

ISSUES IN MEASURING TRAVEL BEHAVIOUR CHANGE¹

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ABSTRACT:

Increasing interest in ‘soft’ policy approaches to demand management, initially in personal travel and now in water and power usage, poses the question of how to measure the effectiveness of interventions. Much of the focus has been on the statistical reliability of measured change, where sample surveys are the primary means of estimating the change. Sample surveys also pose methodological issues that can introduce non-sampling errors, especially when the ‘measure’ is the difference between two estimates (‘before’ and ‘after’).

In some cases, direct measurement of usage appears less difficult – both electricity and water consumption are metered in Australia. The challenge in these cases is to establish the extent to which changes in consumption are real and to what extent they are the result of factors such as seasonal differences (either between seasons or from year to year) or demographic change in the household.

This paper outlines the principles and pitfalls in measuring behaviour change. It draws on experience with voluntary travel behaviour change, using a number of approaches, including but not limited to Individualised Marketing. A key issue is the extent to which repeated experience can validate the effectiveness of voluntary behaviour change interventions in general, despite statistical errors of individual measurements.

Experience demonstrates that the scale of the intervention is important. Interventions with more than 5000 households are consistently more successful than small ones, even allowing for the greater statistical variability of measurement for smaller projects. Large-scale also offers opportunities for intervention design to benefit from the potential for diffusion beyond those directly involved in the project.

¹ This paper is a modified version of a paper jointly-authored by Werner Brög and Ian Ker as a resource paper for the 8th International Conference on Survey Methods in Transport, Annecy, France, 25-31 May, 2008.

INTRODUCTION

The focus of urban transport policy has shifted from roads to public transport and non-motorised transport, from infrastructure to services, and from demand satisfaction to demand management. This is a reflection of changing values about what represents viable and sustainable cities and communities as much as it is of the financial, physical and social realities of attempting to provide major road infrastructure to satisfy continually growing demand for car travel.

Demand management has been given further impetus by the twin pressures of global climate change (private motorised transport is a large and growing source of greenhouse gas emissions) and, more recently, the so-called 'Peak Oil' phenomenon. Peak Oil contends that conventional oil production has either peaked or will do so within the next decade, which would be a major challenge in its own right but is exacerbated by the continuing rapid growth in oil demand from developing economies, particularly China. This paper is not the place to debate the extent or consequences of Peak Oil (see, for example, <http://www.peakoil.net>), but there is widespread agreement that energy for transport (whether from conventional oil, unconventional oil (oil shale, tar sands) or alternative sources) will be more costly.

At the same time, we are more aware of and concerned by the adverse health effects of lower levels of physical activity, partly resulting from increasing car use.

There is a large amount of research over more than half a century to support the analysis and estimation of the impacts of 'demand satisfaction', although much of this is problematic when there are substantial impacts on land use. Demand management does not have the same history of application, monitoring and analysis. Voluntary travel behaviour change is relatively new in the context of demand management.

The primary issue with any new transport initiative is identifying and estimating the impacts on the amount and type (including mode) of travel. This paper outlines the state of current practice, the issues arising and some ways of resolving them for voluntary travel behaviour change through household-based programs, primarily in transport but also in the emerging areas of water and electricity.

VOLUNTARY BEHAVIOUR CHANGE

What Is Voluntary Travel Behaviour Change?

Voluntary travel behaviour change is one part of the travel demand management toolkit, not a single 'silver bullet'. It is the face of demand management that operates largely without 'carrots' (direct incentives, including system improvements) or 'sticks' (disincentives, such as additional charges or regulation). In most cases, voluntary travel behaviour change (VTBC) raises awareness, improves availability of information and support for people to try alternatives to driving their cars, working through empowerment and motivation.

VTBC, itself, has a range of tools that operate in the household, school and workplace contexts, each of which is almost a separate area of development. Household-based programs are of two principal types:

- Those that deal with the whole identified population (usually of an area), but with a structured program that focuses on those with identified sub-populations that have the greatest potential for change. The major program of this type is *Individualised Marketing (IndiMark[®])*. See, for example, http://www.travelsmart.gov.au/training/packaging_comm_indi.html.
- Those that deal with a sub-population, often self-selected. These are largely based on *Travel Blending[®]* and its derivative *Living Neighbourhoods[®]* (see, for example, http://www.travelsmart.gov.au/training/packaging_comm_blend.html). *Travel Blending[®]/Living Neighbourhoods[®]* may also include aspects of community development, but we are concerned only with the travel behaviour outcomes in this paper.

A third group (such as the ‘Households on the Move’ project in the Australian Capital Territory) is aimed at people or households making changes such as moving house that will inevitably have an impact on travel behaviour. Such an approach is potentially very effective for individuals, but because the participating people or households would change their travel behaviour anyway (because they are moving house or changing jobs, for example) there is no way of estimating the extent of travel behaviour change or even demonstrating that it has occurred.

Typically, there are no changes to transport infrastructure or services, other than the provision of route and timetable information at bus stops (in the case of IndiMark, new stands with route maps and stop-specific timetable information). Whilst there is some evidence that service improvements implemented in conjunction with *IndiMark[®]* can have a compounding (positive) effect (UITP, 1998), service changes during an initiative can have a detrimental impact by invalidating information and confusing participants.²

What Is Voluntary Behaviour Change Trying to Achieve?

From a public policy perspective, it is important to be clear about the objectives of an intervention, before we can establish how best to measure the extent to which they have been achieved. This sounds self-evident, but is often not clearly articulated in the professional debates on such topics.

In the case of water and power consumption, the objective is usually expressed in terms of actual consumption of water or electricity. In the case of personal travel, there is less convergence on a single outcome and this has sometimes led to confusion about the nature and validity of measurements.

There are many potential outcomes of travel behaviour change that are of interest, across the full range of economic, environmental and social outcomes, including:

- *Economic*
 - The economic cost of transport
 - Traffic congestion

² The change in bus boardings in the City of Subiaco, Perth, Western Australia, as measured through the Transperth ticketing system, was lower in areas where bus services were re-organised during the IndiMark[®] period (personal communication from Department for Planning and Infrastructure, Perth, Western Australia).

- *Environmental*
 - Greenhouse gas emissions
 - Local air pollution
 - Noise
 - Water pollution
- *Social*
 - Accessibility
 - Social inclusion

In transport terms, these objectives are often reduced to the shorthand of mode share, usually as aspirational targets rather than achievable outcomes. There are good reasons for the aspirational nature of targets: since most of the policy measures that could contribute to this objective are new or have not previously been subjected to rigorous analysis, we do not know how effective they are likely to be, individually or in combination.

