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Publisher Routledge

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New Political Economy

Publication details, including instructions for authors and subscription information:

<http://www.informaworld.com/smpp/title~content=t713439457>

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Adrian Kay ^a; Alex Marsh ^b

^a Department of Politics and Public Policy, Griffith University, Brisbane, Queensland, Australia ^b School for Policy Studies, University of Bristol, Bristol, UK

To cite this Article Kay, Adrian and Marsh, Alex (2007) 'The Methodology of The Public Choice Research Programme: The Case Of 'Voting With Feet'', New Political Economy, 12: 2, 167 – 183

To link to this Article: DOI: 10.1080/13563460701302943

URL: <http://dx.doi.org/10.1080/13563460701302943>

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The Methodology of The Public Choice Research Programme: The Case Of 'Voting With Feet'

ADRIAN KAY & ALEX MARSH

In the half century since 'A Pure Theory of Local Expenditures' was published, as a contribution to the debate over the efficient provision of public goods, the paper has become a mainstay of the public choice approach to public finance.¹ The core of Charles Tiebout's argument – that near optimal provision and allocation of local public goods can result from consumers' making locational choices between competing local jurisdictions offering different tax-service bundles – is a discernible strand in many discussions of local fiscal arrangements and the structure of local government. There is a voluminous literature devoted to testing, critiquing, defending and refining the Tiebout model. For example, a decade ago Keith Dowding, Peter John and Stephen Briggs were able to review some 200 empirical studies that had sought to test for Tiebout effects.²

The model has also generated a fascination among policy makers. For example, the logic of fiscal mobility runs through the 1986 UK Green Paper *Paying for Local Government*, which led to the *Local Government Finance Act 1988* and the introduction of the community charge. Notwithstanding that experience, in 2002 the central government department the responsible for local government in England – the Office of the Deputy Prime Minister³ – commissioned a review of the links between finance and non-finance elements of local government to include the Tiebout literature.⁴ The review noted that much of the literature on Tiebout originates in the USA and concludes that the Tiebout model is less relevant in the UK because the structure and funding of local authorities differ significantly. However, the annual furore over increases in property-based local taxation in recent years, including the jailing of a 73 year old pensioner for non-payment, has reopened the debate about appropriate mechanisms for raising tax revenue locally.⁵ In this context, the issue of fiscal localism and the possibility of local income or sales taxes – which do not currently exist in the UK – have

Adrian Kay, Department of Politics and Public Policy, Griffith University, Nathan Campus, Brisbane, Queensland 4111, Australia. Alex Marsh, School for Policy Studies, University of Bristol, 8 Priory Road, Bristol BS8 1TZ, UK.

been raised, and this seems to have done little to dampen policy makers' interest in Tiebout and the idea of fiscal mobility.

This first part of this article presents a brief history of the vast but rather disparate Tiebout literature organised around two key questions. First, how well has the Tiebout model travelled from its US origins to other institutional contexts, notably the UK? Second, does the way housing markets function in practice support the contention that the Tiebout mechanism operates? More simply, why do people move? Our history reveals the key characteristic of the Tiebout literature as that of enduring among scholars and policy makers interest alongside complex, ambiguous evidence of the model's predictive accuracy.

The second part of the paper investigates why we observe this characteristic. The starting point is the view that the Tiebout model is an ideal type model, of which the principal value is heuristic rather than representational; this underlies the claim that it is the divergence between the reality and the model that allows us to identify clearly which institutions are important. However, although this is a valid defence of Tiebout model, we cannot find it within the empirical literature, which tends to judge the model in terms of its predictive value. Instead, we argue that it is the difficult and interesting methodological problems that arise in the evaluation of formal political economy models that help explain how the Tiebout model is sustained in the face of anomalous evidence. In the testing of any theory, it is not possible to falsify a single hypothesis in isolation, such as that which posits that instrumentally rational agents will move on the basis of relative attractiveness of different locational tax-service bundles. This is because it is invariably conjunctions of hypotheses that are being tested.⁶ As set out in the next section, the Tiebout result hinges on seven assumptions or auxiliary hypotheses that can never be known to hold with certainty. All we can say when testing the Tiebout model is that it is the conjunction of the particular, single-target hypothesis with a group of auxiliary hypotheses which is false, in the sense of being in conflict with some piece of empirical evidence; the supporting, auxiliary hypothesis may be responsible for the anomalous evidence.

