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What do practitioners want from research, what do funders fund, and what needs to be done to know more about what works in the new world of children's services ?

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Abstract

This article is intended as a contribution to the debate on the place of research in children's services from a funder of social research and three researchers, two of whom spent (between them) 12 years in an organisation delivering services to children and young people. We draw on two studies to inform a discussion of the kinds of research and research infrastructure needed to support high quality social care services for children. The first study identified the questions to which practitioners in social care wanted answers; the second describes the degree of 'fit' between funded research and practitioners' questions. We conclude with a commentary on what some of the solutions might be to the problems we identify, and the kinds of research infrastructure that might be needed to move towards these solutions.

Background

The question of how research gets into policy and practice is a longstanding one in applied areas (Alderson et al. 1996; Bero et al. 1998; Crosland et al. 1998; Richardson, Jackson & Sykes 1990;Sheldon 1998); although it tends to be a concern more consistently raised by researchers than by practitioners. One clear barrier to getting research into practice, identified in a number of studies (e.g. Barnardo's Research and Development 2000; Fitzgerald, Ferlie & Hawkins 2003) has been the tendency of researchers to adopt a 'push' model of knowledge transfer in which research reports thud onto desks (or appear in inboxes), rather than a ‘pull’ model in which practitioners, planners and policy makers pull down research on the topics they need, when they need it. Some steps have been taken to address this, most notably perhaps in the funding now made available for 'intermediary' organisations like Research in Practice, who perform a variety of roles to bridge this gap. In addition, the ESRC investment in What Works for Children (http://www.whatworksforchildren.org.uk/) enabled a group of researchers from City University and the University of York to work with R&D and policy colleagues in the children’s charity Barnardo’s on a suite of knowledge exchange topics, including the production and dissemination of *Evidence Nuggets* ; signposting relevant research, websites and databases; generating and helping practitioners to formulate research questions; logging practitioners’ questions; providing evidence-based answers to practitioners' questions and providing training seminars on relevant subjects.

In the UK and elsewhere, a number of factors are moving research further up the ‘to do’ lists of practitioners working in social care. These include the requirements of many programme funders for research evidence to underpin interventions, and a growing recognition of the rights of children to services based on the best available evidence. Research on what might ‘work’ is in relatively short supply in social care, where traditional strengths have been in qualitative research and in identifying need. Those who research, practice, plan or fund services in areas where there is at least as much potential to do good (or harm) as in medicine need to consider what scope there is for improvement. This is particularly the case since those who use social care services tend to be less well-positioned to seek (or demand) evidence about the ‘treatments’ they receive than many of those who require healthcare interventions. At the same time, service providers tend to be less well-informed about the effectiveness and cost effectiveness of what they have to offer than their counterparts in health (Roberts, Shiell & Stevens 2008), while researchers may have been less responsive to practitioners’ questions, and less engaged in knowledge exchange than they might be.

The identification of problems of this kind is by no means new. In relation to the setting up of the welfare state, Richard Titmuss was among those whose work had influence, not only through his publications (Titmuss 1958; 1950; 1943; 1938) but also through dialogue with policy makers (Alcock et al. 2008). In 1991, Roy Parker and his colleagues wrote: ‘It has … become increasingly apparent that unless outcomes in childcare can be adequately measured, we have no means of justifying the actions of social workers, which may have far-reaching and permanent consequences for individuals’ (Parker 1991). More recently, while the *development* of the Sure Start programme was underpinned by research on the importance of early childhood interventions, (Glass 1999; Roberts 2000), there is a question about the extent to which evidence about efficacy adequately underpinned the menu of interventions eligible for public investment. Rutter (2006) has also pointed out that political constraints on research placed limits on what the evaluation of the initial phase of Sure Start can usefully tell us.