At the strategic level, the extent of car driver travel is a direct measure of the amount of private motor vehicle travel, which is the principal determinant of most of the higher-level outcomes. There is much less certainty about the achievability of targets of the non-car modes than for the car driver mode and measurement of change is also statistically more difficult as they represent a smaller initial quantum.

INDICATORS OF CHANGE

It is difficult to measure the impacts of behavioural changes as a result of marketing actions. There are several methods available and each has advantages and disadvantages. If they all point in the same direction and are consistent with the same size of change, we can have greater confidence in the outcomes.

There are three main indicators that should be considered:

- *Marketing Indicators*, based on the amount and type of information requests, and the feedback from participants. For example, one of the desired changes is to increase the use of public transport and stop-related timetables are sought after by many people. It is unlikely that households will order specific, address-based timetables that they will never use.

In traditional direct marketing, these types of indicators are the only success factors used. That might be a little too courageous, but they are reliable, precise and easy to measure indicators that should not be ignored.

- *External Indicators* include measuring public transport patronage. In the WA TravelSmart program, bus boarding data are collected from the Transperth ticketing system and analysed independently. The ability to do so is enhanced by the recent introduction of the SmartRider ticketing system. ITP (2007) supports the value of 'robust corroborative data'. In Darebin, Victoria, attempts were made to measure changes in road traffic volumes (Richardson, 2005).
- *Behavioural Indicators*. The effectiveness of travel behaviour change projects can also be evaluated by measuring changes in the mobility characteristics of residents, by conducting extensive 'before' and 'after' travel surveys. The

analysis is detailed, based on mode share, activities and travel time and shows the mode shift from car-as-driver trips to environmentally friendly modes.

Most attention has been focussed on behavioural indicators, often to the exclusion of the marketing and external indicators. For both external and behavioural indicators, it is important to measure changes against a control group (ITP, 2007, para 9.48).

Whatever methods are used, acceptance of the results will be highly dependent upon comprehensive and consistent documentation of processes and outcomes.

MEASURING BEHAVIOUR

Direct Measurement?

The behaviours in which we try to measure change are often not directly measurable. Water, electricity and gas, in Australia, are directly metered at the consumer household level. However, we still do not know what the water or electricity is used for and the effect of exogenous factors can be difficult to identify. Some of the complexity of doing so, even at a highly aggregated level, is outlined in <http://www.daa.com.au/case-studies/water-usage>.

A common problem with direct metering is ensuring accurate and timely readings on a regular basis. This can be rectified by using self-reporting meters, already available on a walk-by basis for water, electricity and gas (<http://www.neptunetg.com>) with no need for access to premises or manual recording of meter reading) and could be adapted to automatically send readings by phone at specified times. Alternatively, data-loggers could be attached to meters for periodic automated walk-by download.

For the basic parameter of concern in VTBC, car use as driver, the extent of experience now available to support the robust effectiveness of the main travel behaviour change tools, has been interpreted (AGO, 2006) to support the use of simpler single measures such as odometer readings. However, this is not without its own problems, particularly in the case of households with multiple motor vehicles, where vehicles are used both for personal and business travel (a common issue in Australia with its Fringe Benefits Tax providing concessional taxation of cars as part of remuneration packages) and where vehicles have been bought or sold during the monitoring period.

For travel generally, and car use in particular, we are forced to less direct means of estimation, unless the outcome is so large that it can feasibly be measured through road traffic volumes. Even then, we face the problem of differentiating the traffic generated by the target population from that generated by the rest of the city.

It has been argued that geographical positioning systems (GPS) technology can provide a means of directly measuring travel and, by inferential means, mode use (see, eg, Stopher 2007a). However, this is, as yet, unproven in at least two key areas:

- The robustness of the algorithms for inferring mode use; and
- The potential for the measurement actually to influence travel behaviour – a person ‘equipped’ with a GPS device might be more aware of their travel behaviour and use transport more efficiently while carrying the device.

Unfortunately, there do not appear to have been any comparisons between GPS and surveys in respect of either error or data capture (AGO, 2006, p57).

GPS might have potential value in validating a travel diary or survey (for example in terms of places visited and times of travel), subject to the caveat of measurement influencing behaviour, but the reported difficulties of recruiting people to take part in trials (Stopher et al, 2005) and retaining them in subsequent waves of a GPS panel (Table 1) suggests that it might be difficult to achieve a sampling basis that is sufficiently representative of the community generally for useful results to be derived.

Table 1 Recruitment and retention in GPS trial (South Australian Panel)

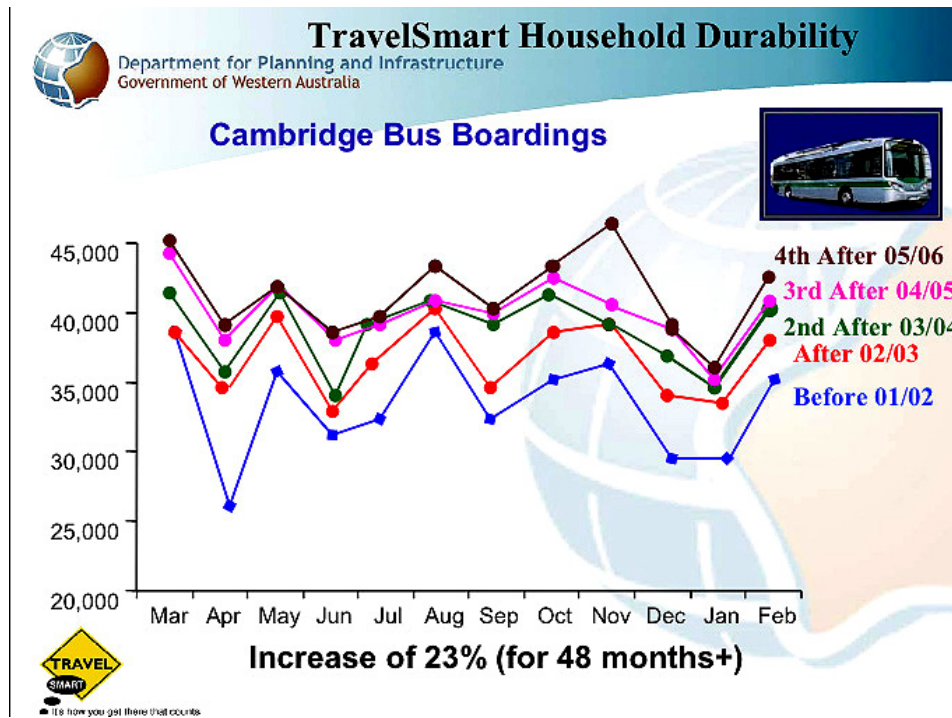
| Status | South Australian Panel (50 Households nominally) | | |
|-------------------------|--|----------------|---------------------|
| | Wave 1 | Wave 2 | Wave 3 |
| Recruited (new to wave) | 57 | 17 | 0 |
| Completed | 50 (88%) | 14 (82%) | 0 |
| Continuing (Recruited) | -- | 35 (61% of 57) | 44 (59% of (57+17)) |
| Completed (Continuing) | -- | 32 (56% of 57) | 36 (49% of (57+17)) |

Source: Stopher et al (2007a, Table 3)

Alternatively, GPS might have a more robust role if attached to the vehicle rather than the person, subject to the caveats already noted about multi-vehicle households, vehicle changes and identifying business use of vehicles.