This is a significant problem for the falsificationist methodology because of the implication that single-target hypotheses can always evade falsification on the grounds that we never know which of the conjunction of hypotheses is false. In the case of anomalous evidence, this may be a useful feature in sustaining a research programme. Imre Lakatos' theory of the methodology of research programmes has previously been applied in the economics discipline and represents an important response to these limitations of falsificationism.⁷ Lakatos argues that researchers in practice arrive at consensus on which parts, within the grouping of hypotheses, to revise in response to a refutation. He distinguishes between the hard core of a research programme that is never refuted and the protective belt, which is adjusted, changed and supplemented in response to falsification. The article considers the Tiebout model and wider discussions of fiscal mobility as part of the protective belt that encircles the hard core of the public choice programme. Using this insight, we speculate more generally in the final section about the status that formal public choice models should have when they are difficult to confirm or deny.

The pure theory of local expenditure

Tiebout's model is simple, built on seven assumptions:

1. Consumer-voters are fully mobile and will move to that community where their preference patterns, which are set, are best satisfied.
2. Consumer-voters are assumed to have full knowledge of differences among revenue and expenditure patterns and to react to these differences.
3. There are a large number of communities in which the consumer-voters may choose to live.
4. Restrictions due to employment opportunities are not considered ... assume ... that all persons are living on dividend income.
5. The public services supplied exhibit no external economies or diseconomies between communities.
6. For every pattern of community services ... there is an optimal community size ... defined in terms of the number of residents for which this bundle ... can be produced at the lowest average cost.
7. Communities below the optimum size seek to attract new residents to lower average costs. Those above optimum size do just the opposite.⁸

The key argument is as follows:

The consumer-voter may be viewed as picking that community which best satisfies his preference pattern for public goods ... Except when this system is in equilibrium, there will be a subset of consumer-voters who are discontented with the patterns of their community ... Given the ... assumptions ... movement will take place out of the communities of greater than optimal size into communities of less than optimal size. The consumer-voter moves to the community that satisfies his preference pattern.⁹

Mobility can be seen as the means of registering demand, and the resulting distribution of households will approximate to an ideal market solution. Hence, contrary to prominent arguments circulating at the time, notably those by Paul Samuelson, Tiebout argued that for *local* public goods at least it is possible, in principle, for a near optimal allocation to arise through a process of 'voting with feet' analogous to the market.

Tiebout is clear that the model he presents is offering a solution at a conceptual level and was well aware that the simplifications underpinning the model are radical.¹⁰ Indeed, he refers to the model as 'extreme'. Most evidently, by labelling the model a 'pure' theory, the implication is that this is a highly stylised version of reality. For example, the model avoids questions such as strategic action by assuming that taxes and benefits are largely fixed from the point of view of local decision makers.

The founding assumptions of the Tiebout model that are particularly significant for our purposes are assumptions 1, 2 and 4, which are interlinked. Assumption 1 removes a range of complicating factors that might tie households to particular

locations. Equally significantly, it sets the model up so that choices between different tax-service bundles will result in mobility, rather than simply leading to dissatisfaction *in situ*. This approach is reinforced by assumption 2, which makes the assumption not only of perfect information but also that consumer-voters act upon this information. Finally, assumption 4 states that within the model mobility is not restricted due to employment opportunities and that a convenient way to rationalise this is to assume that all persons are living on dividend income. This allows Tiebout to remove one major potential source of friction from the model. The foundation of the model are thus perfectly informed and perfectly mobile households whose principal reason for moving is to optimise their consumption of local public goods.

The original article notes that factoring in the costs of relocating will result in a less optimal allocation of households to jurisdictions – the greater the relocation costs, the further from optimality the outcome will be. This is one example of the way the article recognises, explicitly or implicitly, many of the grounds upon which the model has subsequently been challenged. A second example is Tiebout's recognition that in practice consumer-voters do not have perfect information, fixed preferences or full mobility. Rather, early in the article Tiebout recognises that for households considering moving 'of course, non-economic variables will also be considered', but this can be dismissed as 'of no concern at this point'.¹¹ He later recognises that very little is known about what motivates people in choosing a community. A major question for later empirical research has been what role differences in tax-service bundles between jurisdictions play in practice.

A brief history of the fiscal mobility literature

The empirical literature on the Tiebout model is dominated by the search for the existence and strength of Tiebout effects in various local government contexts. To do this, authors have tended to adopt either an aggregate or a micro-level approach. Aggregate approaches are differentiated into those that look at city size, household sorting, capitalisation or fiscal migration.¹² Although the aggregate literature is bedevilled by methodological problems, the last comprehensive review of the literature concludes that the analysis conducted to that date, particularly of fiscal migration, can be interpreted as lending support to at least some of the implications of the Tiebout model. This conclusion is qualified, however, by the observation that '[t]he use of aggregate data to test individual decisions is problematic'.¹³