In the discussion that follows, we draw on the findings of two studies - one that identified questions to which practitioners in social care wanted answers and one that looked at the sort of research funded. We describe the degree of ‘fit’ between questions and answers, discuss what these findings might mean, what some of the solutions might be to the problems we identify, and the kinds of research and research infrastructure which might be needed to work towards solutions.

The UK context in which we are working is one where, over the last decade or so, there has been a growing interest in the place of research evidence in underpinning services to children and families. UK policy emphasises the centrality of *outcomes*. The Children Act 2004[1] and the accompanying guidance, Every Child Matters[2] require children’s services to be organized to improve the well-being of all children living in their area.

Bias cannot be eliminated from academic research, but it can be reduced, and potential sources of bias made transparent. In any case, in a piece such as this, where much of the content arises from reflections based on a wide range of evidence, it may help if our backgrounds are clear. Three sources of bias are institutional affiliation, intellectual or philosophical starting points and financial conflicts of interest. The last tends to be more relevant in health research where powerful industrial stakeholders may fund research and have a natural interest in promoting (or otherwise) particular findings. In our case, as in most work in social science and social care, there is no such conflict of

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[1] http://www.everychildmatters.gov.uk/strategy/guidance/
[2] http://www.everychildmatters.gov.uk/

interest. The empirical work we described here was funded through a research council, the Economic and Social Research Council (ESRC).

All of the authors are social scientists by training and for large parts of our careers. While conducting this study, and for most of the time we were preparing it for publication, MS, KL and HR were working in the Child Health Research and Policy Unit, City University London. Our current affiliations are on the title page. We three are career researchers, and to that extent, have an interest in the usual conclusion that ‘more research is needed’. Two of us, KL and HR, spent between us 12 years in Barnardo’s, a UK children’s charity, HR as Head of R&D and KL as a member of the research team. SW was one of the researchers with lead responsibility for the initiation in the 1980s of the *British Social Attitudes* reports, and worked as a quantitative researcher for 15 years. She has been at the Nuffield Foundation overseeing its work in social research for more than a decade, and is currently Deputy Director there. The Foundation aims to bring about improvements in society through research and practical experiment. This article reflects her personal views, and not those of the Foundation.

We all have experience of the appropriate use of both quantitative and qualitative methodologies and a wide range of research designs including interviews, surveys, longitudinal work of various sorts, intervention studies and systematic reviews. Most importantly, it would be fair to say that we all came to social science and social research because we believed that it could help make a better world; we are all committed to the belief that children and families have a right to services that are more likely to do more good than harm, and are based on the best available evidence.

What we did

We carried out a systematic review to identify studies which asked UK social care practitioners what research they felt would help their practice. We searched a range of databases[3] and online journals, carried out citation searches and made direct personal contacts to locate studies published between 1995 and 2004 (See Stevens, Liabo & Roberts 2007 for full details). We then identified research on child and family social welfare funded by five major UK funders between 1996 and 2004, and compared the results to look at the ways in which funded research activity maps onto practitioners' expressed needs. The funders were the Economic and Social Research Council (ESRC), the Joseph Rowntree Foundation, the Nuffield Foundation, the Gatsby Foundation[4] and the Community Fund[5] (now the Big Lottery Fund). In order to obtain data on funded studies we searched funders' websites and annual reports.

The review of practitioners' research priorities found 1005 research suggestions from five studies (see Table 1), while the mapping of funded studies resulted in information on 625 studies (see Table 2).

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[3] Medline, PsychInfo, British Nursing Index, Cinahl, Embase (all via Ovid), CareData, ChildData, Applied Social Sciences Index and Abstracts (ASSIA), Evidence Network bibliography, Regard, British Education Index, Education Resources Information Centre (ERIC)

[4] Economic and Social Research

[5] Research grants

Table 1: Sources of practitioners’ responses

Response source	Frequency	Percent
CEBSS (Sheldon 2000)	429	43
NCH (O’Leary 2000)	238	24
NI Barnardo's (Hart 2004)	186	19
NCH (O’Leary 2004)	78	8
WWfC (What works for children 2006)	74	8
Total	1005	100

Table 2: Sources of funded studies

Funder	N	%
ESRC	314	50
Joseph Rowntree Foundation	228	36.5
Nuffield Foundation	35	6
Community Fund	28	4.5
Gatsby Foundation	20	3
Total	625	100

We categorised each practitioner research suggestion, and the question addressed by each funded study, by both topic and question type. The question type categories were:

1. Employee issues where practitioners were the focus of the question
2. Interventions:
    2.1. Does it work – is this intervention effective?

