groups are ‘Employed’ and ‘England’. Other refers to countries outside of the UK and Ireland.

7. ELECTING HAPPINESS: DOES HAPPINESS AFFECT VOTING AND DO ELECTIONS AFFECT HAPPINESS?

7.1 Introduction

The previous chapters of this thesis have illustrated a degree of mis-wanting in non-market valuation. We take this context of comparing preferences and experiences to that of voting behaviour to see if voting preferences are consistent with their experiences of the outcome of the elections. Issues such as affective forecasting are usually found in small-scale experiments, so addressing some of these issues in public choice issues seems a very important area of research.

Voting in elections has long have been an area of research for political scientists and economists. Since Downs (1957), most models of why people vote – and what they get out of voting – are based on the assumption of economic agents that are rational according to an underlying set of preferences. Despite a great deal of work that builds on such models, voting and its consequences for individual welfare are still not very well understood (Green and Shapiro, 1994; Levine and Palfrey, 2007). Whilst the relationship between SWB and voting does not form a major part of existing formal models of voting, individual satisfaction is a significant part of the rational civic duty model of Riker and Ordeshook (1968). So its consideration represents one way to develop a greater understanding of voting and the impact of elections.

This chapter attempts to identify the causal relationships between voting and SWB (for more on possible associations, see Di Tella and MacCulloch, 2005). We test whether the result of elections affect SWB by using a panel dataset running over the three most recent national elections in the United Kingdom (which were all won by the Labour Party). The dataset contains information on life satisfaction.

7.2 Relevant literature and hypotheses

The Downs (1957) preference-based model of voting has stimulated a great deal of research, see Blais (2000) for a review. The model suggests that voting is costly to each individual, that no individual voter obtains direct utility from the act of voting itself, and that the benefits to society from the vote are discounted by the probability of casting a decisive vote. While it has been found that the potential decisiveness of the vote matters in large elections (Rosenthal and Sen, 1973; Silberman and Durden, 1975) and experimental settings (Levine and Palfrey, 2007), this result is not unequivocal (Ashenfelter and Kelly, 1975).

Importantly, the model significantly under-predicts the number of people that vote in elections. Myerson (2000) illustrates the problem of the decisiveness model using a Poisson distribution of random voters. He finds that the probability of voting pivotally for one candidate is so tiny that the benefit to a voter who prefers a candidate must be more than eight billion times greater than the cost to vote. If it costs $10 to vote in an election, then the expected benefits of electing one's preferred candidate must be greater than $80 billion. Extensions to the Downs model suggest that people obtain direct consumption benefits from voting due to civic duty (Riker and Ordeshook, 1968; Opp, 2001), which may provide its own contagion effect to vote (Shachar and Nalebuff, 1999). Social norms can increase voter turnout (Gerber and Green, 2000; Gerber et al, 2008) and the media can also play an important part (DellaVigna and Kaplan, 2007; Gerber et al, 2009).

It is possible to fit a theory of SWB into existing theories of rational voting in the Riker and Ordeshook (1968) approach. Their approach specifically introduced civic duty into the rational model, but they believed that civic duty was made up of: satisfaction from performing the civic duty; satisfaction from affirming allegiance to the political system; satisfaction from affirming a partisan preference (i.e. stand up and be counted satisfaction); satisfaction of deciding and going to the polls (i.e. procedural utility); and satisfaction of affirming one's efficacy in the political system.

There is therefore a great deal of SWB in this model i.e. if someone did not vote, they might experience dissatisfaction from not performing their civic duty and/or

affirming their partisan preference etc. Before we can see whether voting affects people's satisfaction, we should examine whether there are some differences in the satisfaction of those who intend to vote and those who do not. Therefore, the examination of how SWB affects voting intention is crucial for further insight into the Riker and Ordeshook model.

The research examining the impacts of voting (or an election) has usually used economic outcomes. For instance, Alesina and Roubini (1992) find evidence that there are electoral cycles on the inflation rate for a range of countries. For the U.S., there are mixed results on the effect of elections on economic outcomes. Grogan (1994), Knight (2000), and Besley and Case (2003) find an impact of elections on fiscal outcomes, and Snowberg et al (2007) find an effect on monetary outcomes. On the other hand, Lee et al (2004) show that voters elect policies proposed by the parties instead of affecting the policy positions of the parties, and Ferreira and Gyourko (2009) find that the outcome of mayor elections in major U.S. cities does not affect size of city government, the allocation of public resources, property, and crime rates.

Some work however has considered subjective assessments of welfare. Radcliff (2001) found that SWB is positively affected by left-wing governments i.e. liberal political systems, whereas – and somewhat contradictorily – Bjørnskov et al (2007) found that excessive government spending has detrimental effects on country aggregated SWB. Di Tella and MacCulloch (2005) find that when the government leans more to the right ideologically, right-wing individuals have higher SWB and left-wing individuals have lower SWB. Napier and Jost (2008) confirm this result although they attribute it to the fact that right-wingers are better able to justify income inequalities. Stutzer and Frey (2006) provide evidence from Switzerland to suggest that the opportunity to engage directly in the democratic process through referenda increases life satisfaction although Dorn et al (2007) have cast doubt on the robustness of this finding.

There has been no work to date that has directly tried to test the Riker and Ordeshook (1968) theory, i.e. that a large determinant of voting in the rational model of voting is that people get satisfaction, in various forms, from voting. In

addition, many of the studies mentioned above rely on cross-sectional data, suffer from potentially serious specification errors and selection effects and cannot establish causality. Given the lack of causal work in this area, we approach this important gap in the literature by directly examining how elections can affect SWB. More specifically, we aim to test the following two hypotheses. H_1: no effect of voting on SWB; and H_2: no effect of voting on SWB allowing for type and strength of political affiliation

7.3 Data and methodology

We use data from the British Household Panel Survey (BHPS), which covers the 1997, 2001 and 2005 general elections in the UK. The BHPS begun in 1991 and is a nationally representative sample of British households, which contains over 10,000 adult individuals. The entire sample of the unbalanced panel contains 30,336 observations (17,206 individuals). Of those, 4,197 stayed in all waves from wave 6 onwards (this is the first wave SWB ratings were elicited). The interviews for the BHPS take place between September and December of every year, and the general elections in 1997, 2001 and 2005 were in May, June and May respectively. So, the wave before an election is roughly six to nine months before and the wave after the election is roughly three to six months after the election has taken place. The SWB rating in the BHPS is based on a life satisfaction question: "*How dissatisfied or satisfied are you with your life overall?*"; with seven possible response categories ranging from "1. *very dissatisfied*" to "7. *very satisfied*". Due to the fact that 2001 did not have a life satisfaction question, we omit the 2001 general election from our analyses. As a result, we use four waves of the BHPS, i.e. 1996 and 1997, and 2004 and 2005.

To test whether the voting and the election affects SWB, we use a simple random effects model. Since the dependent variable is the life satisfaction variable, the average life satisfaction over time is not included as an error-correction term. For the first hypothesis, we would have the simple differences-in-differences model, which examines whether the election improves SWB:

$$LS_{it} = \alpha + \beta_1 V_i + \beta_2 T_{t+1} + \beta_3 (V_i \times T_{t+1}) + \sum_k \delta_k x_{k,it} + \varepsilon_{it} \quad (4)$$

where V_i is a dummy variable which is unity if the individual votes in the general election at time t and T_{t+1} is a dummy variable which is unity for the sample wave after the general election takes place. For the years before the election, the V variable takes the value of one if the individual votes in the forthcoming election. So for H_1, we test whether β_3=0. For the second hypothesis, we would have the equivalent of a difference-in-difference-in-differences model, where $P_{a,i}$ is the type of party preference, a, for each individual i:

$$LS_{itv} = \alpha + \sum_a \theta_a P_{a,i} + \beta_1 T_{t+1} + \sum_b \psi_b (P_{a,i} \times T_{t+1}) + \sum_k \delta_k x_{k,it} + \varepsilon_{it}, \; v = 1,0 \quad (5)$$

So we run two partial regressions, where we examine those who vote (v=1) and those who do note vote (v=0) separately. This is equivalent to extending a further interaction effect to equation 5. The second hypothesis suggests that voting in the last election has not effect on SWB allowing for political party preference, i.e. ψ_b=0. The k set of x explanatory variables included are those already found to affect life satisfaction from the literature e.g. sex, age, education, marital status, employment status, number of people in the household and household income (Dolan et al, 2008).