While aggregate studies of mobility can demonstrate, for example, that household behaviour is consistent with predictions derived from the Tiebout model, they cannot shed any light on whether it is the Tiebout sorting mechanism that is driving observed patterns. They can only ever provide Tiebout-consistent evidence but never Tiebout-confirming results. This is because '...validating the Tiebout model requires demonstrating a motivational link between tax-service packages and household movement'.¹⁴ It is for this reason that micro-level studies which focus upon households or individuals have been the centrepiece of empirical research on fiscal mobility. If it were possible to access household decision-making processes, then it would be possible to assess their motivations

for moving, the factors they take into account and the role that Tiebout-type concerns play in this process. This would provide crucial support for the conclusion that the patterns in the aggregate data are most appropriately attributed to fiscal mobility. The micro-level studies reviewed by Dowding, John and Briggs, while also displaying methodological flaws, seem to provide evidence that behaviour is at least Tiebout-consistent and in some cases provide some support for the behavioural foundations of the model. More specifically, they conclude that '[t]here is evidence that both taxes and service affect locational decisions'.¹⁵

This conclusion relates to the United States. The first strand of our history of the Tiebout literature uses the UK as an example and asks how well this finding travels from its US context. The second strand considers the 'residential mobility' literature and explores the puzzle of why this substantial body of work that investigates reasons for relocation has gone almost unnoticed in the Tiebout literature.

Fiscal mobility: the UK evidence

As is well rehearsed in the literature, the structure of local government in the UK is significantly different from that in the USA. The applicability of the Tiebout approach to the UK is limited because of the commitment to equalisation/redistribution, the unitary rather than federal nature of the system and the pervasiveness of inter-governmental grants.¹⁶ Kenneth Newton broadens this point when he argues that the Tiebout model relies on a number of further unstated assumptions including that there are 'no financial transfers between units of government, and no national minimum standards for local services, or other kinds of central government controls which undermine the direct relationship between local jurisdictions and consumer voters'.¹⁷ George Boyne examines the UK system in terms of a competitive local government system, across three dimensions: structure of the system; level of local autonomy; and the extent of central funding. He argues that on all counts the local government system in Britain is such as to reduce the likely salience of Tiebout effects.¹⁸ The structure of the system refers primarily to the size of local authority jurisdictions: the argument is that in the UK jurisdictions are generally too large, relative to typical relocation distances, to have to compete with each other. Similarly, the lack of local flexibility over the range and level of service that can be offered, the limits on local tax-raising powers and the heavy reliance on central government funding and equalisation grants mean that the bundle of services offered and levels of taxation do not vary sharply between localities.

There are, however, more subtle differences in the characteristics of local government between the USA and the UK that also affect the plausibility of the Tiebout model. As noted above, a component of assumption 2 of the Tiebout model is that consumer-voters have full knowledge of differences among revenue and expenditure patterns. A feature of the local public finance system in some states of the USA is the requirement to vote annually on school budgets and a more common requirement is that voters are required to approve tax increases.¹⁹ This leads to the argument that considerable information about public services and their costs is regularly presented to consumer-voters and hence that assumption 2 of the Tiebout model cannot be judged to be implausible

prima facie. Within the present UK system, local taxpayers are provided with information on proposed local taxation bills but there is currently limited public participation in budget setting. As a consequence, with the exception of particular groups such as those on fixed incomes, it would be hard to argue that this is a mechanism through which consumers in general become well informed about the composition and cost of local council spending. Equally significant for the Tiebout model is that information disseminated is rarely comparative.

Importantly, there are also problems in generalising from studies of Tiebout effects in the USA associated with variation in the size of local government jurisdictions. Caroline Hoxby suggests that when 'consider[ing] what the Tiebout process can do, I consider a housing market with many jurisdictions. . . This description. . . is clearly better for some metropolitan areas than others. Boston has 70 jurisdictions within a 30-minute commute of downtown and 130 jurisdictions in its metropolitan area, while Miami has only one jurisdiction in its metropolitan area.'²⁰ Keith Dowding and Thanos Mergoupis, in their recent study of the effect of greater fragmentation of local government in England on efficiency, add a further complication to the investigation of Tiebout effects in the USA: variation in the practices of contracting out, franchising and internal market operations between jurisdictions. They point out that no existing US study controls for these and argue that England, with a simpler structure of non-overlapping multi-purpose authorities and little variation in new public management practices between local authorities due to UK law, might be a better test case for the model.²¹

There are two principal studies testing for Tiebout effects using micro-level data in England. The first examined mobility in four London boroughs in the period covered by the community charge; the second study used data from a 1997 survey of the three largest metropolitan areas in England.²² The aim of the initial study was to test for Tiebout effects in the most favourable situation that can be envisaged. The authors selected four London boroughs with very different tax-service bundles. In this context short distance moves that embody adjustment in the light of differences in tax-service bundles are possible. The logic of the approach is that if Tiebout effects are not identified in this situation, then they are very unlikely to be found in the less favourable local government contexts elsewhere in the UK. Conversely, if they are found in this particular London context, this is an initial step but does not represent a strong basis for generalisation.

