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The Effects of Budgets on Doctors Behaviour: Evidence from a Natural Experiment

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Abstract

In many health care systems primary care physicians act as 'gatekeepers' to secondary care. We investigates the impact of the UK fundholding scheme under which general practices could elect to hold a budget to meet the costs of elective surgery for their patients. We use a differences in differences methodology on a large four year panel of English general practices before and after the abolition of fundholding. Fundholding incentives reduced fundholder elective admission rates by 3.3% and accounted for 57% of the difference between fundholder and nonfundholder elective admissions, with 43% a selection effect due to unobservable differences in practice characteristics. Fundholding had no effect on emergency admissions.

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1 Introduction

The physician 'gatekeeper' is one of the most common features of managed health care. The gatekeeper has a dual role, simultaneously acting as an expert clinical agent on behalf of the patient, and a rationing agent on behalf of the payer for care (employer, government, or other insurer). Gatekeepers are found in almost all health care systems, examples being various forms of US managed care and the UK system of general practitioners.

The incentives offered to gatekeepers seek to balance their two roles. Without countervailing pressures gatekeepers might make heavy use of health services for their patients in order to demonstrate professional excellence and attract and retain patients. The negative consequences of any 'overuse' are likely to be small compared to the costs to the physician of 'underuse' amongst her patients. Funders of health care have therefore experimented with incentives for gatekeepers to reduce demands by their patients. The most ubiquitous is capitation, under which the gatekeeper receives a fixed – possibly risk-adjusted –sum for each enrolled patient. The gatekeeper must then bear some or all of the risk associated with variations in expenditure from the budget formed by the sum of her capitation payments. The sanctions for overspending might range from a modest threat of a utilization review, through to earnings being 100% at risk for any amount overspent.

Effective policy requires information on the responses of gatekeepers to the different types of budgetary regimes. In this paper we present evidence from a recent large natural experiment in the English National Health Service on the effects of confronting gatekeepers with an explicit budget and prices for care received by their patients.

The UK National Health Service (NHS) has a long tradition of general practitioner (GP) gatekeeping. All NHS patients must be registered with a GP, and - other than in an emergency – no NHS patient can gain access to secondary care without a referral from a GP. A patient's GP also acts as the main gateway for securing access to pharmaceuticals. There has been a tradition in the UK of low rates of referral to secondary care and low pharmaceutical usage relative to other developed nations.

In 1991 the UK government introduced a split between purchasers and providers of health care in the NHS (UK Government, 1989). Health Authorities (HAs), geographically defined entities covering on average initially about 300,000 citizens, became the main purchasers of health care. The providers of secondary care (principally hospitals) were termed NHS Trusts and were removed from the direct control of HAs. They remained within the public sector but were required to compete for contracts from purchasers in what was known as the NHS internal market.

As part of the1991 reforms larger general practices could elect to become fundholders (Glennerster *et al*, 1994). Fundholding practices became responsible for purchasing some elective (non-emergency) procedures from local providers. They were given an annual budget by their local Health Authority to do so. The HA thus delegated part of its budget and purchasing responsibility to those of its general practices that chose to become fundholders.

Fundholders could retain any budget surplus for spending on services to patients. Budget deficits were often made good by HAs, and there were limits on the maximum cost per episode, so that the financial regime was not unduly demanding for fundholding practices. Although a fundholder surplus could not be used for direct payment to the practice partners, it could be used to pay for enhancements to the practice's premises, which might yield capital gains that could be realized when the premises were sold. By 1997 over 50% of the population was registered with a fundholding general practice, and their expenditure accounted for 15% of NHS expenditure on secondary care (Audit Commission, 1996).

A change of national government in 1997 led to a further major reform of the NHS (UK Government, 1997). Part of the rationale were criticisms of the fragmentation, inequalities in treatment and high managerial costs allegedly brought about by devolved purchasing in the form of fundholding. No new fundholders were allowed from April 1998 and fundholding was abolished in April 1999. New organisations, Primary Care Trusts (PCTs), in which primary care professionals (particularly GPs) were intended to play a central managerial role, became the main purchasers of health care.[1] All practices had to join their local Primary Care Trusts. PCTs are formally responsible for health care in a geographically defined area, though their populations (around 150,000) are the populations of their constituent practices (typically numbering around 20 to 25).