For public transport, ticketing systems may allow direct measurement of trips originating in an area. Where this was possible, ticketing data has been consistent with the change estimated from travel surveys (see, eg, Figure 1). Smartcard ticketing systems facilitate use of ticketing information to estimate changes in public transport patronage related to VTBC.

Figure 1 Public transport boardings and IndiMark[®] - Cambridge, WA



Source: <http://www.dpi.wa.gov.au/mediaFiles/ts-Cambridgebusdata.pdf>

Surveys

VTBC initiatives have most often been assessed by surveys of various types. We generally measure change as the difference between 'before' and 'after' surveys, with 'corrections' indicated by a control group. It follows that it is critical that the 'before', 'after' and 'control group' surveys are undertaken using the same survey design and methodology. Differences in design or methodology will affect response rates (see 'Response Rates', below) or have more subtle impacts on the accuracy of responses that make comparisons problematic.

Cross-Sections or Panels for Surveys

Most travel behaviour change interventions have adopted a cross-sectional survey approach to estimating 'before' and 'after' travel behaviour. For small projects, in particular, this can create issues of statistical reliability of the difference between the two surveys, but these are less severe for larger applications.

It has been suggested that a panel survey approach, using the same people for both surveys, would be more suitable. However, panel surveys have their own problems that are more systematic and less amenable to treatment by statistical analysis.

- Even an ageing population ages more slowly than the individuals that comprise it. At a more aggregated level, it ages more slowly than the households or other groups that comprise it. Even a panel that remains intact over a period of time, will not be the same at the end of the period as it was initially.
- Whilst this might not make very much difference for the typical 'before-after' time period of 12 months, it has an increasing impact over time and adversely affects the comparability over longer time periods and, hence, the estimation of the durability of behaviour change.

- In practice, there is attrition of panels. If no action is taken to replace those who leave, the integrity of the panel is compromised by the possibility of self-selection bias among those opting out. Replacement poses the difficult problem of identifying and recruiting replacements who have the same characteristics as those who leave.
- People also suffer from 'survey fatigue' and may drop out or become less reliable for that reason. Whilst this can be addressed through 'refreshment' (replacing a proportion of the panel on a periodic basis), this again raises the issue of ensuring that replacements have the same characteristics as those who are discarded.

Whilst panels have been argued to require lower sample sizes, some of the parameters enabling smaller sample sizes also give rise to survey designs that are more difficult to undertake. For example, panel survey data is more difficult to obtain (with full control of other biases) than repeated cross-sectional data (Richardson et al, 2003). Acquiring repeated data from the same respondents is challenging and a reduced response rate in the 'after' survey can lead to sampling bias (ITP, 2007, para 9.24).

One example of a panel survey to estimate the effect of a VTBC initiative had a low initial response rate (49% - 1346 out of a sample of 2772 households) for the 'before' survey and substantial further loss between the 'before' and 'after' surveys. In addition, in the 'after' survey only 682 of the 881 households that responded had the same composition as in the 'before' survey (Richardson, 2005).

In two cases where both panel and cross-section surveys were carried out (North Brisbane, Queensland, and Victoria Park, Western Australia), the panel survey recorded slightly smaller, but still very substantial, reductions in car driver trips (11%/13% and 12%/14%, respectively). This supports anecdotal evidence that people who are flexible also move more, change more easily and are underrepresented in panels, which tend to focus on stability, less change and the status-quo. Behaviour changes measured in panels therefore seem to be smaller than in reality.

Integral measurement of travel behaviour, as part of the intervention, is a form of panel survey that highlights some of these problems, most notably those of attrition and measurement actually influencing the outcome (travel behaviour).

Sample Size

With a sample survey, the reliability of a sample estimate (in this case, the mean proportion of trips by car as driver) is inversely related to the sample size, both in absolute terms and as a proportion of the population being sampled.

A great deal of attention has been paid to the theoretical requirements for sample sizes to identify relatively small changes at a reasonable level of confidence. Stopher, (2005) suggests that to identify a change of 2% at the 95% confidence level it might be necessary to gather data from around 8,000 people in a before and after study. Such a requirement would be beyond the resourcing likely to be available for monitoring any such project and would, in any case, substantially diminish the resources available for actually delivering the VTBC initiative.

The focus on establishing statistical confidence requirements for small levels of behaviour change appears to stem from an erroneous assumption that the behaviour change with *IndiMark*[®] was measured only over the actively-participating population

(Stopher, 2003). The Australian Greenhouse Office has noted that: *In some of the reports evaluated, there appears to have been misunderstanding of statistical methods used to interpret results, and hence problems in the sample sizes, sampling methods and evaluation periods employed* (AGO, 2006, p58).

Much of the contention about sample sizes, however, is predicated on a desire for accurate estimation of the level of travel behaviour change, whereas the public policy imperative is for assurance that the level is greater than the level that would justify the initiative in socio-economic terms. Given the high benefit levels from travel behaviour change, this latter level is a very low bar – the prospective benefit-cost ratio for completion of the Perth TravelSmart Household program estimated a benefit-cost ratio of 67:1 on the basis of a 10.7% reduction in car driver trips (the weighted average of projects to-date in Perth, including two project areas (Marangaroo and Cambridge) chosen for their ‘low potential’) (Ker, 2004). Typically, urban transport projects are regarded as ‘justified’ if they achieve a BCR of 3:1 or 4:1. On this basis, VTBC would be justified in benefit-cost terms with a car trip reduction as low as 1%.

There has also been a strong tendency to focus on the probability of the change being less than estimated through sample surveys, with little or no recognition of the corresponding probability of its being greater.

If the population mean is greater than 2% (as all the interventions except Darebin indicate), by a factor of up to seven times and an average of around 4-5 times, the sample size requirements become much less onerous.