7.4 Results

As can be seen from Table 1, the expected turnout for each election is higher in the BHPS than the actual turnout. It might be that respondents in the BHPS are more inclined to act pro-socially and/or that they simply over-estimate their likelihood of voting but we cannot test for the relative weight of these possibilities. Table 2 breaks down the BHPS sample and the electorate for the three largest political parties. From 1997 to 2005, the representativeness of the BHPS in terms of the electorate has declined. We appreciate that these data are not perfectly representative of the British electorate but it represents the best

dataset available to conduct longitudinal analysis at the national level on the relationship elections and SWB.

Table 3 shows the various regressions that test hypothesis three. Unfortunately, the life satisfaction question was not asked in 2001 and so we cannot conduct a difference-in-differences analysis for this election. Regressions 3.1 and 3.2 present the random effects generalized least squares model to see whether the elections of 1997 and 2005 had any effect on SWB. Whilst the variables expected to affect SWB behave in the expected ways, voting in the previous election does not appear to have any effect on SWB. Distinguishing between the strength of party preference, that is between partisan (regressions 3.3 and 3.4) and weak (regressions 3.5 and 3.6), does not change the previous result.

What about when we account for political preferences? Table 4 splits the sample by those who voted and those who did not and also by strength of support. Regressions 4.1 and 4.2 show the results for the 1997 election for 'strong' and 'weak' supporters, respectively. The 1997 election had no effect on the SWB on either set of supporters – not even on the strong Labour supporters, whose party had been out of power for 18 years. Regressions 4.5 to 4.8 are analogous results for the 2005 election. This time, there are some odd results: non-partisan Labour voters reported significantly (but only marginally) lower SWB and those who had no political preference had higher SWB after the election. Overall, the election results do not appear to affect SWB very much at all but these odd results mean that, strictly speaking, we should reject hypothesis four.

7.5 Discussion

We showed that voting in the last two out of three elections in the UK have not in themselves affected life satisfaction and, when linked to political affiliation, they produce some odd results that suggest that elections generally have no effect on SWB. These results are consistent with the literature on affective forecasting but are contrary to Riker and Ordeshook's (1968) civic duty model. In order to test Riker and Ordeshook's (1968) model to a greater extent, one would need access to data on: whether the respondent expects that the opposition

party will make them more or less happy than the current party in power; and some subjective estimate of how much importance each individual places on the outcome of the election. These data are currently not available on any panel dataset around the world so we would welcome the inclusion of such questions within their surveys. One could then imagine conducting field experiments on varying individual SWB and eliciting their intention to vote and actual voting to clearly see the effect of SWB on voting.

In general, it seems that we have a tendency to overestimate the intensity and especially the duration of our reaction to events (Gilbert et al, 1998). The issue of incorrect forecasting found in the previous chapter is very relevant here. For example, Gilbert et al (1998) asked voters in Texas during the 1990 election for governor (which was won by George W. Bush) how they would feel one month after the election if their candidate had lost. Respondents expected to feel miserable but when asked how they felt one month later, they were as happy whether their candidate had won or lost.

One explanation for our defective forecasting is the focusing effect (Schkade and Kahneman, 1998; Wilson et al, 2000) whereby we over-estimate how much we will think about an event in the future. The very nature of thinking about something actually makes it appear more important than it will probably turn out to be. Many individuals may believe that the outcome of the election will affect them for much longer than it actually does because they imagine thinking about the election much more than they do.

While our results are informative, there are obvious shortcomings. We limit ourselves to the last three general elections in the UK, which were all won by the Labour Party and there were no real surprises in the election results – this is clearly very important. Of course, our results may be peculiar to the UK and to the measure of SWB we used. The analysis should be extended to other countries, to local elections and, in particular, to elections where the outcomes are much less certain and where decisiveness becomes an important factor. In particular, it is an open question whether mood on the day of the election is an important determinant of the propensity to vote and the voting preference, and

how this impacts on mood and SWB after knowing the outcome of the election. One could also examine the reverse causality, so as to determine how SWB might affect political preferences (see Clark and D'Angelo, 2009).

Nonetheless, we have shown that affective forecasting is prevalent in public choice and future research should examine other large-scale national effects in public goods, and how exogenous shocks in one country can impact upon preferences and experiences in other countries. For instance, did the recent US election victory by Barack Obama make UK individuals happier? Or does any event in another country have an impact on SWB in the UK? These questions are important for CBA but they are possible to find.

Table 1: Comparing intended voting turnout (in the BHPS) versus actual turnout

Turnout	Projected before election from BHPS*	Actual official turnout
1997 General election	82%	71%
2001 General election	75%	59%
2005 General election	73%	61%

***Are you planning to vote in next year's general election?**

Table 2: Comparing BHPS turnout preferences versus actual preferences

General election	Labour BHPS	Labour actual	Conservative BHPS	Conservative actual	Liberal Dem BHPS	Liberal Dem actual
1997	55.8%	43.2%	26.7%	30.7%	13.1%	16.8%
2001	47.9%	40.7%	20.5%	31.7%	12.0%	18.3%
2005	36.5%	35.3%	21.6%	32.3%	14.7%	22.1%

Table 3: The effect of the election on life satisfaction (LS) – random effects generalized least squares

	(3.1)	(3.2)	(3.3)	(3.4)	(3.5)	(3.6)
	All		Strong (partisan)		Weak	
	LS 1997 election	LS 2005 election	LS 1997 election	LS 2005 election	LS 1997 election	LS 2005 election
Vote in the last election	0.081	0.079	0.076	0.105	0.085	0.066
	[0.036]*	[0.023]**	[0.082]	[0.048]*	[0.040]*	[0.027]*
Election year dummy	-0.011	-0.109	-0.049	-0.115	0.001	-0.102
	[0.037]	[0.021]**	[0.084]	[0.047]*	[0.042]	[0.024]**
Vote in the last election x Election year	0.000	0.024	0.049	0.050	-0.014	0.004
	[0.042]	[0.026]	[0.092]	[0.052]	[0.047]	[0.031]
Regional dummies (20)	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies (2)	Yes	Yes	Yes	Yes	Yes	Yes
Background variables (31)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,745	26,010	4,384	9,389	12,361	16,621
R^2 overall	0.08	0.09	0.11	0.09	0.08	0.09

Standard errors in brackets. * significant at 5%; ** significant at 1%. [1] Background variables included in these regressions are: sex, age, age^2, education (7), marital status (10), employment status (9), household size, and the log of household income. The political reference group is the Labour party. We also include a variable to control for those who were ineligible to vote. The full table is available from the authors upon request.

Table 4: The effect of the election on LS allowing for partisanship and party political preference – random effects generalized least squares

	(4.1)	(4.2)	(4.3)	(4.4)	(4.5)	(4.6)	(4.7)	(4.8)
	Voted in the GE		**Not voted in the GE**		**Voted in the GE**		**Not voted in the GE**	
	Strong	**Weak**	**Strong**	**Weak**	**Strong**	**Weak**	**Strong**	**Weak**
Election year	**LS 1997**	**LS 1997**	**LS 1997**	**LS 1997**	**LS 2005**	**LS 2005**	**LS 2005**	**LS 2005**
Conservatives party affiliation	0.279	0.084	0.317	0.336	0.070	0.058	-0.189	0.121
	[0.082]**	[0.042]*	[0.268]	[0.130]**	[0.058]	[0.048]	[0.181]	[0.100]
Lib Dems party affiliation	-0.008	-0.034	0.317	0.130	-0.047	0.052	-0.138	-0.037
	[0.107]	[0.055]	[0.370]	[0.169]	[0.083]	[0.047]	[0.255]	[0.109]
Other party affiliation	0.127	-0.161	-0.427	-0.273	0.004	-0.050	0.048	0.024
	[0.231]	[0.095]	[0.568]	[0.193]	[0.081]	[0.051]	[0.162]	[0.093]
No preference	0.583	-0.154	0.249	0.136	0.520	-0.038	-0.214	-0.020
	[0.648]	[0.085]	[0.223]	[0.106]	[0.497]	[0.071]	[0.137]	[0.070]
Missing/refused/don't know	0.253	-0.139	0.446	0.507	-0.127	0.011	-0.054	0.129
	[0.276]	[0.061]*	[0.529]	[0.154]**	[0.202]	[0.048]	[0.283]	[0.081]
Election year dummy	0.033	-0.021	0.030	0.033	-0.034	-0.073	-0.316	-0.058
	[0.044]	[0.028]	[0.172]	[0.102]	[0.033]	[0.033]*	[0.110]**	[0.075]
Conservatives party affiliation x Election year	-0.040	-0.050	-0.285	-0.282	0.008	-0.068	0.218	-0.002
	[0.077]	[0.046]	[0.309]	[0.163]	[0.054]	[0.052]	[0.208]	[0.129]
Lib Dem party affiliation x Election year	-0.048	0.049	-0.299	-0.138	-0.004	-0.054	0.051	0.094
	[0.101]	[0.062]	[0.452]	[0.226]	[0.080]	[0.054]	[0.311]	[0.146]
Other party affiliation x Election year	-0.112	-0.022	0.081	0.690	-0.088	-0.025	0.275	-0.061
	[0.223]	[0.117]	[0.633]	[0.272]*	[0.048]	[0.057]	[0.160]	[0.117]
No preference x Election year	-0.399	-0.104	-0.158	0.018	-0.827	-0.014	0.286	-0.043
	[0.685]	[0.131]	[0.252]	[0.132]	[0.509]	[0.094]	[0.135]*	[0.086]
Missing/refused/don't know x Election year	-0.179	0.235	-0.267	-0.263	0.097	0.018	0.167	-0.042
	[0.272]	[0.082]**	[0.560]	[0.191]	[0.201]	[0.060]	[0.296]	[0.106]
Regional dummies (20)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies (2)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Background variables (31)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,362	9,698	757	2,201	7,233	10,394	1,666	5,656
R^2 overall	0.12	0.08	0.17	0.12	0.01	0.01	0.01	0.02