A sizeable proportion of the respondents to the survey cited taxes and services as a factor in their locational decision making and the authors argue that these respondents are also Tiebout-rational in the sense that they move to areas that improve the tax and/or service bundle they can consume. The authors conclude that the behavioural assumptions of the Tiebout model are corroborated for the sample area. They noted that whether this result would be confirmed once local taxation shifted from a poll tax to the council tax – a move that was then occurring – needed to be tested rather than assumed.

The second main study used a purpose-built survey to investigate fiscal mobility in England's three largest metropolitan areas – London, Greater Manchester and the West Midlands. The analysis of the survey is broken down into push factors,

which influence the decision to exit a jurisdiction, and pull factors, that affect decisions about where to move to. In addition, the survey produced data on reported satisfaction with local government services and thus allowed for the estimation of the effect of local government structure on satisfaction. The main findings were again some evidence of fiscal mobility, most prominently in the pull of locations with access to good public transport and schooling. These effects varied with distance of move: good public transport affected intermediate-distance moves within metropolitan areas and long-distance moves, whereas proximity to good schools affected moves within districts more strongly. However, despite this evidence of fiscal mobility, the study produced the result that the three largest metropolitan areas in England would benefit from reduced government fragmentation. The standard Tiebout logic – that increased fragmentation increases efficiency because it facilitates ‘voting with feet’ by expanding the jurisdictional choice set of mobile citizens – was thus reversed in this case.²³

These studies testing for Tiebout effects in the UK reveal a common difficulty in stating criteria for falsification in the empirical evaluation of models.²⁴ If no moving households take taxes and services into account when relocating, then clearly Tiebout effects are irrelevant and the market analogue that Tiebout proposes fails to hold. At the other extreme, if all movers act as Tiebout proposes then the model is corroborated. The reality is likely to lie somewhere in between. The question is, what proportion of movers need to act as Tiebout proposes for the market analogue to hold? John, Dowding and Briggs recognise that ‘[t]he number required is moot’.²⁵ They draw on the study by Teske *et al.* and propose that if 10 per cent of consumers discriminate on price, then this is sufficient to generate a market. However, this number is contested by David Lowery, William Lyons and Ruth DeHoog, who argue that Teske *et al.* had no real basis for the figure of 10 per cent.²⁶ Indeed, the limited evidence invoked to support the 10 percent figure is in part drawn from markets for furniture and appliances. Hence, it is necessary to proceed by analogy from the market for furniture or appliances to the market for local government tax-service in order for this evidence as having any bearing on the issue being discussed. These are grounds for debate over what proportion of Tiebout-rational consumers is necessary for a market analogue, and this has significant consequences for what is accepted as confirming or disconfirming evidence.

Did the study by John *et al.* identify enough such consumers? In their exchange with Kenneth Newton, Dowding and John observe: ‘We think so; Newton thinks not.’ They recognise that ‘[e]conomics has not demonstrated the percentage of consumers requiring product knowledge in order to generate market competition, so we cannot be assured of the market analogue. Nor can Newton be assured of the contrary.’²⁷ This is an important point akin to the ‘how big is big?’ example discussed in Deirdre McCloskey’s study of the rhetoric of statistical testing in economics.²⁸ She argues that the answer can only be settled by debate, persuasion and consensus building, rather than being resolved by appeal to ‘the facts’ or objective knowledge.

This is a problem for John *et al.*, who are explicit in drawing their methodological inspiration from Popper’s falsificationism: ‘That is Popper’s method, that is our method.’²⁹ For a proposition to be falsifiable, it must be at least *in principle*

possible to make an observation that would show the proposition to be false, even if that observation had not been made. However, the limiting proportion of total movers being Tiebout-rational, which if observed would falsify the model, is not clear; for John *et al.* it lies somewhere in the range between 10 per cent of movers and the limit of zero where no-one is Tiebout-rational. However, that range remains moot in terms of what constitutes falsification.

The residential mobility literature

Almost contemporaneous with the publication of Tiebout's paper was the first edition of Peter Rossi's seminal *Why Families Move*.³⁰ Rossi employed a life cycle model to frame his empirical study and used sample survey data collected in four areas of Philadelphia. A key finding of the study was that a sizeable minority of moves could be seen as what Rossi labelled 'forced' moves. These include involuntary moves (such as evictions) and moves consequent on marriage, divorce or unemployment. In total, 39 per cent of moves in Rossi's sample were categorised as 'forced' rather than a result of a free choice. Rossi then pursued the detail of those who had a complaint about their current residence and were thinking of moving, and examined the specifications that movers had for the characteristics of a desirable alternative dwelling. Almost half of those with a complaint about their current housing circumstances focused that complaint upon the space offered by the dwelling, some 14 per cent referred to neighbourhood and 12 per cent to costs. Of those identifying neighbourhood as the issue, the principal concern was social composition while 13 per cent cited services and 15 per cent other aspects.