Most of the debate on sample size has been on the basis of sampling error estimation. In practice, the error due to systematic factors can be many times larger than statistical error. This is a serious problem, even if we do only focus on significance, because there are always two types of errors to be considered in empirical studies:

- Random errors (sampling-related), which can be calculated but not corrected; and
- Systematic errors (design related), which can be corrected but not calculated.

In travel behaviour, the systematic errors can outweigh the random errors in size significantly.

Response Bias

There is a potential danger that the very fact that a person is part of a program will influence that individual’s actual or reported behaviour. In the case of VTBC, the influence on actual behaviour is precisely what the program aims to achieve, provided that this is an ongoing response and not a temporary one simply to be able to present in a better light for the program itself.

There is also a danger of ‘program-compliant’ reported behaviour, although this is more likely to be found in panel surveys than in repeated cross-section surveys. Participants in a panel survey will know what their base travel behaviour was (the ‘before’ survey) and could, theoretically, report appropriately changed behaviour in the ‘after’ survey, even if actual behaviour had not changed. Participants in a cross-sectional ‘after’ survey will not have a ‘base’ to compare themselves against and will be less familiar with the detailed program objectives.

Misreporting travel behaviour in surveys can happen for a variety of reasons, irrespective of any potential desire of the ‘observed’ to please the ‘observer’. However, since even apparently simple activity and travel patterns can be quite complex, involving issues of location, duration, speed and mode, falsifying a survey response realistically is more difficult than might be expected. Detailed inspection of survey responses will show up unusual, inconsistent or infeasible travel behaviour that can then be followed up for clarification and, where necessary, correction.³

Bonsall (2007) draws attention to the possibility the repeated cross-section surveys will *by happenstance, result in some households being contacted more than once*, which he says could have negative (eg households feeling hassled by repeated surveys) or positive (the ability to explore changes in individual behaviour) consequences. In practice, unless sample sizes are larger than necessary or surveys are continually repeated to assess durability, multiple-contact households will be too few for any such issues or opportunities to be substantial.

Response Rates

The response rate to a survey is an important measure of its reliability. It also affects the value of key parameter estimates from the survey. To obtain accurate data, it is important to interview a representative sample. When a survey’s response rates are low, it’s unlikely that the sample interviewed is completely representative. The data collected from a survey with low response rates can suffer from non-response bias and the decisions made using those data may be flawed. Response rates in all forms of travel surveys have been declining and telephone survey response rates are rarely above 60% and are often 20-30% (<http://rns.trb.org/dproject.asp?n=14124>).

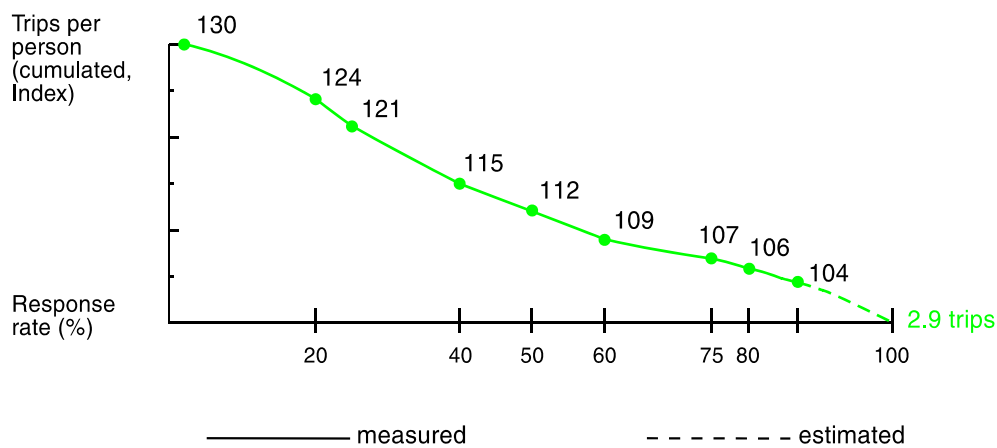
With telephone surveys, non-respondents can include high-mobility people (who are often not at home) as well as those who don’t consider themselves to have a sufficiently strong ‘stake’ in travel issues (eg because they travel very little). However, telephone surveys are increasingly affected by unlisted numbers and people with only mobile phones and no land-line.

With mailback surveys, the average number of trips per person per day varies inversely with the response rate (Figure 2). In other words, people who are less active in terms of travel are less likely to respond to surveys. This has a double importance when we are seeking to estimate the *difference* between two estimates:

- For any percentage change, the number of trips will depend upon the absolute values which in turn is inversely related to the response rate. It follows that a low response rate is likely to over-estimate not only the before and after trip making but also the difference between them.
- A difference in response rate between the ‘before’ and ‘after’ surveys will affect the estimate of the difference between them. If the ‘before’ response rate is higher than the ‘after’ rate, the difference is likely to be under-estimated, and vice versa.

³ The author has direct experience of this in an *IndiMark*[®] project, having been part of the ‘after’ survey and contacted for clarification of his household’s ‘unusual’ travel pattern – only one car and one licensed driver in a household of four adults; circular trips involving delivery on-foot of local newspapers; low level of travel activity; very low car driver trips.

Figure 2 Trip rate by response rate: Vienna 1993 (response rate 85%)



Source: Brög and Erl (1999)

More recent work (MVRWAVV, 2007) has indicated that improved survey design can reduce this effect, but the trip rate at a 40 % response rate would still be 10 % higher than the true value, which requires 80+%.

The best estimate of difference between two cross-sectional surveys of given sample size will come from surveys that achieve a similar high response rate. IndiMark applications typically have survey response rates of 70-80% for ‘before’ surveys and 70-85% for ‘after’ surveys. These surveys also show a consistency of frequency of out-of-home activities between the ‘before’ and ‘after’ surveys, indicating that the difference between them is unlikely to be a major influence on trip rates or mode usage.

Low and inconsistent (between ‘before’ and ‘after’ surveys) response rates might be a partial explanation for differences in measured effectiveness between types of intervention.

It has been suggested that the use of GPS for measuring travel behaviour would permit a smaller sample size than for a conventional travel survey (Stopher, et al, 2007b). For example, the sample size proposed for long-term monitoring of travel behaviour change in North Brisbane, using GPS, is 300 households. It is not clear why required sample sizes would be any lower than would be the case for multi-day diaries, as the variability due to random sampling would be the same in both cases. The potential for non-sampling and response errors exists, in different ways, for both methods, but is not related to sample size. Indeed, the difficulties of recruiting and retaining households in GPS panel surveys (Stopher et al, 2005 and 2007a) suggest that non-sampling errors are likely to be a severe problem.