8. DESTRUCTION AND DISTRESS: THE IMPACT OF THE SEPTEMBER 11 ATTACKS ON BRITISH WELL-BEING

8.1 Introduction

Throughout this thesis, we have been primarily concerned with life satisfaction (or happiness) measures of subjective well-being and changes in goods that have been predicted/anticipated. Urban regeneration, wind farms, football matches, and voting and elections all have a very predictable element to them. So questions arise about whether we can use other SWB measures to value non-market goods and can we value unpredictable/unexpected changes in non-market goods. Furthermore, so far we have only attempted to value non-market goods in the UK, but another interesting issue is whether we can use SWB to value international externalities.

In this chapter, we use the example of terrorism, which is a major negative externality (Frey, 2004). Some costs of terrorism are very direct and relatively easy to measure, such as the value of lives lost, reduction in consumption, etc., whilst others are more indirect and much more difficult to measure, such as increased fear and anxiety. One of the most well-known terrorist attacks occurred on September 11th 2001 in the US. It has been shown that the attacks had detrimental effects to the economy in the US, particularly in the New York region (Chernick and Haughwout, 2006) as well as intangible psychological costs (Galea et al, 2002; Schlenger et al, 2002).

The indirect effects of terrorism, which might be large in their own right, could extend beyond national borders as they dominate media coverage (Eisensee and Stromberg, 2007). It is very difficult to identify the causal effects of terrorist attacks on individuals, regions or countries since there are sometimes no good comparable counterfactuals. As a result of this, one way of valuing the indirect negative externalities would be through a stated preference study. Smith et al (2009) analysed US households' ex ante WTP for three security

policies that all address a terrorist attack on commercial aircraft with shoulder mounted missiles. The main policy being proposed was anti-missile laser jamming countermeasures mounted on commercial aircraft, and this was compared to two other policies as well as the prospect of remaining with the status quo. Their WTP estimates for the anti-missile laser jamming intervention ranged from $100 to $220 annually per household. Using a random utility model Viscusi (2009) finds that reductions in deaths from terrorism have a value almost twice as great as reductions in deaths from natural disasters, suggesting a large premium for dread risk. To identify the international negative spillovers of the 9/11 attacks in the UK, we could ask the UK population how much they are willing to pay to eliminate the risk of terrorist attacks in another country, such as the US.

This chapter however presents one of the first attempts to estimate an international spillover of terrorism using people's SWB. We use the British Household Panel Survey (BHPS) to examine whether the 9/11 attacks in the US had an impact on the SWB of residents in the UK. The BHPS allows us to examine the 9/11 attacks in a quasi-experimental setting. The BHPS is administered annually between the months of September and December, but the majority of surveys take place in September in a random manner. So comparing the SWB of the UK population before and after the 11th of September in 2001, and comparing this to the same residents in 2000, provides us with a novel and powerful quasi-experiment.

8.2 Background

Terrorism and terror attacks have long been a major international problem, with potentially serious consequences for human welfare (Frey et al, 2007). The attacks of September 11th 2001, were one of the most prominent acts of terrorism in recent times but we need to determine the consequences of such attacks. Economists use the underlying exogeneity of terrorist attacks as a way of establishing the causal relationship between those attacks and various economic outcomes, such as tourism (Enders et al, 1992), national output (Abadie and Gardeazabal, 2003; Eckstein and Tsiddon, 2004), net foreign

direct investment (Abadie and Garzeazabal, 2008) and urban expansion (Blomberg and Sheppard, 2007). Terrorism however, only directly affects a small fraction of the capital stock (Becker and Murphy, 2001), and there are also studies which show that it does not affect all economic outcomes. For example Glaeser and Shapiro (2002) find that terrorism has not altered the urban form. The well-being consequences of terrorism have also been studied in terms of the birth weight of babies in areas with a higher concentration of land mines, where the causal mechanism is thought to be the effect of stress on mothers during pregnancy (Camacho, 2008).

The terrorist attacks of September 11th 2001, have stimulated quite a bit of research in their own right. For example, there is now evidence to suggest that the attacks had a detrimental effect on the financial market (Chen and Siems, 2004; Straetmans et al, 2008) and New York's fiscal position (Dolfman and Wasser, 2004; Chernick and Haughwout, 2006). It has also been shown that the 9/11 attacks reduced the demand for air travel (Blunk et al, 2006; Blalock et al, 2007), with estimates ranging from $14 to $43billion a year (Santos and Haimes, 2004) to $214 to $420 billion (Gordon et al, 2007). There was also a significant increase in the number of fatal traffic accidents after 9/11 (Gigerenzer, 2004; Su et al, 2009), which has been found for other terrorist attacks (Stecklov and Goldstein, 2004).

In terms of the intangible effects of 9/11, it has been found that survivors from damaged buildings reported substantial physical and psychological health problems three years after the event (Brackbill et al, 2006). Post-traumatic stress disorder (PTSD) has been shown to be associated with direct exposure to the 9/11 attacks and the prevalence of PTSD in the New York City metropolitan area was substantially higher than elsewhere in the country (Galea et al, 2002; Schlenger et al, 2002). Eidelson et al (2003) find a significant increase in the amount of work – in terms of the number of clients – received by psychologists working closest to Ground Zero compared to those received by their colleagues working elsewhere in the country.

The intangible effects of 9/11 were felt elsewhere in the US. For example, PTSD was not limited to those who experienced the 9/11 attacks directly (Silver et al, 2002), although the actual levels of stress outside of New York are disputed (Schlenger et al, 2002). In a small sample from Wisconsin, Krueger (2007) found that 9/11 increased sadness temporarily and decreased enthusiasm for at least seven days after the attacks. In a nationally representative sample of Americans, Lerner et al (2003) found a heightening level of fear and anger amongst the US population following 9/11. More recently, the terrorist attacks in London (UK) in 2005 have been shown to have negative effects on stress and have altered travel behaviour (Rubin et al, 2005)

Despite these and a range of other studies, we are unaware of any attempt to determine the effects of terrorism attacks on the SWB of those living outside of the attacked country, let alone quantify such effects.

8.3 Data and empirical strategy

This study examines the effects of 9/11 on the SWB of those living in the United Kingdom. This study has two main strengths. First, we use a large longitudinal dataset, consisting of approximately 10,000 individuals, which provides us with strong statistical power to discern patterns whilst controlling for individual heterogeneity and underlying trends. Second, 9/11 acts as an exogenous shock to the randomised sampled population, which provides us with a very powerful quasi-experiment.

The British Household Panel Survey (BHPS) is a nationally representative survey of British households, and is conducted between September and December of each year (started in 1991). Respondents are interviewed in successive waves and the sample has remained representative of the British population since the early 1990s. For the study to be thought of a quasi-experiment, the timing of terrorist attacks need to be exogenous and largely randomly assigned in terms of the BHPS interviews. The 9/11 attacks were clearly exogenous to the survey since many respondents are interviewed in

September each year and the date in September in which they are interviewed is randomised.