There now exists an extensive literature on residential mobility.³¹ The key messages to emerge from this literature can be briefly summarised. Most residential mobility takes place over short distances, within metropolitan areas, and renters are more mobile than owners because they face lower transaction costs and have typically made less of an emotional investment in an area. Younger people are more mobile than old and this in turn is because mobility is strongly related to the changing circumstances and significant life-events of individuals and families, which occur more frequently before the age of 35 years.

A typical starting point in the literature is thinking about housing adjustment in terms of 'housing stress'; when a household's housing requirements become significantly out of line with its current housing circumstances (a situation of housing stress) this acts as a push factor to consider relocation.³² A range of factors then come into play to act as pull factors in the selection of an alternative dwelling. The approach implies reflection, strategy and search on the part of the household in question. Yet the literature confirms Rossi's finding that much mobility and transition between tenures can be attributed to unanticipated events.³³ W. A. V. Clark and Jun Onaka provide a more comprehensive framework based on the distinction between forced, voluntary-adjustment and voluntary-induced moves.³⁴ There is some evidence that propensities to move can be rather more deep-rooted – a consequence of childhood experience of mobility or of some national cultures seeming to emphasise mobility as a solution to problems more than others. Longer-distance migration, on the other hand, is dominated by

job-related moves, with professional workers – who are more likely to access relocation expenses – and the unemployed most likely to migrate between regions.³⁵

While family circumstances and changes through the life course play a major role in shaping patterns of residential mobility, the economic context plays a significant role in, for example, influencing transitions such as the move into owner occupation. It is also clear that within a single country turnover rates, and hence mobility, can differ markedly between areas as a result of variations in broad socioeconomic factors including rates of population growth, house price inflation and new construction. Differences in mobility rates can also be identified between urban and rural areas and areas with differing levels of state intervention in the housing market, although authors differ on the direction of impact that state intervention has upon rates of mobility.³⁶

One striking feature of the residential mobility literature is that Tiebout-related issues of public services or local taxes are usually absent from, or peripheral to, discussions of reasons cited either as push or pull factors in the decision to move. Indeed, as Dowding *et al.* observe, ‘omission of factors pertaining to the effects of local government is almost universal’.³⁷ Surveys often structure questions about reasons for moving around options that relate to dwelling characteristics (size, cost, quality), job-related change, life course-related change (marriage, divorce, birth of child) and neighbourhood-related factors. Other reasons for moving are subsumed under a catch-all ‘other’ category.

The residential mobility literature appears to present the Tiebout model with a problem. The well-established finding that a substantial proportion of moves are involuntary means that for many movers the selection of tax-service bundles, or indeed any sort of rational deliberation, is not paramount among the push factors for mobility. Similarly, the survey results on residential mobility seem to suggest that for most households public services, taxes or some combination of the two are not a principal concern. In many instances taxes and services barely register. Taking a more generous interpretation, it could be argued that Tiebout effects are subsumed under the ‘other’ category and possibly the ‘neighbourhood’ category. While neighbourhood, in housing research, is perhaps more closely associated with issues of poor social or physical environment, it could be construed more broadly to include issues of public services. Indeed, this can be detected in Rossi’s original study, which points to a relatively small minority of potential movers who cite services in the neighbourhood as their key complaint. Issues such as crime could be seen as indirectly related to public services, even if households may not think of them in those terms. Similarly, in instances where the questions are not absolutely specific, it would be possible to interpret housing costs as referring to property-based taxes, where these apply, as well as the direct rental or mortgage costs. Yet even adopting such a generous interpretations only results in the conclusion that a relatively modest minority of movers are primarily moving for Tiebout-relevant reasons.

In terms of the relationship between the literatures on residential mobility and Tiebout, Dowding *et al.* argue that ‘[t]hough these studies provide useful information concerning reasons why people move, they are not helpful in explaining whether local government services and taxes affect residential mobility’.³⁸

The claim is that most residential mobility studies do not include local taxes or services explicitly on the list of possible reasons for moving presented to survey respondents. While this is indubitably true, it is important to appreciate why. Consider the standard principles of survey design and interview process methods. On survey design, let us assume that all the residential mobility research has been conducted in accordance with good survey design methodologies, according to which the response categories offered to survey questions are developed through qualitative interviewing/piloting to reflect the answers most frequently encountered, rather than being plucked from thin air. In terms of the interview process, assume that respondents for whom tax-service issues were a significant reason for moving were able to identify a category in the survey that expressed that reason (even if it is the 'other reason' category). These two assumptions about the survey method used in the residential mobility literature allow inferences about fiscal mobility from the relative scarcity of Tiebout-type reasons amongst the whole litany of reasons why people move: only a small minority of movers do so as the result of a comparison tax-service bundles in different locations.