    2.2. What works – what is effective for this problem?
    2.3. What is there for this target group – what interventions are available for people experiencing this problem ?
    2.4. Non-intervention – what are the consequences of not intervening?
    2.5. Users' views – what do users think of the intervention?
3. Understanding clients' problems
4. Policy
5. Other
6. Not clear

Research topics

We grouped practitioners' questions according to their main focus. For the ten topics identified most frequently, we searched the database of funded studies to see whether the questions had been addressed. Table 3 shows that nearly every question had been addressed by at least one study. However, most of these questions represent problems for which even several studies would be unlikely to provide an answer - certainly not a complete answer.

Table 3: Research areas most frequently raised by practitioners

Question asked more than 20 times	**N**	Subject covered in funded studies	Question matched in funded studies
Child protection – what works?	31	21	12
Effects on children living with substance-abusing parents	27	3	3
Engaging with hard-to-reach families/individuals	26	8	3
Should adopted children have contact with their birth family	25	2	1
Preventative family support work vs crisis intervention	24	0	0
Multi-agency working – what works?	23	5	3
Family support services – do they work?	23	18	8
Workloads and associated problems	23	0[6]	0
Behaviour problems – what works?	21	6	1

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[6] The CareData database indicates a large literature on this topic in the UK, however, our search was designed to identify studies relevant to services to children and families. Most of these questions came from practitioners working in local authority social services departments.

Question type

Table 4 shows the proportion of funded studies and practice questions coded to the different categories of question type. In summary, this shows that practitioners were more likely to ask a question about service effectiveness, and funded studies were more likely to look at the causes or the nature of social problems.

Table 4:Types of research question asked by funded studies and by practitioners

Type of question	**Funded studies (n)**	**Practitioners' questions (n)**
Effectiveness of interventions or services (‘does it work’ and ‘what works’ questions)	13% (82)	45% (456)
What interventions are there?	2.4% (15)	4% (45)
Effects of not intervening	0.2% (1)	1% (8)
Users’ views	8.5% (53)	2% (22)
Causes/understanding problems	64% (402)	16% (165)
Policy	4.2% (26)	2% (16)
Employment issues	3.7% (23)	19% (187)
Other/not clear	3.7% (23)	2% (23)
Total	100% (625)	100% (1005)

Methodological approach of the funded studies

We also gathered data on methodological approaches. When the funding organisation’s records did not describe study methods, a Google search using search terms based on available data enabled us to ascertain the research designs of all but 9% (n=54) of the included studies. As Table 5 indicates, a qualitative design was the most common (37%,n=230) including focus groups, observational methods, in-depth interviews and action research.

We were particularly interested in the methodologies used in those studies that made a claim to investigate effectiveness. Of the 82 studies that looked at the effectiveness of interventions or services (led by a ‘what works’ or a ‘does x work’ type question), 29% (n=24) used qualitative techniques. Seventeen percent (n=14) of the ‘effectiveness’ studies were reviews, of which only one was a systematic review. Ten percent (n=8) used a before and after design and 10% (n=8) mixed qualitative and quantitative methodologies. Two percent (n=2) of these studies were document analyses, 2% (n=2) were longitudinal or cohort studies, 2% (n=2%) laboratory style experiments and 1% (n=1) a survey. There were insufficient data to determine the methodologies used in 12% of the effectiveness studies (n=10) and only four randomised controlled trials were found, comprising 5% of the effectiveness studies and fewer than 1% of the overall coded studies. Given the methodologies used, it is unlikely that many of these studies would have been able to provide robust evidence about effectiveness.