The measure of SWB used in this analysis is the twelve items from the negative affect scale of the General Health Questionnaire (Goldberg, 1978). Respondents are asked how often (on a four point category scale) over the past few weeks they: (i) had lost sleep over worry; (ii) felt constantly under strain; (iii) felt they could not overcome difficulties; (iv) had been feeling unhappy and depressed; (v) had been losing confidence; (vi) had been feeling like a worthless person; (vii) were playing a useful part in things; (viii) felt capable of making decisions; (ix) had been able to enjoy day-to-day activities; (x) had been able to concentrate; (xi) had been able to face up to problems; and (xii) had been feeling reasonably happy. The number of times a person places himself or herself in the top two categories was given a one, and then all twelve questions were added together to produce what is known as a caseness measure of SWB. This is a well-being score from zero to 12, coded so that the response with the lowest well-being value scores 12 and that with the highest well-being value scores zero. For simplicity, this count is reversed here, so that higher scores indicate higher levels of well-being.

This composite rating is a good proxy for the transient component of moods (Watson and Clark, 1984) and has been used as a measure of SWB in recent studies by economists (Blanchflower and Oswald, 2008; Clark, 2003; Clark and Etile, 2002; Clark and Oswald, 1994; Gardner and Oswald, 2007; Jones and Wildman, 2008). It has also been used to value intangible goods (e.g. Oswald and Powdthavee, 2008).

The well-being equation in a difference-in-differences (D-i-D) setting takes the following form:

$$W_{it} = \alpha + \beta_1 Post9/11_{it} + \beta_2 T_t + \beta_3 (Post9/11_{it} \times T_t) + \varepsilon_{it}, \qquad (1)$$

where W_{it} denotes SWB of individual *i* at time *t*, $Post9/11_{it}$ is a binary variable which takes the value of 1 if the individual was interviewed post-9/11 attacks (between 12th September 2001 and 30th September 2001), T_t is a year dummy, i.e. year 2001, and ε_{it} is the error term. The parameter β_3 represents the true causal effect of the September 11th attacks on SWB of those interviewed between 12th September 2001 and 30th September 2001. Assuming that in the absence of the September 11th attacks W_{it} would have changed identically in the pre-9/11 and post-9/11 groups between 2000 and 2001. More formally, in the absence of treatment, β_3 would be zero, i.e. there would be no difference in the mean well-being scores between pre- and post-9/11 (see Meyer, 1995). In this case, an unbiased estimate of β_3 can be obtained by D-i-D as:

$$\begin{aligned}\hat{\beta}_3 &= \Delta\overline{W}_{2000}^{\text{Post-9/11}} - \Delta\overline{W}_{2000}^{\text{Pre-9/11}} \\ &= \overline{W}_{2001}^{\text{Post-9/11}} - \overline{W}_{2000}^{\text{Post-9/11}} - (\overline{W}_{2001}^{\text{Pre-9/11}} - \overline{W}_{2000}^{\text{Pre-9/11}}).\end{aligned} \tag{2}$$

Note that this approach can accommodate multiple time periods and multiple treatment groups. We can then estimate β_3 by applying OLS to equation (1).

The panel nature of the BHPS allows us to follow the same individuals who were interviewed in September 2000 and September 2001 (excluding September 11th itself). A key assumption here is that, for those interviewed in September of each BHPS year, the date of the interview is orthogonal to the date of treatment, i.e. September 11th. This yields for the years 2000-2001 a balanced panel that consists of 9,535 observations (4,908 individuals). Of those, 1,020 individuals were interviewed between 1st and 10th of September in 2001.

8.4 Results

By applying OLS on equation (1) – without any control variables – we obtain

$$W_{it} = \underset{(0.098)^{**}}{9.842} + \underset{(0.110)}{0.211 \times Post9/11_{it}} + \underset{(0.092)^{**}}{0.334 \times T_t} - \underset{(0.105)^{**}}{0.316 \times (Post9/11_{it} \times T_t)}$$

$R^2 = 0.001; N = 9{,}535$

Standard errors are in parentheses; * < 5%; * * < 1%.

This implies that whilst there is a significant increase in the average SWB scores for the control group from 2000 to 2001, the same cannot be said for the treated group, i.e. those interviewed after the September 11th attacks. The D-i-D estimate is negative, statistically significant, and sizeable; the average treatment effect is -0.316 with a well-determined standard error of 0.105.

To check whether the above results are not driven by seasonality – i.e. the control group may have done their interviews when the weather was gloomier in winter of 2000, whilst the treated group may have done their interviews in the Autumn time of the same year – we can rerun OLS on those interviewed in September of both years only. By restricting to the 'September interviewees' of both years only, OLS yields:

$$W_{it} = \underset{(0.180)^{**}}{9.700} + \underset{(0.207)^{**}}{0.655 \times Post9/11_{it}} + \underset{(0.152)^{**}}{0.395 \times T_t} - \underset{(0.187)^{**}}{0.430 \times (Post9/11_{it} \times T_t)}$$

$R^2 = 0.073; N = 1{,}965$

Standard errors are in parentheses; * < 5%; * * < 1%.

The average treatment effect continues to be negative and statistically significant at the 1% level; those interviewed post-9/11 report a 0.430 lower SWB score than they should have experienced in absence of the September 11th attacks. There is a slight difference in the size of the average treatment effect between the first regression (no seasonal adjustment) and second regression (restricting to September interviewees in both years). This implies that we may need to control for the seasonal effects, i.e. the month of the

interview before or after the September 11 attacks, if multiple time periods were to be incorporated into the D-i-D estimation.

One of the key assumptions underlying validity of the above D-i-D estimate is that differences between treatment and control group would have remained constant in the absence of treatment (Meyer, 1995). We can check whether this is the case for the September 11 attacks by plotting the well-being trends for the control and the treated groups prior to 2001. Here, a 5-year period before and 1-year after the event is arbitrarily chosen to generate the plot, although similar patterns (but with significantly smaller N) can be obtained with longer leads and lags.

We can see from Figure 1 that the average levels of SWB for both pre and post-9/11 groups follow a very similar trend in the years that precede 2001. The trend however diverges in the year of the September 11 attacks. That is, there is a noticeable increase in the average level of SWB of those interviewed pre-9/11 from 2000 to 2001, which could be due to a number of a reasons such as a very good summer, general mood in the country after the 2001 general election (see he previous chapter for the effects of national elections on SWB). However, consistent with the estimated average treatment effects obtained in the previous OLS regressions, the average SWB levels for those interviewed post-9/11 hardly changes at all from 2000. In other words, there appears to be an 'offsetting' effect on the rising trend of SWB for the treated group, thus providing some validations for the average treatment effect obtained in our D-i-D model. Since both groups have already been exposed to the event by the time the survey was conducted in 2002, it is not surprising to see that the trend of SWB converges again one year after the 9/11 attacks. What is interesting is that the actual SWB levels do not return to the same levels as the previous year for the treated group.

Table 1 provides further robustness checks on the D-i-D estimates. Column 1 of Table 1 controls for a number of covariates that are consistent with the determinants of well-being (Dolan et al, 2008a), including household income, age, age squared, gender, education, employment status, health status, number

of children, and regional dummies. We also control for the pre- and post-9/11 seasonal effects by including dummies for the month interviewed in 2000. In the full specification, OLS continues to produce a negative and statistically significant average treatment effect; the coefficient on the interaction between the post-9/11 group and the year (=2001) dummy is -0.342 with a statistically well-determined standard error of 0.107.

To check whether the results might be driven by those individuals who were interviewed immediately after 9/11 in 2001, the second column of Table 1 splits the post-9/11 group into two groups: interviews that took place between September 12th-20th and 21st-30$^{th.}$. Whilst those interviewed between 21st and 30th September 2001 reported a slightly lower average well-being score than those interviewed immediately after the September 11th attacks, i.e. between 12th and 20th September 2001, both still reported a significant drop in SWB between 2000 and 2001 compared to the control group. It is interesting here that those who were interviewed later on in the month (i.e. 21st – 30th) appeared to be more affected by the terrorist attacks than those interviewed closer to the attacks (i.e. 12th – 20th).

It could be argued that the selection process into the treated group is not random, i.e. the selection process may be correlated with unobserved factors that are also correlated with measures of SWB. To check for this, we estimate in the first column of Table 2 a D-i-D model with multiple time periods using a fixed effects estimator. Using a seven-year balanced panel (1996-2002), the fixed effects estimator produces a treatment effect of -0.345 with a statistically significant standard error of 0.138. This average treatment effect is remarkably similar to the one obtained in the OLS regressions, which suggests that even if there was selection by unobserved time-invariant factors into the treated group, the effect is negligible. The absence of unobserved heterogeneity bias also means that we can estimate equation (1) using either OLS or random effects models.