Daily or Weekly ‘Diaries’?

Most travel surveys to measure behaviour change are based on a single day for each household, rather than a full week, although travel behaviour for any individual will vary by day of week. This is most notable, on a systematic basis, between weekdays and weekends, with most trip purposes, other than leisure and recreation and some types of shopping trip, being focussed on weekdays. The rationale behind a daily diary is precisely that this differentiation is systematic and therefore can be picked up

by appropriate sampling on each day of the week. Daily diaries should represent each day of the week equally in terms of persons or households.

A weekly travel diary will pick up the variation between days for individuals and individual households, but does so at the expense of:

- A lower response rate, as fewer people are willing to commit to the extended commitment;
- A higher attrition rate, as people cease to record their travel during the week; and/or
- Lower quality data towards the end of the survey week, as the ‘novelty’ wears off.

Control Groups

Behaviour change initiatives do not operate in isolation. Almost by definition, such initiatives are funded because there is a sympathetic government policy environment. In recent times, the increasing price of petrol will also be having a systematic impact on travel behaviour. This requires that, wherever possible, we identify ‘control groups’ of people or households similar to those subject to the intervention, so that the impact of factors external to the intervention can be identified and appropriate adjustments made to the measured outcomes in the intervention group.

This poses a problem for interventions that do not attempt to measure change over the whole target population, as there is a potentially high degree of self-selection in the intervention group that makes them different from any potential control group.

The key criteria for a control group include:

- Spatial location
- Demographics (age, gender, household size/structure, income, education, employment type and status)
- Opportunities in the relevant area (eg similar transport systems and activities)
- Exposure to the same exogenous factors such as petrol price, public transport fares and transport system development.

The only group that, in principle, meets all of these criteria (subject to sampling variability) would actually be a random sample from the population of the intervention area itself. This is particularly so with a very large-scale application that covers a spatially-large and demographically-diverse area. In practice, however, even those who are not part of the target population are highly likely to be influenced by the intervention itself (see ‘Diffusion into the Broader Community’, below).

Control groups, where used, are usually external to the intervention community and any measured travel behaviour changes in them have been small, as would be expected over a short period of 12 months. In recent years, the major systematic influence has been from increasing fuel prices and it is highly likely that the same impact would be found in both the control group and the identified target population. The effects of this can be unexpected, however – for example, in the recent North Brisbane project, the petrol price in March/April 2007 (‘after’) was very similar to the price in March/April 2006 (‘before’), but in between there was an increase (followed by a decrease back to earlier price levels). The reduction in price leading up to

March/April 2007, coming after a 2-year period of sustained price increases, could account for a measured increase in car use in the control group (Ker, 2008).

Repetition of Results

Experience with repeated application of *IndiMark*[®] (Figure 5) shows a high degree of consistency with:

- greater variability of outcomes for small applications (consistent with statistical expectations resulting from smaller survey sample sizes); and
- larger reductions in car driver trips (and lower variability) for larger applications.

The probability of all of these outcomes being over-estimates of the population value (let alone sufficiently large over-estimates to invalidate the principal evaluation conclusions for VTBC) is very small and gets smaller with every repeated application.

Alternatively, if the individual projects were to be aggregated into a single notional 'program' with a number of different spatial locations for evaluation purposes, the total sample size would now exceed the theoretical sample size required for measuring a level of change that is less than a quarter of that actually achieved. If these had been undertaken as one project, even this requirement would have been achieved. The consolidated result for Perth TravelSmart projects, including locations chosen for their likely poor performance (Marangaroo and Cambridge), is a 10% reduction in car driver trips (<http://www.dpi.wa.gov.au/travelmart/14960.asp>).

This conclusion is supported by the Australian Greenhouse Office (AGO, 2006, p53):

For households, predicting the effectiveness of a Travelsmart project is now essentially a solved problem. The eleven Australian projects evaluated here join over 50 other formal evaluations conducted internationally and many other informal assessments (See Ker 2003, Maunsell Australia 2004 and UK Department of Transport 2004 for a list of some other evaluations.) While individual outcomes vary with geographic location, what can be said broadly is that community-based household projects will achieve a reduction in car travel of 5–15%, and this change appears to be sustained for several years without further intervention. Methods for achieving these results are now well understood, and further evaluations are unlikely to add much to existing knowledge.

REPORTING BEHAVIOUR CHANGE

Differences in Behaviour

It is intrinsically more difficult to measure changes in behaviour than behaviour itself, largely because changes can only be measured as the difference between behaviour before and after. Estimation of differences is, therefore, affected by all of the issues associated with the measurement of both the before and after states.

In principle, this problem can be eliminated in the limited context of measuring travel behaviour across the whole participant population (as, for example, with early *Travel Blending* applications), with the before and after travel diaries being an integral part of the overall intervention. Unfortunately, this provides no information about

behaviour change among those who do not participate right through the process and no generalisation to the whole population is possible.

For those interventions that do measure behaviour change across the whole identified target population, irrespective of their level of involvement (including zero), it is still possible to make some useful assessment of the reliability of estimates.

So, let's look at a specific example. The North Brisbane Household TravelSmart project achieved an estimated 13.4% reduction in car driver trips (8 percentage points, from 58.4% to 50.4%), which is significantly different from zero with a 99%-plus probability⁴ (Socialdata, 2007). However, the tests do not estimate the potential range of the magnitude of the difference.

This result can be used inferentially to estimate the upper and lower confidence limits for the value of the difference between the means:

- The 'before' and 'after' surveys each have a sampling distribution for the estimated mean value of car driver trips;
- The sample size for each of the surveys is sufficiently close that we can assume, for practical purposes, that the sampling distribution for each is the same;
- The sampling distribution can be described in terms of the standard deviation;
- A 99% probability that the proportion of car driver trips was lower after IndiMark[®] means that there is less than 1% overlap between the two sampling distributions;
- With a normal distribution, 99% of the sampling distribution is within 2.5758 standard deviations of the mean;
- It follows that the difference between the estimated means is at least 5.15 standard deviations with 99% probability (Figure 3);
- Whilst the distributions of the 'before' and 'after' sample means indicates that the true value (99% probability) could be anywhere between zero and 15.6 percentage points (26.8% of previous car driver trips), the distribution is heavily weighted towards the mid-range;
- The difference between any two normal distributions is itself normally distributed. A normal distribution with a 99% probability range of 0 to 15.6 has a standard deviation of 3.0.

Since Figure 4 shows an 80% probability of the reduction in car driver trips being between 3.8% and 12.2% of all trips and the range outside that evenly distributed above and below that range, the probability of the reduction being greater than 3.8% is 90%. There is a 10% probability that the reduction exceeded 12.2% of trips.