What sustains interest in the Tiebout model

Belief in fiscal mobility continues alongside a lack of clear and unambiguous evidence of its extent. One response is to ignore the evidence or issues of methodology and instead appeal to common sense and argument grounded in practical politics: 'we suspect that if local politicians thought that one-fifth of households would consider moving in or out of their jurisdiction for tax-service reasons over a five-year period, it would make them consider very seriously the demographic effects of different tax-service packages.'³⁹ This is a version of the precautionary principle: the consequences of Tiebout being true are so great as to justify, on prudential grounds, believing in Tiebout. However, we dismiss this argument as implausible on the grounds that model does not provide such *a priori* grounds for holding it to be true, and hold that some reference to the evidence on Tiebout is crucial to any belief in the model.

Nevertheless, a sceptical and moderate position on the limited evidence can be outlined. A sceptic might accept the conclusion that Tiebout mechanisms are plausible in theory, but there is insufficient incontestable evidence to justify placing any emphasis upon them in practice. The mainstream residential mobility literature has singularly failed to detect Tiebout effects as a significant influence upon mobility. In contrast, both studies in the UK that have gone looking for Tiebout effects have claimed more success in finding them. Yet even these support a moderate position: most mobility is involuntary and not caused by the sort of comparisons of tax-service bundles proposed by Tiebout's pure theory. Instead, Tiebout effects play a secondary role, mainly in terms of pull factors affecting the selection of an alternative dwelling in a 'forced' long-distance move. Thus, although most moves are involuntary, good schools and public transport are sometimes significant in decisions of where to locate within a metropolitan area.

However, this moderate position, by accepting that people move for a host of other reasons and may not even do so of their own volition, bears little resemblance to the pure theory other than the invocation of Tiebout's name. Results

from analysis that employs spending in a particular policy field – typically education – as a proxy for the whole of the service bundle available in a particular jurisdiction lend little support to Tiebout. The survey data that reveal schools and public transport as sometimes factors in locational choice are not evidence of Tiebout-type comparisons of tax-service bundles in different jurisdictions independently prompting mobility.

How then does the literature on the Tiebout model sustain itself? Rebecca Morton stresses the importance of evaluating a model on its assumptions as well as its predictive value. Our understanding of models, and therefore the reality that we ultimately aim to represent, is improved by observing the consequences of adjustments and relaxations of different assumptions for the model's qualitative results.⁴⁰ Newton's critical response to the first UK study of Tiebout lays considerable emphasis on the unrealistic nature of the assumptions in the model and raises the broader debate within political science between those who advocate an economic approach to the analysis of political phenomena and those who see such an approach as an inadequate foundation upon which to build understanding. Dowding and John make this clear elsewhere: 'these attacks are ... not upon Tiebout as such but rather upon the economic method.'⁴¹ The position taken by John *et al.* to address this criticism is revealing. As part of their defence of Tiebout model, they argue that the assumptions underpinning the model 'are no more unrealistic than those contained in models of perfect competition'.⁴²

Unrealistic assumptions are a feature of ideal-type models and the significance of this fact within economics has been extensively debated.⁴³ In terms of the economic method, the *ceteris paribus* assumption that underpins all models in economics is the most obvious departure from reality. The 'all else being equal' claim allows holding all variables constant except the few that the model is attempting to investigate; this is intended to facilitate the isolation and clarification of that specific relationship. However, in reality all else is never equal, so economic models are always ideal in the sense of having unrealistic assumptions. In empirical terms, they also encounter the problem of joint hypothesis testing to which we alluded in the introduction and which will be developed in the next section.

In terms of the perfect competition analogy, most mainstream economics textbooks hold perfect competition as an ideal- or pure-type heuristic to facilitate the exposition of competitiveness in markets. While some economists may claim that the perfect competition model is useful for partial analysis of particular types of market activity, the general equilibrium result that at least one Pareto-optimal equilibrium obtains under highly restrictive assumptions is not one that is tested in the literature. This is because the stringent assumptions of the model do not map across well to the operation of real-world markets, leading some economists to eschew the perfect competition model as a guide to policy. For example Joseph Stiglitz argues that, in choices about economic systems, policy makers should:

... not be confused by theorems and ideologies based on an irrelevant model of the market economy. Most important, it means that in deciding on what form of market economy they might adopt,

including what role the government ought to play, they need to have in mind how actual market economies function, not the quite irrelevant paradigm of perfect competition.⁴⁴