Given that we have more than two time periods in our analysis, there is the potential for serial correlation which could understate the standard deviation of

the estimated treatment effects, leading to an overestimation of the *t*-statistic (Bertrand et al, 2004). However, the introduction of AR(1) errors into the random effects regression in the second column of Table 2 does not lead to a substantial increase in the standard errors. In fact a virtually identical average treatment effect to the one obtained in OLS can still be obtained in a random effects model.

8.5 Valuing the losses in SWB

The effect of the 9/11 attacks on SWB seems large and robust, and valuing the impact in monetary terms would facilitate cost-benefit analysis. We can estimate monetary values in two ways: (i) cost of illness; and (ii) income compensations.

8.5.1 Cost of illness

A GHQ score of around 2 is a conservative threshold level at which lower levels of SWB can be diagnosed as clinical depression (Goldberg et al, 1998). Using this threshold we can see how many people in the United Kingdom may have suffered the equivalent of clinical depression as a result of the 9/11 attacks. From the BHPS sample in 2001, there were 253 people with a GHQ value between 1.01 and 2. A 0.316 or 0.430 change in the GHQ (the range of values from our estimates) at this part of the distribution represents 80 to 109 people. That is, 0.47% to 0.64% of the BHPS sample could have been diagnosed with clinical depression as a result of 9/11. Aggregating this up to the 45.5 million adults in the UK in 2001, around 214,000 to 291,000 UK residents may have experienced depression as a result of 9/11.

To treat such clinical depression, GPs often provide a course of cognitive behavioural therapy (CBT). The cost of one course of CBT is around £800 (NICE, 2008). Therefore, aggregating this up, we could argue that the 9/11 attacks had the equivalent effect of costing £171million to £233million for the treatment of the depression. This range of values can be seen as a lower bound

estimate for three reasons. First, the depression threshold used here is a relatively conservative one. If we used a threshold of 3 on the GHQ, the costs would become between £211million to £273million. Second, CBT is not fully effective. The current effect rates are around 60% (Layard et al, 2007). So if we gave another course of CBT treatment to the 40% of people who did not recover first-time around, and still using the conservative threshold value of 2 on the GHQ, our estimates would rise to £214million - £290million. Third, these estimates are local effects since they are based on one particular threshold – they are not average effects from the whole of the BHPS.

8.5.2 Income compensations

The calculation of the IC for the terrorist attacks is based on the implicit utility-constant trade-off between the terrorist attacks and income. As demonstrated in chapter 3, the IC is defined as the increase in income necessary to hold utility constant if the individual has been exposed to the terrorist attacks. In an indirect utility function, this would be given by:

$$v(T_1, y_0 + IC) = v(T_0, y_0) \qquad (3)$$

where $v(.)$ is the indirect utility function, y_0 is the initial income, T_0 is the pre-9/11 attacks condition, and T_1 is the post-9/11 attacks condition. Given this, and the micro-econometric specification in (1), the IC (at mean income levels) can be defined as:

$$IC = e^{\left(\frac{\hat{\beta}_2(T_1 - T_0)}{\hat{\beta}_1} + \ln(y_0)\right)} - \bar{y}_0 \qquad (4)$$

where $\bar{y}_i$ is average household income of the sample population.

In our sample, we did not find a significant income effect due to our sample restrictions. As a result, we use the IC as a guide to what the costs could look like using this approach, but it cannot be considered a definitive result. There have been other studies, however, that have shown that an income effect in

GHQ regressions can be found, especially using instrumental variables (IV). We could, for example, use Oswald and Powdthavee's (2008) estimate of the natural logarithm of personal income on GHQ – i.e. 0.818 for fixed effects using IV and 1.159 for random effects using IV.

We can use these estimates in equation (4) to estimate the value of terrorism on SWB. The estimates of 0.316 (OLS no controls) to 0.430 (OLS seasonality controlled) represent our best causal effects of the terrorist attacks. Using these estimates, and an average UK personal income of £24,000, we find that the average treatment effect for each individual is worth between £7,500 and £17,000. This is a very large amount of income needed to compensate each individual for experiencing the 9/11 attacks. The cause of this large IC, apart from the large effect the 9/11 attacks have on SWB, is the income coefficient. This income coefficient is low because of the use of panel data, where the effects of income seem to be small. While the income-SWB debate will shed further light on our estimates, we do not currently have good estimates of the causal effect of income on SWB throughout the income distribution to know what coefficient to use and how to weight it. Therefore, these costs are a representation of what could be, and we arrive at the same conclusion as Deaton et al (2009) that without a more robust income coefficient, we cannot provide accurate income compensations.

8.6 Discussion

This chapter has shown that the 9/11 attacks in the United States lowered the SWB of United Kingdom residents – by a GHQ well-being score of approximately 0.3-0.4. Comparing this magnitude with other life events within our data is difficult since many events, such as marriage or being unemployed, are endogenous. Notwithstanding this, the magnitude of the 9/11 effect is potentially worse than becoming divorced, and about one-third of the effect of being unemployed or widowed in the same sample using the same methods. These are significant and robust effects.

The effects of 9/11 on GHQ provide us with cost estimates of the effect of the 9/11 attacks on UK SWB of between £170million-£380million. Using an income compensation significantly inflates these numbers but we do not have a robust income coefficient to use, which renders such values as imprecise. These cost estimates go some way towards demonstrating that the psychological cost induced by terrorism is substantial and might greatly exceed the discounted physical harm (Sunstein, 2003; Becker and Rubinstein, 2004). This is due to the fact that 'dread' makes up a significant part of the risks from terrorism (Viscusi, 2009), and this especially true given that media coverage that was dominated by the 9/11 attacks (Eisensee and Stromberg, 2007).

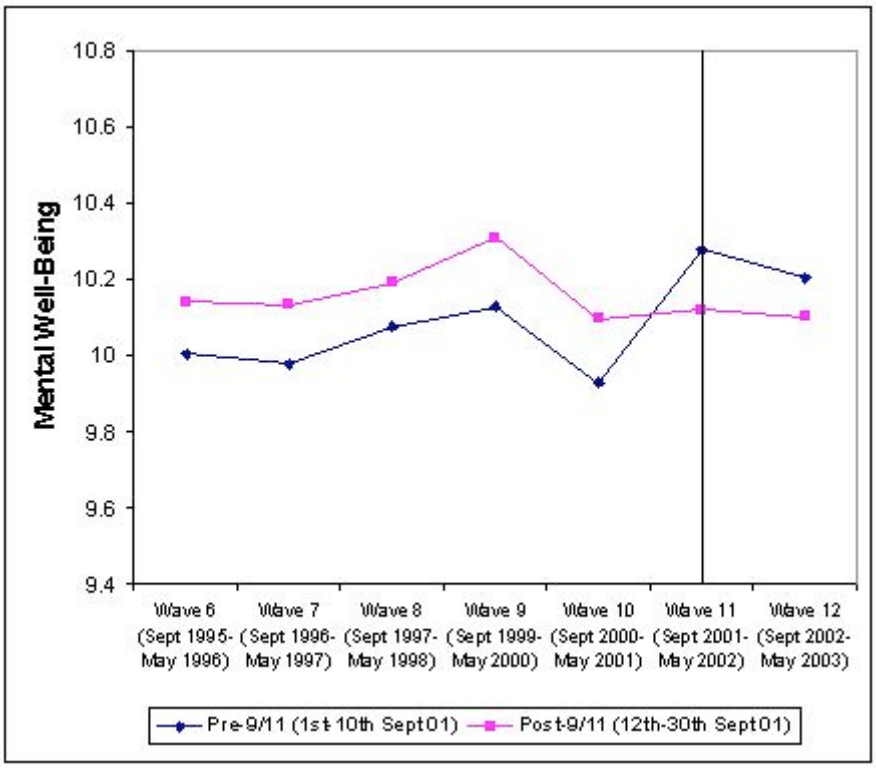
Whatever the precise scale the impact of 9/11 across the UK population, it is possible that individuals in the UK were affected by 9/11 because they believed that such events were more likely to happen in the UK in the near future, thereby increasing their fear and uncertainty. Given Krueger and Laitin's (2008) finding that terrorists are more likely to attack wealthy countries, it seems natural for individuals in other wealthy countries to be affected by terrorist attacks overseas. Indeed, the results from our study support the Caplin and Leahy (2001) model, but here the events that caused the initial fear and uncertainty took place in another country.