In terms of the more commonly stated percentage reduction in car driver trips, these figures are equivalent to:

- An 80% probability of the reduction being between 6.4% and 20.4%;

⁴ Target Group net sample size 1,309 ('before') and 1,381 ('after'), with 76% and 79% response rates, respectively. Total population: 277,000 people or 113,000 households.

- A 90% probability of the reduction being greater than 6.4%; and
- A 10% probability of the reduction being greater than 20.4%.

Figure 3 Sampling distribution of the mean (Before (P1) & After (P2) IndiMark®)

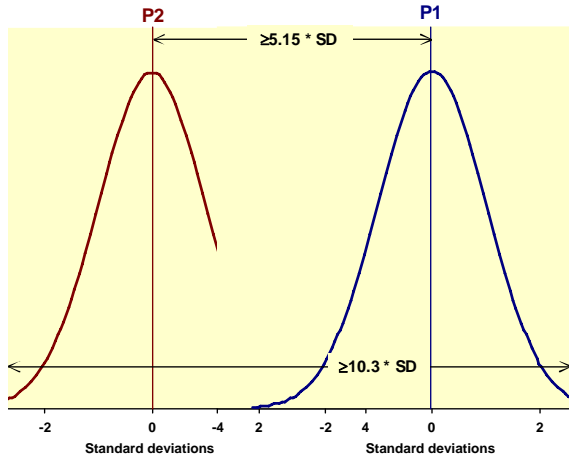
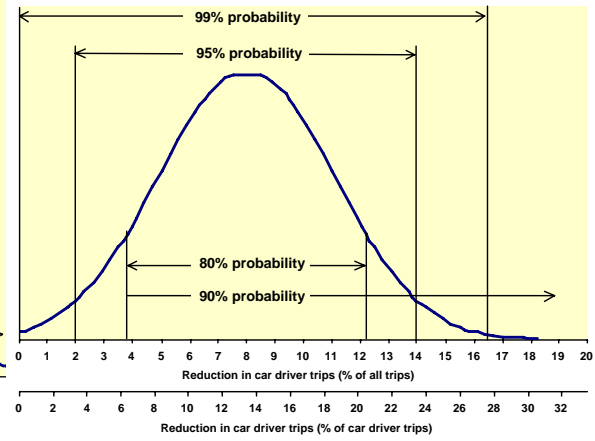


Figure 4 Sampling distribution of the difference between means



If we redo this for a 95% probability that the difference exceeds zero and for a more 'typical' measured reduction of six percentage points or 10% (from, say, 60% car driver trips to 54%), we get the following:

- The range of estimates will be between zero and 12 percentage points (20% of car driver trips);
- There is an 80% probability of the reduction being between 2.1 and 9.9 percentage points (3.5 to 16.5%);
- There is a 90% probability of the reduction being greater than 2.1 percentage points (3.5% of car driver trips);

There is a 10% probability of the reduction being greater than 9.9 percentage points (16.5% of car driver trips).

Population or Participants?

A specific issue that has received little attention is that intensive interventions may have substantial impacts on particular individuals or households but, because of their intensive or intrusive nature, may achieve lower participation rates. It is essential that comparative evaluations are on a like-for-like basis and, given that the strategic objectives for travel behaviour change are population-based, reflect the outcomes across the identified target population as whole, not just for the participating individuals or households.

IndiMark[®] reports travel behaviour change across the whole identified target population, including those who do not actively participate. This approach also facilitates the use of control groups (qv) not affected by the intervention. Although one would not normally expect external factors to have a major impact on travel behaviour over the typical 12-month period between the 'before' and 'after' surveys, the rapidity of petrol price changes in recent years is an exception to this.

Travel Blending[®] and other approaches that use travel diaries as an integral part of the process, are only able to report outcomes across the participating population – and specifically only those who complete both before and after travel diaries. Because the participants self-select through their choice to be part of the program, and there is a further stage of self-selection in the 'loss' of participants between the Stage 1 and Stage 2 diaries, the only measurement is of a demonstrably non-representative sample. Perkins (2002) estimated that the reported results of Travel Blending in South Australian pilot projects would have to be factored down by 0.6 to estimate population-wide outcomes on a basis comparable to *IndiMark*[®].⁵

⁵ This is not a criticism of *Travel Blending*[®] alone. It is an observation of the difficulty of identifying policy-relevant outcomes from interventions that have the measurement tool as an integral part of the process. Behaviour change can only be estimated for those who participate right through the process and cannot be extrapolated to any larger group.

ISSUES RAISED BY VOLUNTARY TRAVEL BEHAVIOUR CHANGE

Engagement of the Community

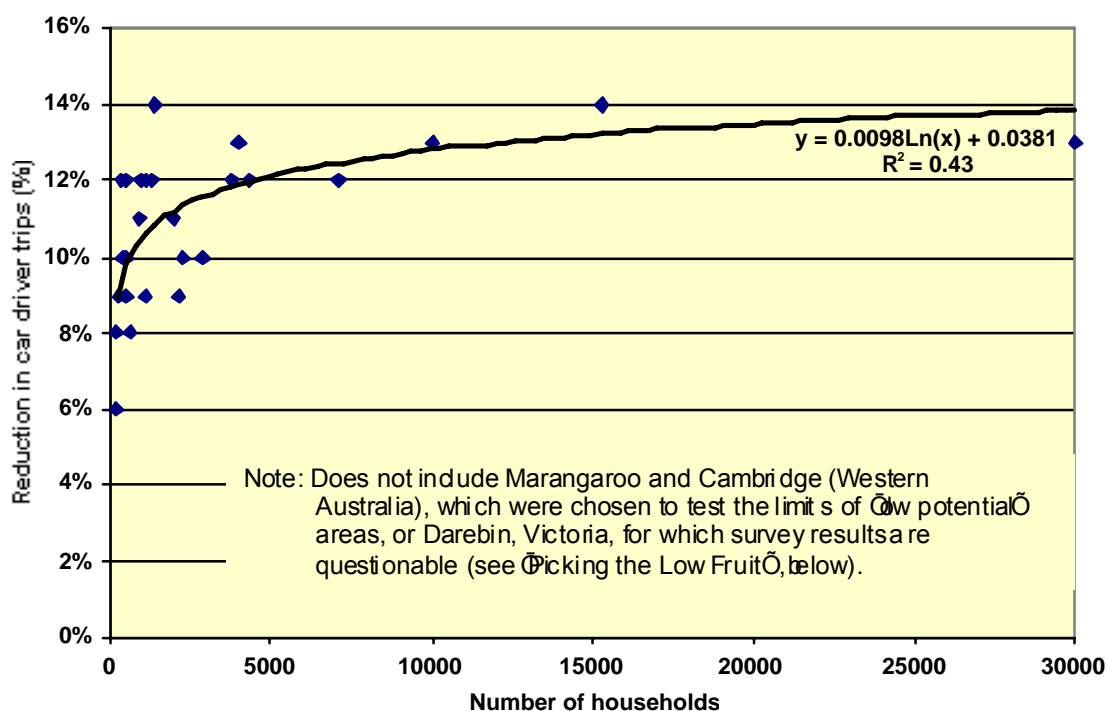
A key success factor for any program of VTBC is the engagement of the community. According to AGO (2006, p23):