Table 5: Methodology employed by funded studies

Research method	**Frequency**	**Percent**
Qualitative	230	37
Mixed methods (qualitative and quantitative component)	108	17
Longitudinal/cohort	74	12
Developmental descriptive	54	9
Literature review	31	5
Survey	25	4
Quantitative dataset analysis	16	3
Before-after	8	1
Non-randomised controlled trial	8	1
Randomised controlled trial	3	0
Systematic review	2	0
Other	12	2
No data	54	9
Total	625	100

What we found

These studies found an overall fit between *topics* of funded research and topic priorities in practice, but a disparity in the *types of research questions* being asked. While the research studies focused mainly on understanding the reasons for, and the nature of, social problems, practitioners mainly asked questions about how to solve these problems. In the light of the Every Child

Matters agenda, the outcomes focus of practitioners is to be welcomed, but research evidence for the effectiveness of interventions in social care remains disappointingly thin (Craig, Cook & Fraser 2004; Hawkins & Law 2005; Liabo 2005; Millward, Kelly & Nutbeam 2003; Roberts et al. 2004).

Discussion

Our discussion focuses on two areas. First, how might we interpret the evidence about the disjunction between what practitioners wanted and what funders were funding? And second, what possible reasons might there be for the relative paucity of studies of effectiveness?

We note that context is important. The welfare state in the UK, the provision of free health care at the point of need, and the long history of at least some universal services such as health visiting and the early establishment of the importance of, and services for, early years underpin provision. The United States, on the other hand, has a stronger record of trials in this area, but in general, worse outcomes for the most disadvantaged children. But while our policy framework and social services are relatively benign in comparison with the United States, we do not have the framework of wider social support and interventions which have enabled the Nordic countries to have the best outcomes in the world for children.

We believe it is likely, of course, that in this area, as in all areas of frontline practice, what practitioners are partly seeking is evidence about what to do in a particular case. Research evidence in general - and certainly in this area – is very rarely of this type. Research can identify possible interventions, or choices, or things that might be worth trying, or things that are unlikely to work, or the context in which any choices must be made. But even if the state of our current knowledge were far more advanced, it is unlikely that research would determine what a practitioner should do exactly in a particular case, given the nature of the problems and the range of issues they will have to consider. We lack the evidence to give even rough probabilistic guidance on what to do in many situations. As Lipsky (1980:29) has pointed out: " ... street level bureaucrats [a group which we would take to include social workers] work with a relatively high degree of uncertainty because of the complexity of the subject matter (people) and the frequency or rapidity with which decisions have to be made. Not only is reliable information costly and difficult to obtain but for street level bureaucrats high case loads, episodic encounters, and the constant press of decisions force them to act without even being able to consider whether an investment in searching for more information would be profitable."[7]

The situation in children's services is in fact such that research evidence is not yet likely to fully map the terrain of possible interventions or assess their claims to effectiveness across a range of relevant outcomes. Despite the history to which we refer in the introduction, and the impressive work done by Barnardo's, Research in Practice and others, it is only now, with a strong

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[7] We are grateful for Kenneth Macdonald's webpage which drew this to our attention http://www.nuffield.ox.ac.uk/Users/Macdonald/ASS-Ethics/Exhbt.PDF

emphasis on outcomes in children’s services that the thinness of the evidence on effectiveness is beginning to be widely acknowledged.

Work done in the 1980s, 1990s and at the start of the 21st century has demonstrated how ineffective some interventions may be in the contexts in which they have been used. In other areas we have an idea about what the characteristics of effective interventions might be, but we still have very little evidence about what actually works (or how much it works), for whom and in what contexts. Much of the effectiveness evidence we do have has come from the United States, from which source a common language has tended to obscure very different social and welfare contexts. Thus, current studies of effectiveness may well not meet the question underpinning practitioners’ desire for research on what works, because they will not tell them what may work with their own populations, in their own social structures, let alone their caseload.