So why is the competitive equilibrium model so prominent in mainstream economics? It is because it is a heuristically useful ideal type: the comparison between reality and the model helps to identify institutions and practices that are important because of the divergence between reality and the model. The model of perfect competition might be irrelevant for policy makers, but it is important for economic theorists. Morton generalises this point to all pure or ideal models in the social sciences: 'I cannot sufficiently reiterate that, without pure theory and the almost completely unreal early formal models of social science, our current applied models would not be possible.'⁴⁵ They help develop empirical models that yield predictions that can be tested; purely theoretical research, however unrealistic, can allow us to distinguish a plurality of predictions from different sets of assumptions in a way that assumptions aimed closely at reality might not give us such clear predictions to be tested.

Two points serve to introduce the next step in the article. The first is that the ideal type defence of Tiebout is not usually employed in the literature.⁴⁶ The ambition of the overwhelming majority of scholars is to test for Tiebout effects, to demonstrate that the Tiebout model has substantial representational rather than heuristic value. Second, even if we accept the ideal type defence as valid, we are confronted by the question of why we should favour the Tiebout ideal type among a whole range of ideal types that might help us understand the essential characteristics of residential mobility.

Lakatos' theory of methodology and public choice

Imre Lakatos argued that Karl Popper's criterion of falsifiability ignores the remarkable tenacity of scientific theories.⁴⁷ The problem of never being able to test a single, target hypothesis in isolation was mentioned in the introduction. Empirical evidence can only disconfirm a group of hypotheses allowing researchers to either invent some rescue supplementary hypothesis or re-fashion consensus around a different grouping of hypotheses to explain the anomaly. Alternatively, they may ignore it, and direct their attention to other problems: scientists talk about anomalies, and not refutations. For Lakatos, the history of science shows that scientists do not abandon a theory or model because facts seem to contradict it. He described a methodology of scientific research programmes where progress is not based on an isolated hypothesis but rather as an overall research programme. Research programmes consist of a 'hard core' of key propositions, which is tenaciously protected from refutation by a vast 'protective belt' of auxiliary hypotheses. The insights of Lakatos' work have been employed extensively and fruitfully in the analysis of different branches of economics.⁴⁸

There is also a dynamic part to Lakatos' work. Research programmes are progressive if there is an increase over time in the range of empirical phenomena for which the programme gives refutable predictions. So, as the different hypotheses in the protective belt are adjusted or reorganised in response to the challenge from

evidence, a progressive research programme increases its empirical content and the range of problems it covers. Degenerative research programmes have the opposite characteristic: over time there is a reduction in the empirical content and range of problems engaged by the programme.

There is a public choice research programme and debates about the Tiebout model, including evaluations of its assumptions and implications, are a good example of the protective belt around its hard core. In the hard core exists the assumption that there is a political market analogous to an economic market and that the state should respond to the revealed demand for public goods in the manner of firms reacting to signals from competitive markets for private goods. Further, both policy makers and citizens are rational egoists and the latter reveal their demand for public goods through various exit and voice mechanisms. These assumptions can never be the subject of destructive argument, for to falsify them would be to undermine the entire public choice research programme by having, for example, rational citizens failing to signal their preferences to government or policy makers ignoring citizens' preferences for public goods.

The Tiebout literature in the UK suggests that some factors in the fiscal mobility category have a statistically significant effect on mobility. The evidence from the residential mobility literature suggests a range of factors, which have a greater influence on mobility than tax-service bundles. The two literatures are not strictly inconsistent, but without some consideration of the relative magnitude of different effects on mobility this claim does not reveal much. Yet within the fiscal mobility part of the protective belt there is a marked reluctance to test a *range* of competing explanations for residential mobility and assess their relative importance; in particular, if patterns of behaviour are consistent with Tiebout then we detect a tendency to take this as sufficient evidence of Tiebout. Lowery *et al.* comment that '[b]eing human, we are all too inclined to interpret evidence selectively when motivated to do so, however inadvertently'.⁴⁹

The insight from Lakatos allows us to see the view that fiscal factors will induce mobility as a cornerstone of public choice analysis of public finance and part of the protective belt for the hard core public choice view of the world. The Tiebout model in the protective belt gives expression to fiscal mobility and, by demonstrating that in theory optimal allocations of public goods can result from individual consumer choices, provides a resource for those who wish to argue that more local autonomy, smaller government units, less central government intervention and less inter-jurisdictional redistribution would improve social welfare. These are normatively attractive properties for some policy makers and part of the intellectual foundation of public choice theory. As such, many scholars have a lot of intellectual capital invested in the continued relevance of the fiscal mobility. If it were ever to be rejected as failing to reflect how the world works, then the implications for the wider intellectual project of which it is a part would be substantial.