We can only speculate about such issues here as there has been little discussion of the international spillover effects of security or terrorism. The US Congress Joint Economic Committee (2002) has suggested that some of the largest costs of terrorism were the difficult to measure costs of added anxiety, stress, and psychological disorders associated with the increased threat of terrorism. This paper has shown that these costs may also have been very significant outside of the US.

Overall, this chapter has demonstrated that it is possible to value non-market goods using alternative measures of SWB. Combining the GHQ with the cost of illness approach enables us to place a monetary value on terrorism. This chapter also shows that it is possible to find the effects of unexpected events on SWB. Much of the SWB literature to date has focused primarily on expected

events, for example, marriage, divorce, pay increases etc., which all have a sense of inevitability about them. It might be that unexpected events are especially important to SWB and more innovative research is needed to establish the effects of unexpected life changes on SWB and it would also be interesting to compare the effects of unexpected and expected life changes. The key underlying message from this chapter is that we can value non-markets goods without necessarily using ICs, and it is possible to use SWB in non-market valuation in a wide range of contexts that might seem a priori very difficult or even impossible to value.

Figure 1: Trends in subjective well-being before and after 9/11/2001



Note: This is a balanced panel, with 691 individuals completed the survey between 1st of September 2001 and 10th of September 2001, and 2,320 individuals completed the survey between September 12th 2001 and September 30th 2001. The same individuals are then tracked over the 7-year period from 1996 to 2003. The vertical line represents the year of the September 11 attacks.

Table 1: Well-being and the September 11 attacks: OLS regressions

Dependent variable: Subjective well-being	**(1)**	**(2)**
Post-9/11 (12th Sept-30th Sept 2001)	0.239	
	[0.112]*	
Post-9/11 (12th Sept-20th Sept 2001)		0.239
		[0.121]*
Post-9/11 (21st Sept-30th Sept 2001)		0.219
		[0.127]
T = 2001	0.308	0.311
	[0.095]**	[0.095]**
T = 2001 x Post-9/11 (12th Sept-30th Sept 2001)	-0.342	
	[0.107]**	
T = 2001 x Post-9/11 (12th Sept-20th Sept 2001)		-0.268
		[0.115]*
T = 2001 x Post-9/11 (21st Sept-30th Sept 2001)		-0.425
		[0.123]**
Regional dummies (20)	Yes	Yes
Month interviewed dummies (9)	Yes	Yes
Background variables (20)	Yes	Yes
Observations	9521	9240
Overall R-squared	0.0635	0.0644

Note: Standard errors are in parentheses. **<1%; *<5% significance levels. Background variables include age^2/100, log of household income, employment status (9), education (6), and marital status (5). All unique individuals interviewed after September in 2001 are dropped from the analysis. All of our regressions are available upon request.

Table 2: Well-being and the September 11 attacks: FE and AR(1) errors RE regressions

Dependent variable: Subjective well-being	**Fixed effects**	**AR(1) errors RE**
Post-9/11 (12th Sept-30th Sept 2001)		0.213
		[0.126]
T = 1996	-0.134	0.136
	[0.159]	[0.124]
T = 1997	-0.104	0.099
	[0.144]	[0.124]
T = 1998	0.032	0.167
	[0.132]	[0.123]
T = 1999	0.158	0.229
	[0.125]	[0.116]*
T = 2001	0.383	0.315
	[0.125]**	[0.116]**
T = 2002	0.365	0.226
	[0.134]**	[0.124]
T = 1996 x Post-9/11 (12th Sept-30th Sept 2001)	0.003	-0.038
	[0.138]	[0.140]
T = 1997 x Post-9/11 (12th Sept-30th Sept 2001)	-0.015	-0.044
	[0.137]	[0.140]
T = 1998 x Post-9/11 (12th Sept-30th Sept 2001)	-0.041	-0.056
	[0.137]	[0.139]
T = 1999 x Post-9/11 (12th Sept-30th Sept 2001)	0.032	0.022
	[0.137]	[0.131]
T = 2001 x Post-9/11 (12th Sept-30th Sept 2001)	**-0.345**	**-0.331**
	[0.138]*	**[0.132]***
T = 2002 x Post-9/11 (12th Sept-30th Sept 2001)	-0.244	-0.222
	[0.138]	[0.140]
Regional dummies (20)	Yes	Yes
Month interviewed dummies (9)	Yes	Yes
Background variables (20)	Yes	Yes
Observations	22,168	
Number of individuals	3,209	
Overall R-squared	0.02	

Note: Standard errors are in parentheses. **<1%; *<5% significance levels. Background variables include age^2/100, log of household income, employment status (9), education (6), and marital status (5). All unique individuals interviewed after September in 2001 are dropped from the analysis.

Table 3: The valution of the 9/11 attacks on UK SWB

	Lower bound estimate	Upper bound estimate
Cost of illness		
(i) GHQ threshold of 2 – 100% CBT effectiveness	£171 million	£233 million
(ii) GHQ threshold of 2 – 60% CBT effectiveness	£239 million	£326 million
(iii) GHQ threshold of 3 – 100% CBT effectiveness	£211 million	£273 million
(iv) GHQ threshold of 3 – 60% CBT effectiveness	£295 million	£382 million

9. CONCLUSION

This thesis concludes by summarising the main results from the thesis and the implications it has for academic research and public policy (section 9.1). Section 9.2 then illustrates some of the limitations of the thesis and addresses what would be needed to circumvent the limitations. Section 9.3 addresses some of the important future research that could be stimulated from this thesis, and section 9.4 concludes the thesis.

9.1 Summary

The two main aims of this thesis were to determine how SWB compared to WTP for valuing non-market outcomes, and to establish whether SWB could be used for welfare appraisal and public policy. This thesis showed that valuing non-market goods by the SWB method produces different estimates of the non-market good than the WTP approach. The SWB approach is a promising way to value non-market goods and, as a result, there is a large potential for SWB to be used to value non-market goods for public policy, especially as a complement to preferences. This would supplement what the UK HM Treasury currently practices in non-market valuation in the UK. The HM Treasury currently places ex ante preferences on a pedestal for welfare appraisal. As this thesis shows, however, ex ante and ex post preferences might generate different monetary values, and what people want ex ante does not always provide individuals with the same change in SWB over a certain time period. The role of mis-wanting creates problems for the sole use of preferences in welfare assessments. While using SWB is promising, a few issues remain that currently limit its potential for welfare economics, such as scale recalibration and the double focussing effect. These problems also apply to the preference-based approach. If SWB can be measured accurately, we could advise decision makers to use such assessments in welfare appraisal.

This research therefore has clear implications for academic research in economics. While it has direct relevance to environmental, health and transport economics (and other non-market areas such as crime), there are other areas of

economics that this thesis might also impact upon. For instance, welfare economics uses people's preferences for deciding the effect of policies designed to improve welfare. Clearly, we cannot know if these preferences improve happiness unless SWB is measured and analysed. There is also the potential to use SWB to measure and value the non-market effects of new inventions and innovations (Dolan et al, 2008c). The role of innovation policy is clearly very important to productivity and economic growth (Haskel, 2007; Nesta, 2008), and where public policy is directed to improve human well-being through innovation, there is the possibility to determine the effects of such policy on SWB (beyond productivity).

Another relevant area is that of public choice, in that most models of voting, in local or national elections, explicitly argue that individuals do obtain some utility from voting. Using SWB explicitly allows us to test this in a range of public decisions. It might be that the role of public participation in shaping policies is very important to SWB. Not enough of the voting research focusses on the actual outcomes of individuals from voting and elections.

The double focussing effect found in Chapter 6 has important implications to the subjective responses found in panel data within many disciplines. Given that the majority of social sciences use panel data in some form or another, this research might have consequences across the social sciences. Whether it is subjective data regarding well-being, health, preferences, behaviour, or attitudes, we may have to be careful to interpret the findings from panel surveys. There is clearly a trade-off here since panel datasets greatly improve the efficiency of our empirical estimates as opposed to the typical cross-sectional estimates. There might however, be some people whose subjective assessments are shaped by the panel nature of the survey. This clearly needs to be researched further before we can assess its true implications for the social sciences.

This thesis has implications for the foundations of microeconomics and the preference satisfaction account that indirectly assumes that what we want the most provides us with the greatest well-being. It has previously been argued that

the goal of economics is to evaluate the social desirability of alternative allocation of resources so as *"to achieve the maximum well-being of the individuals in society*" (Just et al, 2004: 3). Social desirability may well depend upon how well-being is defined, and this thesis could be used as the platform to evaluate the relationship between preferences and SWB for non-market goods, which will enrich the debate about how best to allocate scarce resources.