A second thing that practitioners may reasonably seek from effectiveness studies is evidence relevant to planning services overall. That too is difficult given the current evidence. Those studies that we have that look at effectiveness tend to compare a new intervention with ‘normal’ practice in ways that are not very robust. In a very few cases we have evidence about the comparative costs, but almost none that would help map the comparative costs of a range of interventions against changes in outcomes in a way that might inform some optimum mix of costs and effectiveness (Stevens et al, in preparation).

This is further complicated by the fact that a number of studies (Oakley & Fullerton 1996; Sefton 2003) have indicated that practitioners, planners and policy makers, like politicians, have a tendency to seek evidence in order to argue a case rather than in order to make a decision. Related to this is a tendency for untested interventions, promoted by persuasive advocates, to engage political interest, and be rolled out before proper piloting and robust evaluation (Jowell 2003).

Why might funders fund so much descriptive work, and often rather small-scale descriptive work? Of course, sometimes, descriptive work may be needed to understand just what is happening in ‘normal services’. Much of the work in children’s services funded in the 1980s and early 1990s was necessarily of this type, as there was so little extant systematic evidence about what happened to children ‘in care’. Descriptive work can also help decision-makers understand why current service delivery has difficulties in some areas or in some types of cases or at some key points in case work, or it can inform evaluations of newly introduced statutory frameworks, to see if they are working in the way that was intended. Many of these aims would however require at least some quantitative descriptive studies, and these are themselves relatively thin on the ground.

In general, research training for social scientists in the UK has not been strong on the quantitative side, although this is changing. Combined with a professional focus of social care professionals on relationships and process (a concern from which those in other professions have something to learn), this has tended to result in a high volume of qualitative work. We suspect too that qualitative studies are also popular among many social care professionals and social care researchers precisely because they feel closer to the ‘clinical’ case than the abstraction that necessarily comes with numbers. The findings feel as if they have relevance to the day-to-day decisions social workers may have to make, and remind them of the experiences of service users. But without representative quantitative evidence, evidence that helps map the clustering of risk and protective factors, or evidence about effectiveness and differential impact of different interventions, we do not, in effect, have an ‘epidemiology’ that helps put that ‘lived texture’ into any wider perspective, either descriptively, analytically or causally.

From the point of view of funders, an underlying issue is that studies of effectiveness, especially intervention studies, tend to be more expensive than either descriptive studies of social work processes or small scale studies of user or practitioner views. On the other hand as others have pointed out (Beecham & Sinclair 2007; Research in Practice 2007) while high quality evaluative research is expensive, its costs are almost certainly dwarfed by the current cost of using resources ineffectively. The solution to these problems depends on the joint efforts of researchers, funders and service agencies.

There are further problems. While the UK has an exceptionally strong background in longitudinal studies, particularly the birth cohort studies, in general child welfare, in high quality qualitative research, and in studies identifying important problems, for instance inequalities in health and wealth, which are very pertinent to child welfare, quantitative skills, longitudinal analysis skills and particularly skills in designing trials have historically been in relatively short supply among the UK researchers working on the population of children looked after by the state, or with detailed knowledge of the statutory sector of child protection.