The introduction of the internal market was never subjected to rigorous evaluation. Indeed the UK government at the time consciously rejected the need for co-ordinated evaluation, and claimed that calling on the advice of academics for such purposes was a sign of weakness (Robinson and Le Grand, 1994). A review of the evidence to 1998 by Le Grand, Mays and Mulligan (1998) suggests

1. Compared with non-fundholding practices, GP fundholders appear to have secured a relatively slower growth in pharmaceutical costs, at least in the early years of fundholding (Baines *et al*, 1997; Gosden and Torgeson, 1997).
2. GP fundholders received generous budgets relative to GPs covered by health authority contracts, and fundholders experienced few serious sanctions for overspending (Dixon *et al*, 1994; Audit Commission, 1996).
3. Geographical proximity, perceived quality and long term relationships seem to have been the principal criteria for GP choice of provider, rather than prices charged (Mahon *et al*, 1994; Baines and Whynes, 1996; Propper *et al*, 1998).
4. Patients of GP fundholders may have secured favourable waiting times in comparison with patients of non-fundholders (Dowling, 2000; Goodwin, 1998).
5. The evidence on the effect of fundholding on practice admission rates was inconclusive.

Methodological problems have bedevilled attempts to evaluate the behavioural impact of the introduction of fundholding. Because of government opposition to evaluation the studies that were done tended to be small scale, to lack adequate controls and to be difficult to generalise. The major problem was selection bias: because fundholding was voluntary it was difficult to disentangle the effect of the financial incentives of

[1] Initially in April 1999 Primary Care Groups were introduced as subcommittees of their local HA but it was intended that they would progress to become separate legal entities as PCTs. By April 2002 all PCGs had become PCTs and HAs were abolished.

fundholding from the unobservable characteristics of the practices that could influence both their referral rates and their decision to become fundholders. There is evidence that fundholders, especially early wave fundholders, were different from non-fundholding practices (Baines and Whynes, 1996). Some of these differences, such as the mean age of the GPs in the practice, are observable in administrative data sets but others, such as the strength of entrepreneurial attitudes are not (Whynes et al, 1999).

Two studies of the effect of fundholding on admission rates have attempted to deal with selection bias. Gravelle et al (2002) examined practice admission rates for cataract surgery in a large HA with 109 practices and allowed for endogenous fundholding status in two ways. They used an ordered logit model to estimate the probability that a practice would become a fundholder of a given wave and then used the predicted probabilities in the admission rate equation. They also estimated a selection equation for fundholding status and added the resulting Mills ratio to the admission rate equation. Both methods yielded similar results to OLS models: fundholding practices had significantly lower admission rates. Attempts to compare the change in behaviour of practices becoming fundholders with those which remained non-fundholders were not successful because they had only three years of data and only a small number of practices became fundholders during the period.

Croxson et al (2001) used a four year panel of data on 58 practices in a different HA. Their prime aim was to examine whether fundholders had gamed the budgetary system. The budget for a fundholding practice was set by applying standard prices to the practice's elective admissions in the period immediately before it became a fundholder. This created an incentive for fundholding practices to increase their admission rates in the year before they become fundholders. Croxson et al (2001) compared the change in admission rates for fundholders on becoming fundholders against the change in admission rates of non-fundholders and established fundholders, thereby reducing the likelihood of selection bias. They found that there was indeed evidence that fundholders increased their admission rates in the year prior to becoming fundholders and reduced them in their first year of fundholding. The study clearly demonstrates that fundholding practices responded to the financial incentives in the budget setting process. However, it did not address the question of whether the fundholding financial regime caused changes in the behaviour of practices after the preparatory and first year of fundholding.

We use the opportunity offered by the abolition of the voluntary fundholding regime and its replacement by the compulsory PCT regime to examine the effect of budgetary regimes on the behaviour of fundholding practices. We have a newly constructed data set including information on admissions for over 7000 practices (fundholding and non-fundholding) for the two years before (1997/8, 1998/9) and the two years after (1999/2000, 2000/1) fundholding was abolished. In December 1997 practices were given a clear signal that fundholding was to be ended in April 1999, and – unlike the situation when fundholding was introduced – GPs had a good understanding of the implications of the policy change. The fact that the 50% of practices which were fundholders had to switch from the fundholding regime to the PCT regime means that the difference in differences methodology has a greater chance of identifying the effect of the change in budgetary regimes compared with studies based only on data from the fundholding period when the numbers switching financial regimes in any year was much smaller. Furthermore, by 1997 data sources had improved[2] so that it is possible to examine the admission rates of fundholder and non-fundholder practices on a national basis, rather than within single HAs.