On the potential for SWB to be used in welfare appraisal, if the causal role of absolute income on SWB can be established, then the SWB approach has the potential to generate meaningful monetary values of non-market goods for public policy. The research on generating monetary values for non-market goods from SWB, however, is still in its infancy and is literally thirty years behind that of generating monetary values from revealed and stated preferences. So, we need more research on using SWB for economic valuation and, in so doing, we will be in a better position in the future to judge just how meaningful and robust this method actually is.

Beyond this thesis, there are still issues regarding scale recalibration and framing that need to be overcome before the regular usage of SWB in welfare economics and policy-making. The SWB approach may also be unable to elicit non-use values. Notwithstanding the normative issues surrounding the appropriateness of non-use values in public policy appraisal, a simple cost-benefit rule might be viewed as inappropriate where non-market goods touch on issues such as beauty, knowledge, justice or morality. These aspects however might well affect SWB, but it has yet to be shown that they actually do. Many non-use values are for future states of the world, but expectations about future states of the world (Graham, forthcoming) in any sense might however impact on an individuals' SWB today, even if they do not experience that state of the world. So while individuals in the UK might never go to Alaska and experience an oil spill in that state, it could still cause distress to them. Chapter 8 demonstrated how events in another country could impact on the SWB of the UK population – i.e. cross-border impact on SWB. So non-use values (in the conventional preference sense) might actually generate changes in SWB for

some populations. Researchers have to be innovative to find and elicit such effects in these populations.

9.2 Limitations

While the originality of this research is that we have compared preferences to experiences for valuing non-market goods and demonstrated some measurement issues in using both WTP and SWB, we need to acknowledge the limitations of this research. The first limitation is that we have only used evaluative accounts of SWB, e.g. life satisfaction. We have not used any alternative measures such as affect or moment-to-moment well-being measures (which has been termed 'experienced utility', Kahneman et al, 1997). Kahneman and Riis (2005) argue that there are two types of selves within an individual: the evaluator and the experiencer. The evaluator is what has been used in this thesis; it elicits people's life satisfaction or general happiness. The experiencer refers to what we experience on a moment-to-moment basis (for example, five to ten episodes during the day), in terms of pleasure and pain. This Benthamite view of experienced utility is a simple calculation of the aggregation of the pleasure during the day minus the pain during the day (Kahneman et al, 2004).

This distinction is important since experienced utility could be seen as being preferred to the evaluation because experiences can possibly be measured more objectively (Kahneman, 1999). For instance, Kahneman et al (1993) exposed students to two different cold-pressor episodes before giving them a choice of which episode they would repeat on a third trial. In the 'short' episode, students held a hand in water at 14°C for 60 seconds, experiencing substantial pain. The 'long' episode lasted for 90 seconds. The first 60 seconds were identical to the short episode; over the final 30 seconds the temperature gradually raised to 15°C, still unpleasant but less so. From the point of the experiencing self, the long trial is clearly worse. For the evaluative self, however, the peak/end rule implies that the added period of diminishing pain makes the memory of the long trial less aversive. Respondents chose to repeat the longer episode, reflecting the misguided preferences of the remembering self. As a result, the same event may

have different implications depending on what measure of SWB is used. Clark and Georgellis (2004) demonstrate such behaviour in the labour market.

So the evaluator in us neglects duration and because of our memory, it may generally misguide us (Kahneman and Riis, 2005). The experiencer in us may be less likely to neglect important aspects such as duration or time. The issue of time is of importance because:

> "*other things being equal, well-being is increased by spending more time in the good states and less time in bad or empty states. This formulation holds whether the good state is defined by positive affect or by intense engagement in a task or in a spiritual pursuit. Time is the ultimate finite resource of life, and finding ways to spend it well is a worthy objective both at the individual level at the level of a social policy that is concerned with human well-being*" (Kahneman and Riis, 2005: 294)

This quote should be recognised by future research using SWB methods on non-market valuation, and researchers in this field must at least acknowledge the issue of measuring SWB by the evaluator and the experiencer. Further research should also recognise the normative issues as well as the empirical ones of using both types of SWB.

Blanchflower (2008), however, provides general evidence to support the view that the experiencer and evaluator provide similar results, since both suggest higher well-being for the young and old, the educated, for the higher-paid, for the employed and for the married. He suggests that both types of data are complements and not substitutes, but acknowledges that we require further research from the experiencer on a larger scale before we can think about how to incorporate their results into the SWB literature. So, it is entirely possible that the results from this thesis might not change if we used both concepts of SWB.

A second limitation concerns the generalisability, time frame and frequency dimensions of the primary data. The majority of this thesis used primary data that are not available elsewhere. The expense of gathering such data means that the number of observations are relatively small. This makes generalisability more difficult. To place this into context, in chapter 4 we show that wind farms

do not affect people's SWB. Yet this result might be particular to the local population, and if a wind farm was built in the heart of an area of outstanding natural beauty or a densely populated area, it is imperative to question whether we would have obtained the same result. This is something that needs further research but we hope that this thesis sheds light on methodology as opposed to the direct application of the values generated.

The time constraints of this thesis made it very difficult to conduct any primary data analysis of a long change in policy. We selected the Scout Moor wind farm project as the timing was ideal for the thesis. For many other policies, however, the process of change takes a great deal longer than the nine months that Scout Moor took. Quasi-experiments, such as that in chapters 3 and 4, are important to use, but ideally we would also want to examine preferences and SWB before the intervention is expected. This would allow us to examine the effects of non-market goods on SWB if they are expected or unexpected. For instance, we could examine whether different factors such as consultation, deliberation or participation, may change the overall impacts of becoming unemployed, divorced, changes in pay, urban regeneration and wind farms on SWB. The effects of terrorism on SWB shown in chapter 8 were very large (in comparison to other life events), which may have not only resulted from the increased uncertainty, but also because of the unexpected nature of this type of non-market good. This is something that we currently do not fully understand.

Related to this, we would like to know how quickly preferences and experiences change after a policy. For the wind farm research, we analysed people's ex ante and ex post preferences and experiences one year apart. It might be entirely possible that there were changes in preferences and/or experiences between this period that were omitted. In order to find out about these potential changes, it would be ideal to have greater monitoring of both preferences and experiences for non-market goods.

A third limitation relates to focussing effects and the administration of surveys. All of the data in this thesis is derived from surveys, whether it be preferences or experiences. To take chapter 3 as a case in point, the surveys were posted to

each individual house. We did not interview respondents in person or over the telephone, so there is a possibility that respondents may look through the survey first and realise that there were some questions relating to urban regeneration in the survey. They may have then focussed their attention on urban regeneration when answering their life satisfaction question (even though this was the first question). This may have caused them to allocate attention to urban regeneration more so than they would have done without first looking through the survey. So the question that remains is if we had interviewed residents in person or by telephone, would we have achieved the same results? This is something that merits further research and has potential implications to the SWB questions in the BHPS since they are also presented in a self-assessed survey.

9.3 Future research

Clearly more research on valuing non-market goods using SWB is required. Firstly, there is a large potential to value a whole host of non-market goods that have yet to be valued by SWB (such as airport expansions, floods, temperature changes (e.g. heatwaves), and even the 2012 Olympics), and then use such values to compare against the traditional WTP approach. There is also the potential to use SWB to assess how changes in ecosystem services have an impact on human well-being (Nicholson et al, 2009), which is the central objective of the Millennium Ecosystem Assessment project (MEA, 2005).

Secondly, in terms of the differing measures of SWB (such as moment to moment measures and life evaluation measures), there is a large potential to compare these measures in the context of non-market goods – see Deaton et al (2009) and Graham and Chattopadhyay (2009) for early examples of this using cross-sectional data. To determine how these measures might give different results for differing non-market goods is clearly very important. For instance, in a study such as the wind farm example presented in Chapter 4, we could have used the day reconstruction method in addition to life satisfaction in both areas over both time periods. There has been no longitudinal study to date using the experiencer (i.e. moment-to-moment utility) to examine the causal link of any

event or intervention. This would be an interesting area for future research in order to determine if there are differences in such measures and, if so, what measure is more in line with what people want.