There appears to be a correlation between changes in travel behaviour and:

- *personal engagement*
- *individualising materials to people's particular circumstances*
- *scale of the intervention – securing community support, as well as individual participation*
- *public visibility of the project (this is less important than the other points)*

The first two of these are difficult to measure directly, but *marketing indicators* (see 'Indicators of Change', above) may provide a useful proxy. However, there is clear evidence that the scale of travel behaviour change intervention has an impact on the outcome measured in terms of the change in car driver trips (Figure 5).

Figure 5 Travel behaviour change outcomes and scale of intervention (IndiMark®)



Public visibility will have a more substantial impact where diffusion is designed into the project.

Diffusion into the Broader Community

Diffusion is not a theoretical statistical concept, but a very real contributor to behaviour change across the whole population – including those outside the identified project population. It is well-known in many areas of commercial marketing. Diffusion relates to the extent to which awareness, attitudes and behaviours are transmitted within a population without external intervention (see box).

In the earlier applications of travel behaviour change, diffusion could only take place outside the area of application (as the total population was targeted). Since before and after surveys are only carried out in the identified target population, there has been no measure of any diffusion effect into the wider community. Indeed, to the extent that the diffusion effect might influence the control group (where one was established), diffusion would have led to under-estimation of car driver trip reduction in the identified target population.

Diffusion

There are three main models of innovation diffusion models, each arising from a different account of how innovations spread (Young, 2007):

- *Contagion*. People adopt an innovation when they come in contact with someone who has already adopted.
- *Social threshold*. People adopt when enough other people in the group have adopted.
- *Social learning*. People adopt once they see enough evidence among prior adopters to convince them that the innovation is worth adopting.

Contagion was deliberately avoided in the original South Perth pilot project, in order to test the effectiveness of the IndiMark[®] methodology itself. Subsequent larger-scale applications have not attempted to limit contagion, but have effectively kept its influence low by targeting a very large proportion of the population of an area – many of each person's contacts will already be part of the initiative.

Social threshold effects are most likely to occur where the innovation is clearly visible to the casual observer – as in the case of clothing fashion or the use of mobile phones. Travel behaviour will have some elements of this (eg if you see more people cycling), but in any area such visibility is limited by the extent to which travel in an area is undertaken by people from outside that area.

Social learning provides an explanation of why a person would adopt an innovation given that others have adopted it – the adoption decision flows directly from expected utility maximization. Specifically, the decision is based on reason to believe the innovation is better than what he is doing now, where the evidence comes from directly observing the outcomes among prior adopters.

In North Brisbane, Queensland, only 66% of households were contacted as part of the Household TravelSmart project. This approach simplifies and reduces the costs of making initial contact to establish a target population. At the same time, it leaves 'space' within total population of the area for diffusion to occur.

The measured travel behaviour change for the target population was consistent with expectation from previous IndiMark applications (13.4% reduction in car trips). There was no measurement of travel behaviour change in the rest of the area population.

The North Brisbane project created space in the intervention communities in which *contagion*, *social threshold effects* and *social learning* could be manifest – only 66% of households were directly contacted. The very large scale of the North Brisbane project will have enhanced the likelihood of *social threshold* effects. There is also likely to have been a diffusion impact from several component (sub-area) interventions being under way at any point in time.

The key indicators of the extent to which the *social learning* model is likely to apply are those relating to the satisfaction of participants with first the process and second the outcomes (ie their actual experience with the non-car alternatives they use).

The evidence regarding the process is largely anecdotal and overwhelmingly positive. It is extremely rare for people to express dissatisfaction with the TravelSmart/IndiMark[®] process, whereas many positive responses are received.

The evidence regarding the experience of the non-car alternatives is more tangible. In the North Brisbane project, the 'after' survey of travel behaviour was undertaken in March/April 2007, between 3 and 12 months after the TravelSmart/IndiMark[®] intervention (the intervention was progressively rolled out across the area over a nine-month period, April-December 2006). It is unlikely that those who had a negative experience with the non-car alternatives would still be using them months later.

There are sound reasons to believe that diffusion effects will work better with large-scale than smaller applications:

- Materials for travel behaviour change are available for the target area only;
- The larger the target area, the greater is the proportion of participants' family, friends, work colleagues and others (to whom information and attitudes might be transferred through such means as general publicity and 'word of mouth') who are likely to live in the area covered by the intervention;
- A large-scale project takes place over a longer timeframe, creating additional opportunities for diffusion while the project is still in progress; and
- A large-scale application increases the likelihood that when a household moves to a new location it will be able to access comparable location-specific information such as local access maps and public transport timetables in a familiar form.

Induced Demand

Stopher (2003) argues that reduction in car driver trips from one part of the community will improve traffic conditions and induce additional car use. This is not a valid argument for car travel generated by households in the target area, as any such effect will already be included in the survey estimates. It might, however, have some validity for car trips generated beyond that area, primarily for trips undertaken in the most congested times and places in peak periods.

No evaluations have attempted to estimate any such effect largely because it is likely to be diffuse and could only be estimated through a detailed network-based traffic model. The author is not aware of any successful attempt to incorporate travel behaviour change into a network-based travel demand and traffic model, although the requirements for doing so have been established (Ker, et al, 2005). It should be noted, however, that any such analysis would have to deal with statistical and modelling errors inherent in all traffic models.

The only reported measurement of VTBC impacts on actual road traffic volumes was for Darebin, Victoria, Australia. This appeared to show a reduction, relative to the metropolitan Melbourne average, of between 1% and 2% on roads outside the project area and between 2% and 3% on both main and local roads within the project area (Richardson, 2005). These data are consistent with diminishing impact of travel behaviour change by Darebin residents with distance from Darebin, but it is not

possible to disentangle the extent of induced traffic from outside Darebin resulting from the lower traffic volumes. The same general point is made by ITP (2007, p110).