We set out a simple model of practice decisions on admission rates under the fundholding, non-fundholding and PCT budgetary regimes in section 2. We use the model to generate a set of hypotheses and to discuss the assumptions which enable the difference in differences methodology to identify the effects of the budgetary regimes on fundholding practices. Section 3 describes the data and discusses the methods. Results are presented in section 4 and section 5 concludes.

[2] Prior to 1997/8 Hospital Episode Statistics, which contains details of all patient admissions in England, did not have a field for the practice with which the patient was registered. Such information was held locally at HA level.

2 GPs and budgets

2.1 A model of budgetary regimes

We assume that GPs care about the number of patients a from their practice admitted for elective surgery and also about the income y their practice receives from the NHS. Income can be used for GPs' personal consumption or to provide additional patient services that are not directly funded by the NHS. We do not need to consider the division of practice income between these uses.

The practice utility function is quasi-linear

$$u(y,a,q,z) = y + h(a,q,z) - qk, \quad h_{aa} < 0, h_q > 0, h_{aq} > 0 \tag{1}$$

z is a temporal shift variable. k is a cost associated with the budgetary regime. It includes transaction costs and any distaste felt by GPs under the fundholding regime about having to make explicit tradeoffs between income and elective admissions. Transaction cost under regime j is k_j. $j = F$ indicates that the practice is a fundholder in 1997/8 and 1998/9, $j = NF$ that it is not a fundholder in those years, and $j = T$ that it is operating under the PCT regime (all practices in 1999/2000, 2000/1). We assume that fundholding has higher transaction costs: $k_F > 0 = k_{NF} = k_T$.

Practices have heterogeneous preferences with respect to the costs of the budgetary regime and patient benefits. We capture heterogeneity by a single parameter q in such a way that practices with higher q have higher admission rates under a given budgetary regime but have a higher lump sum transaction cost under fundholding. Practices with higher q are therefore more likely not to be fundholders in 1997/8 and 1998/9 and to have higher admission rates conditional on the budgetary regime. Hence part of the difference between fundholder and non-fundholder admission rates is due to selection.

We can derive the model from the more epidemiologically orientated notion of the practice referral threshold. GPs refer patients for outpatient appointments with a hospital specialist. Referred patients are seen by the hospital specialist after a delay of some weeks and the specialist then decides whether to admit the patient. If she does so the patient is placed on a waiting list for elective surgery before being admitted.

(See Lindsay and Feigenbaum, 1984; Gravelle et al (2002) for fuller models of the process.)

In this paper the distinction between referrals and admissions is ignored and it assumed that all referred patients are admitted. We normalise the exogenous number of patients who consult to 1. A cost of c is incurred when a patient attends an outpatient clinic at the hospital to be placed on the waiting list for elective surgery. Treatment is received after a waiting time of w and produces a gross benefit of $b+q+z$, where b is the patient specific component of the benefit which has distribution function $G(b)$ (density $g(b)$) over the population of consulting patients. If the practice refers all patients whose specific benefit exceeds the referral threshold b^o the number referred a and the referral threshold are negatively monotonically related, with $a=1-G(b^o)$ and $b^o=b^o(a)$, $\partial b^o/\partial a=-1/g(b^o)$.

With a wait of w, the total benefit from patients who are referred is

$$h=\int_{b^o(a)}^{\infty}\left[(b+q+z)\exp(-rw)-c\right]dG \;=h(a,q,z) \tag{2}$$

with

$$h_a=[b^o(a)+q+z]\exp(-rw)-c \tag{3}$$

GP marginal utility from admissions is positive at small a but eventually declines to zero and becomes negative as the health gain from the marginal referred patient declines. Hence the practice indifference curves are U-shaped in (a,y) space, as in Figure 1.

Non-fundholding practices faced an implicit budget. The budget for all elective admissions for non-fundholding practices was held by their Health Authority. The HA could buy admissions for its non-fundholders under a "cost and volume" which stipulated a price p for marginal variations in admissions around the contracted for planned volume for the HA. Since the HA had a fixed budget, additional expenditure on elective admissions meant less expenditure on other activities. The opportunity costs of additional admission of a patient from a practice would be spread over all practices and patients in the HA. We capture the attenuation of incentives by assuming that marginal savings or additional expenditure on elective admissions for non-fundholders were shared equally by all n_{HA} practices (fundholders and non-fundholders) in the HA. There were about n_{HA} = 100 practices in each HA in 1997/8 and 1998/9, so the effective price confronting non-fundholders $p_{NF} = p / n_{HA}$ was very low. If a practice had no patients admitted its full income was y_{NF}. Practice income under the implicit budget constraint for non-fundholders was $y_{NF} - p_{NF} a$.