For example, the paper by Dowding *et al.* concludes: 'as is usual in social science, conclusions are tentative and disputable. Nevertheless, an impressive array of evidence suggests that the Tiebout family of models holds a number of important truths about urban politics.'⁵⁰ Either the conclusions are tentative and disputable or the literature contains a number of important truths: it is difficult to see how it can do both. The authors' statement about 'important truths'

become even more puzzling because they then seek to apply the assessment criteria of falsification inspired by Popper to the existing evidence, fully aware that the notion of 'truth' has a problematic status within Popperian philosophy. Scientific theories, for him, are not inductively inferred from experience; nor is scientific experimentation carried out with a view to verifying or finally establishing the truth of theories. Rather, all knowledge is provisional, conjectural, hypothetical – we can never finally prove our scientific theories, we can merely (provisionally) confirm or (conclusively) refute them. Hence at any given time we have to choose between the potentially infinite number of theories which will explain the set of phenomena under investigation. Faced with this choice, we can only eliminate those theories that are demonstrably false and choose between the remaining, unfalsified theories.

In such terms, the empirical claim that the residential mobility and Tiebout literatures are not inconsistent does not take us very far. The question remains why, among the plurality of unfalsified theories on residential mobility, the focus should be on Tiebout. We argue that the answer lies in the fact that the Tiebout model is a mainstay of the public choice analysis of public finance and thus attached to a broader public choice research programme. To state this in Lakatosian terms, the reason for the continued fascination with Tiebout amongst the wide range of models and explanations of residential mobility is that it is part of the protective belt for the public choice research programme.

Wider implications for testing public choice models

In this article we have drawn on two elements of the Tiebout debate to highlight the difficulty of transposing the model from its US context and the significance of the parallel literature on residential mobility. In both elements, we failed to find clear, unambiguous evidence supporting the predictions of the Tiebout model. Most of the residential mobility data provides no support for the Tiebout model; furthermore, the Tiebout model is difficult to test directly because the criteria for falsification are not clear. This raises a broad question for public choice models, and indeed formal models more generally: what status should they enjoy in such circumstances?

Our answer is that formal models should not enjoy a privileged status simply because they are part of a research programme. The Tiebout model should be separated from its public choice theory base and be considered as one explanation of residential mobility to be compared with several alternatives in the literature. We do not reject Tiebout or the fiscal mobility hypothesis: as noted previously we do not have clear criteria for falsification and, furthermore, strict falsificationism is impossible because empirical evidence can only relate to conjunctions of hypotheses never single hypotheses in isolation.⁵¹ Lakatos provides a view of how science develops groups of hypotheses in conjunction as research programmes. Alternatively, Paul Feyerabend stresses that no theories can accommodate all the facts in their domain; there are always anomalies and inconsistencies.⁵²

However, the corollary of the view that pure falsification is rare or impossible is that the proliferation of alternatives becomes an important part of intellectual progress. Alternatives are not just catalysts for progress, forgotten once they are

refuted; rather, they serve to increase the empirical content of the views that are the centre of attention or which inform the current working hypothesis, and as such they therefore a necessary part of the falsification process. Science does not necessarily eliminate older theories that have been refuted because these contribute to 'the content of their victorious rivals'.

If the consideration of alternatives is inherent to intellectual progress then we have grounds for criticising the Tiebout debate in the UK: it has ignored the range of competing hypothesis about why people move and too much weight has been placed on studies which detect weak Tiebout effects. This claim relates back to the well-known marketplace of ideas discussed originally by J. S. Mill.⁵³ The stress is on the importance of generating alternatives to the working conjunction of hypotheses; only when eliminative methods show that a working conjunction of hypotheses is in fact the only one consistent with the facts should it be accepted as true. But so long as the data do not determine a unique hypothesis, such groupings of hypotheses cannot be accepted as yielding a new truth. To reiterate, even when a new truth is accepted, it is better understood when the mental processes that lead to the conclusion are transparent – that is, when an account is given of the stages at which alternatives were considered and refuted, the arguments used, the strengths of the old views and weaknesses of the new views. Such an account would increase the reasonableness or rationality of our theoretical commitments, and is particularly important when confronted with research programmes that occlude the debate of certain views and when these research programmes become influential in policy analysis.

To emphasise, this article does not present an argument against formalism or a rejection of public choice theory. Formal models enjoy the usually adduced advantage of providing testable hypotheses and thus facilitating empirical enquiry. But they have another crucial advantage over informal models in the generation of alternatives: they facilitate the clear comparison of alternative theories and models with each other. Essential to this pluralistic view of falsificationism is that alternative groupings of hypotheses are considered and tested not individually and in isolation but against each other. This is how the process of eliminative inference, crucial to social science progress, works.

Notes

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