The practical value of the Darebin data is significantly reduced by apparent problems with the other measurements of travel behaviour change for Darebin, including low response rates and sample losses from the panel.

Picking the ‘Low Fruit’?

Figure 5 does not include three interventions that produced very low car trip reduction. These are:

- Marangaroo (4%) and Cambridge (7%) in Western Australia, which were specifically chosen to test the lower bounds of achievable outcomes. Both had poor public transport, high car use and poor provision of local facilities (important for walking and cycling). Including these would have introduced substantial variation in a range of variables that were not represented in the regression.
- Darebin, Victoria, which had a low survey response rate (50% or 1346 responses out of 2772 sample) for the ‘before’ survey and substantial further losses for the ‘after’ survey (881 responses out of 1346 in the ‘before’ survey – Richardson, 2005). Differences in response rates have been shown to affect the reported travel activity (Brög & Erl, 1999) and the sample losses between the ‘before’ and ‘after’ surveys are likely to be statistically biased.

This prompts the question of whether these outcomes shown in Figure 5 are the result of ‘picking the low fruit’ or selecting target areas with high potential (Bonsall, 2007). The Marangaroo and Cambridge examples indicate that there is potential for this to occur, but the wide range of situations (for example, the pre-existing car driver mode share in IndiMark applications in the UK and Australia varies from less than 50% to 60%) in which comparable results have been achieved indicates wide applicability rather than ‘low fruit’, albeit in the sensible policy context of not choosing overtly unfavourable areas.

Limited analysis of Australian IndiMark interventions has not demonstrated any strong relationships between outcomes and socio-demographic parameters (Ker, 2007). However, this analysis was undertaken for the purposes of providing benchmarks for evaluation of a specific project. A more rigorous multi-variate analysis would potentially allow interventions from other countries to be included and issues of multi-collinearity to be dealt with.

The recent North Brisbane Household TravelSmart project has demonstrated that a wide range of values for key socio-demographic parameters does not preclude the achievement of car driver trip reduction at least comparable to those achieved in smaller, more homogenous, and apparently favourable locations. The North Brisbane area included areas with (ABS 2007):

- 1.8 to 3.1 (whole area 2.5) persons per household;
- 4% to 27% (whole area 12%) of households having no car;
- 42% to 77% (whole area 54%) of population in employment;
- 21% to 62% (whole area 38%) managerial/professional employment status; and
- 40% to 63% (whole area 53%) of employed people driving their car to work.

Durability

There is limited evidence of the durability of travel behaviour change outcomes, although what there is tends to show sustained reductions in car use for 4-7 years (Roth, et al. 2003). Bus ticketing data in Cambridge, Western Australia, has shown a consistent (and, if anything, increasing) increase in public transport use (Figure 1).

The most likely time for 'disillusionment' to be experienced and reversion to previous behaviour to occur, is within the first few months of the changed behaviour. Since 'after' surveys are typically undertaken 6-9 months after the intervention, it is unlikely that those who had a negative experience with the non-car alternatives would still be using them months afterwards. According to Maunsell Australia (2004a, p57), *previous experience indicates that for household/community initiatives there appears to be some reversion to previous travel choices over the first nine months following the TBhC project but that people who have not reverted by this time tend to stay with their new travel choice.* In other words, the 'after' survey will already incorporate the most substantial part of any 'reversion' to previous behaviour.

There are strong arguments to support at least medium-term durability, provided those who change continue to experience the same level of service in their newly-adopted modes. If there are doubts about the durability of behaviour change, it would be appropriate to use a shorter period for sensitivity testing. New Zealand uses a 10 year evaluation period for travel behaviour change and other 'soft' policies (Maunsell Australia, 2004a). At typical public sector discount rates, this would reduce the present value of benefits by 40% compared to a 25-year evaluation period. Whilst this is a substantial reduction, the impact on benefit-cost outcomes is unlikely to be critical, with benefit-cost ratios in excess of 30:1 typically being estimated for *IndiMark*[®] projects with 25-year evaluation periods.

CONCLUSION

Voluntary travel behaviour change (VTBC) aims to reduce car driver trips without specific investment in physical infrastructure or transport services or regulation of transport activity (including pricing). The techniques used in the household context differ in whether they deal with the whole identified target population, the means of identifying those who participate actively, the methods of participation and the way of assessing the extent of travel behaviour change.

Some interventions (such as *Travel Blending*[®] and *Living Neighbourhoods*[®]) work only with self-selected participants and measure the behaviour change for those participants only. Others (primarily *IndiMark*[®]) measure behaviour change across the whole identified target population. This difference has led to some confusion in the literature, with *IndiMark*[®] results being wrongly 'scaled back'.

VTBC is almost unique among transport initiatives in that it has been developed from a strong theoretical and observational basis, developed through a series of interventions with proper experimental design (including control groups) and has been subject to comprehensive monitoring and evaluation of outcomes with the process and outcomes being widely documented in the public domain. Despite (or perhaps because of) that, it has been subject to more intensive scrutiny than any comparable development.

Much of this scrutiny has focussed on the sample size requirements for measuring quite small changes with a satisfactory degree of statistical reliability. In practice, measured estimates of travel behaviour change have consistently been in the range 5-15% reduction in car driver trips. This consistency, repetition of results from successive applications and the cumulative sample size now achieved has countered doubts about effectiveness based on the method of measurement.

The simplest way of overcoming any remaining issues of statistical reliability in measuring change for an individual VTBC initiative is to undertake very large-scale interventions. This reduces the relative sample sizes required to achieve a specified level of statistical reliability and, hence, the cost of monitoring relative to delivery. A recent application in North Brisbane, to 74,500 households out of an area population of 113,000 households, has shown that the change in car driver trips is significantly different from zero with 99% confidence – equivalent to a 90% probability that the reduction in car driver trips exceeds 6.4% (or 3.8 percentage points).

Very large-scale interventions, appropriately designed, also create substantial opportunities for achieving travel behaviour change through diffusion at little or no cost to the project. Further research into the extent of such diffusion would be useful.

Confidence in the estimates of outcomes is further enhanced by consistent high survey response rates, in the case of *IndiMark*[®], which minimises the effects of non-response bias.

Further development is required of the potential for direct or indirect measurement of car driver trips/travel as an alternative to surveys, to provide robust estimates of the primary travel behaviour outcome. This might be achieved through measurement related to household vehicles rather than to individual members of households. However, such approaches do not automatically overcome issues related to sampling and non-sampling errors associated with surveys.

Further research would also be desirable into the extent of induced car travel resulting from the reduction car traffic generated by the population of the intervention area. In particular, this should focus on the circumstances in which such induced traffic might be significant.

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