A further difficulty relates to practitioner ambivalence about intervention studies. For example, Hagell and Spencer (2004) found that children’s social workers were unwilling to take part in simple before-and-after tests to see if provision of evidence about research in the form of audiotapes actually improved practitioner knowledge about particular issues. Recent RCTs in children’s services have reported difficulty in recruiting to trials, even once it has been established that cases were known to exist in the areas approached (Witherspoon, personal communication). In some cases, researchers may not have worked sufficiently closely with practitioners to explain or motivate participation, but practitioners have also voiced concerns, expressing the view that new interventions would inevitably be ‘better,’ so that it would be unethical to randomise. And the funder author has certainly supported trials where practitioners wanted to allocate particularly problematic or difficult cases (for whom everything else had been tried) to the new intervention, rather than allowing a random case mix to receive the new service. Even where service managers have pledged support for trials, recent end of grant summaries have reported that trials have delivered fewer numbers than any realistic model would suggest because practitioners in the front line had any or all of these concerns about intervention trials (MTFCE National Implementation Team 2008; Rushton & Monck 2009). This is not, of course, a concern restricted to social care (Silverman 1980). We return to this issue below.

Taken together, these supply and demand factors in relation to funding may suggest that the current way of funding research on effectiveness and, particularly, testing the effectiveness of new interventions needs a more strategic approach. Waiting responsively for grant applications is unlikely to stimulate the growth of the kind of research capacity that is needed, or to yield a strategic approach towards studying effectiveness. A strategic approach might also result in more and better longer-term follow-ups (the lack of longitudinal studies in children's services of even a descriptive kind is striking), and in cumulative learning from interventions that have promising aspects even if they do not deliver all that we might have hoped.

Any strategic approach to funding is likely to depend at least partly on the development of 'strategic practitioners', who may well be vital in a number of ways. First, the record shows that it can be such practitioners who may be well-placed to conceptualise and develop interventions, or hear about them in other contexts and consider how they might work in their own field. Second, such practitioners are often a crucial and consistent bridge in what ought to be a continuing dialogue between researchers, practitioners and funders, with these practitioners explaining to others why we don't really know what works, why the new intervention may not be preferable, why difficulties in allocating cases can be overcome and so on. Certainly some of the successful centres that have generated a cumulative record of trials invest in having such ‘strategic practitioners’ as a key part of the workforce (for instance, the Office of Child Development at the School for Education in the University of Pittsburgh, or the Nuffield Foundation’s own curriculum centre, which involves many teachers). In the UK children’s care context, the majority of the senior members of the R&D team in Barnardo’s were qualified and experienced social workers, who, rather than being in head office, were located alongside practitioners and service managers.

All this suggests that longer-term funding of research programmes, rather than funding single studies, will be necessary to generate the range of evidence that practitioners say they want, that many funders will want and that the users of children’s services deserve. In the UK, given the scale and remit of this work, and its inevitable links to policy-making, much of this funding would have to come from government.

Two basic models may be worth consideration. First, the funding of research centres of excellence with substantive concentration on particular areas (like the research arm of the newly funded National Academy of Parenting Practitioners) or issues may be a way forward. As with the centres funded by the Medical Research Council, such centres would require periodic review, and advisory boards with strong representation of research experts as well as practice leaders. Staff would include researchers and strategic practitioners. But the key element of the remit would need to be a focus on studies of effective interventions that pushed the research questions as well as the available answers further in the direction of ‘what works’. In order to create a robust evidence base relevant to a substantive issue, it would be essential to draw researchers from a variety of disciplines, including psychology, economics, sociology, social statistics and social policy in addition to the traditional ‘social care’ research workforce. Combining these other research skills with those of the existing social care research force (who will know much more about the statutory processes and practice realities), will allow questions to be posed in new and different ways, and seem a vital part of improving the focus on outcomes that in turn improves child welfare.

Another model might be the creation of a specialist funding agency, like the National Institutes in the USA, part of whose remit is to create such centres of excellence, developing both researcher capacity and practically-focussed robust research. This too would require funding some sorts of centres such as those above, but could take other strategic initiatives to improve research on workforce capacity and to bring researchers from other disciplines into this substantive area of work.

Without strategic interventions of this sort, we are likely to see a continuing shortage of evidence about effective interventions and fail to meet the needs of those delivering services, those using them, and the reasonable requirement that we build in a stepwise way on the solid work done to identify problems and needs.

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