Thirdly, adaptation is clearly key to further research in this field, although in itself, it raises thorny normative issues about when adaptation should or should not be allowed for public policy. For instance, Graham and Chattopadhyay (2009) report adaptation to high crime and corruption rates in Afghanistan, notably because high levels of both are now the norm. They argue that while adaptation might be a key survival strategy at the individual level, it may be bad for overall welfare as it results in a collective tolerance for a bad equilibrium. The role of adaptation has been key in the debates on unemployment and SWB, especially as it seems that unemployment is a domain of life that humans do not easily adapt to (Clark et al, 2001; 2003; Clark, 2003; 2006). Thinking about how adaptation can be both desirable and undesirable for public policy is a worthwhile avenue for future research (Menzel et al, 2003).

This work on adaptation goes hand-in-hand with the issue of scale re-calibration. Unpicking the effects of scale re-calibration is very difficult empirically, but by using different SWB measures over time for the same individuals, we might be able to determine whether adaptation occurs in all measures of SWB or whether in occurs in just one measure. For instance, we could use biomarkers (e.g. cortisol levels, blood pressure etc), the DRM, and a life satisfaction measure to see if the effects an event or intervention diminish quicker for some measures than others. There could also be the possibility for future research to attempt to fix the end-points of scales over time to ensure that individuals' end points do not change over time after experiencing a non-market good. For instance, the zero rating life satisfaction could be augmented to be labelled as being equivalent to being dead. This would link in well with the Quality-Adjusted Life Year (QALY) used in health allocation decisions, where full health is assigned a value of one and death is assigned a value of zero (Dolan and Kahneman, 2008). The potential for SWB to replace the Q in the QALY is large, but the SWB literature should learn lessons from the QALY literature in trying to fix the end points so as to mitigate scale recalibration

issues.

Fourthly, future research should also pick up on the administration issue of eliciting SWB. While it may at some point be possible to measure well-being physiologically or neurologically, those days are not here yet, so the only possible methods involve subjective self-reports (Schooler et al, 2003). Self-reports of global happiness have shown to be vulnerable to some biases and measurement problems (Schwarz and Strack, 1999) – as discussed in Chapter 2. As we showed in chapter 6, additional research is needed to determine more precisely the specific situations in which explicit evaluation of life influences or re-calibrates that evaluation. If life events can substantially change the underlying set-point in life satisfaction (Clark et al, 2004), then there may be reasons to expect that such events affect the end points of the scale. Only after such research is conducted will we be able to assess the full ramifications of the present analysis for economic and social science approaches that rely on the subjective reporting of experiences. Related to this is the need to find out the effects of survey administration on SWB reports and to determine how much of a focussing effect there is in such self-assessed surveys as opposed to in person or telephone interviews.

Fifthly, while research on the effect of the non-market good on SWB will be very fruitful, examining the other direction of the relationship would also be important (for instance, see Graham et al, 2004; Lyubomirsky et al, 2005). Topics such as residential sorting and non-market behaviour would be ideal for such an analysis. For residential sorting, it would be very interesting to determine what types of SWB predict individuals moving to a particular location or geographic area and the possible selection effects by demographic characteristics. For instance, Clark (2009) has argued that income-comparison sensitive individuals will be especially attracted to live in localities that have lower average incomes. This may equally apply to other sorting issues such as in the workplace, and to the debates on reverse causality in general.

In relation to this, work and housing (and/or locality) are the two areas traditionally used to examine revealed preferences. For SWB, there has been a

great deal of work on the former but not on the latter. For instance, there has been a great amount of research that has demonstrated that job satisfaction: follows the same age trend as life satisfaction (Clark et al, 1996; Clark, 1997); is affected by income comparisons (Clark and Oswald, 1996); and predicts quitting (Clark et al, 1998; 2009a; Clark, 2001). This latter result is interesting since it demonstrates how a domain of life satisfaction can impact on behaviour. There is little work examining the impact of local area or housing satisfaction on behaviour, so there remains a need to determine how local area satisfaction, or housing/financial satisfaction, predicts moving home to a new area. This relates to the aspects of reverse causality and non-market behaviours mentioned above. Determining whether high SWB enables individuals to become 'better citizens', in terms of trust, social contact, volunteering etc (Guven, 2007), without selection bias will be a difficult but fruitful area of research.

The issue of causality is especially important in terms of the effect that income has on SWB and vice versa. Eliciting a robust causal coefficient of income on SWB is important for generating monetary values from SWB, so this area of research is especially important for non-market valuation. If robust and consistent income coefficients cannot be found from UK data, then more discussion should be directed as to whether it is best for public policy to use SWB within a cost-utility analysis framework as opposed to a CBA framework.

Lastly, it will be interesting to see how key stakeholders view the results from this thesis or from the studies that use SWB more generally. In particular, it will be important to determine what policy-makers and the public think about using SWB data to inform particular policy issues. The use of SWB might stimulate issues of paternalism, since we might introduce a policy that people initially do not want, through fear (Slovic, 1993) or repugnance (Roth, 2007), but policy-makers implement it as they know people will end up liking or enjoying it. We do not yet know how individuals and society would feel about policy-making being driven by what people enjoy rather than what they want. Whilst there might always be problems of the government not being able to determine what is in the best interests of society (Glaeser, 2006), allowing SWB to become routinely measured across individuals might help facilitate greater scrutiny by

academics of whether the government is maximising welfare. The creation of the UK Household Living Survey (from the BHPS) will help facilitate this, as well as the possibility of having some SWB questions in the 2011 and/or 2021 UK censuses.

In addition, it will be interesting to determine how citizens view the potential problems with SWB measures (e.g. adaptation) as compared to problems with other well-being measures, as well as where they think SWB measures are deemed suitable, and which measures are considered to be best suited to particular policy issues. As this thesis has shown, there could be differences in valuation from one measure to another and there could be large adaptation in some domains, so it would be interesting to determine the course of action different measures of well-being and SWB suggest different options for policy. All of this research will inevitably push the boundary of SWB research further and increase the academic and public debate as well as encourage the political take-up of such measures.

9.4 Conclusion

One of the central roles of public policy should be to protect and promote individual welfare when it is difficult for individuals to do so themselves. Going back to John Stuart Mill, where there are negative non-market effects of consumption, we should seek to intervene, and therefore we require a value of such non-market effects. Public policy should not always use individual preferences due to problems, such as the projection bias, reference dependent preferences and loss aversion, framing etc. The SWB approach might offer an alternative for non-market valuation and public policy, as it determines what is important for individuals as experienced by themselves – it is an autonomous measure in this sense. Given that SWB can be used as an outcome for economic valuation, it would be very useful as a metric for 'joined-up' welfare analysis, used in the very least as a complement to people's preferences.

There is an increasing literature that documents that individuals do not always accurately predict the consequences of a future event. This is especially true

with negative events, where the psychological immune system comes in to play, which has implications for large global policy issues, such as the environmental degradation arising from global climate change. This raises a normative question: Is welfare best measured after individuals experience that future event? If we can cope well (i.e. adapt) with climate change, does this necessarily mean that climate change is as severe a problem as prior attitudes and preferences would have suggested?

The implications generated from the Stern (2007) report suggest that policies designed to lower greenhouse gas (GHG) emissions are justified by the fact that the costs of reducing GHG emissions now are less than the future benefits. If humans do not adapt to more extreme weather events and flooding, the benefits for mitigating climate change should outweigh the costs at a much quicker rate than suggested by Stern (ignoring issues on the discount rate). This is because the Stern report did not include non-market values of climate change. There is therefore a large potential for SWB to inform future debates on climate change, so as to include SWB non-market values into the long-run integrated assessment climate models currently being used. Knowing what individuals do and do not adapt to is crucially important for the debates on climate change, as well as in other domains such as health (e.g. obesity), crime/terrorism and migration (the latter resulting in part from climate change – Vörösmarty et al, 2000; Patz et al, 2005). This adaptation is also important for using preferences, since adaptation can equally occur in preferences, as is shown in the literature on defaults (Choi et al, 2008) and choice architecture (Thaler and Sunstein, 2008), as well as in this thesis.

At present SWB measures should be seen as work in progress, and resources dedicated to evaluating their performance and consistency are necessary. Even in their current form they still have a great deal to offer policy makers. Many determinants of an individual's SWB can be influenced by both the external environment facing the individual and their personal choices. The provision of information on the determinants of SWB may help individuals make choices that are in their future best interests.

To close this thesis, despite some on-going measurement issues, it is possible to value non-market goods and hence allocate scarce resources based on what people experience and enjoy in addition to what they want. To allocate scarce resources based on the effect that the non-market good has actually had on people's lives, as opposed to predicting the effect of the non-market good on people's lives, does not seem an unreasonable future avenue for economics and public policy.

10. References

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