Under the PCT regime after the abolition of fundholding, PCTs made contracts with providers for all elective admission from their practices.[3] The contracts were similar to those made by HAs for elective admissions from non-fundholding practices. With n_T practices in the PCT, and assuming the difference between planned and actual expenditure on electives is shared equally across practices in the PCT, the implicit price for elective admissions is $p_T = p/n_T$. In our data set a typical PCG/T had n_T = 21 practices in 1999/00. The practice's full income with no admission under the PCT is $y_T = y_{NF}$.

The fundholding regime gave the fundholder an additional explicit budget B_F to purchase elective admissions at the explicit fundholder price p_F. Fundholding practice income was $y = y_F - p_F a = y_{NF} + B_F - p_F a$ if $B_F \geq p_F a$. Fundholders who overspent their budget did not have to pay for the additional admissions so that the marginal price for such admissions was the very much lower implicit price p_{NF} facing non-fundholders. The fundholder budget line was thus kinked at the point where $p_F a = B_F$.[4]

The practice's optimal choice of admissions under budgetary regime $j = F, NF, T$ is $a^*(p_j, q, z)$, giving the practice an optimised utility $u^*(p_j, y_j, q, z)$. With the benefit function (2), admissions are decreasing in the price p_j and increasing in the taste and temporal parameters q and z.

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[3] Although it was the intention that PCTs would introduce 'notional' budgets for hospital care at practice level linked to financial incentives (Department of Health, 1997), very few PCTs introduced such practice budgets in the first three years of the PCT regime (Dusheiko et al, 2002).

[4] Fundholders also did not have to pay for elective cases which cost more than £6000 but we ignore this complication in the modelling since it affected a very small proportion of admissions.

The explicit budget for fundholders was set so that it was sufficient to buy the number of admissions for the fundholder in the last period before they became a fundholder (Dowling, 2000; Croxson et al, 2001). Thus $B_F = pa^*(p_{NF}, y_{NF}, q, z)$, where we assume that the price facing fundholders is the same as the price paid by the HA and the PCT for marginal elective admissions.

Figure 1 illustrates the optimal choices a_{NF}, a_F, a_T under the non-fundholding, fundholding and PCT regimes. The temporal shift factor z is held constant in the figure. The additional budget for fundholding gives the practice, which chose a_{NF} as a non-fundholder, a budget line through a_{NF} with slope $-p$ and intercept y_F on the income axis. Because the practice utility function is linear in income the effect of the transaction cost of fundholding can be shown as an inward parallel shift of the budget line so that its intercept is $y_F - q k_F$. The practice's optimal choice under the fundholding regime is a_F which is less than under the non-fundholding regime because it faces a higher price and there are no income effects.

Elective admissions from practices under the PCT regime will be less than under the non-fundholding regime because PCTs have fewer practices than HAs, so that the financial gains from a reduction in admissions are shared across fewer practices and the implicit price is higher: $p_T > p_{NT}$. Hence the abolition of fundholding will reduce the admission rate from practices which were not fundholders, unless the effect on implicit prices is regarded as negligible by practices. With no income effects the abolition of fundholding will increase the demand from ex-fundholding practices.

There are two reasons why the argument that practices under the fundholding regime faced higher prices and hence would have fewer admissions than under the non-fundholding regime may be incomplete. The first is that, either because of gaming by fundholders (Croxson et al, 2001) or because the budget setting process was rough and ready, practice fundholding budgets could be more than sufficient to purchase their previous level of elective admissions. It has also been suggested, though the evidence is mixed (Goodwin, 1998; Dowling, 2000), that fundholders received overly generous budgets, especially in the early years of the scheme in order to encourage more practices to become fundholders. With quasi-linear preferences overly generous budgets do not alter the conclusion that the fundholding regime will have fewer admissions. But if admissions are a sufficiently strongly normal good, admissions may be higher. Given the very large difference between the explicit fundholder price and the implicit prices under the other regimes it seems implausible that the income effects of inaccurate budgets would offset the substitution effects.[5]

The second reason why fundholding could lead to more admissions is that they could purchase care of higher quality than non-fundholders. In particular it has been suggested that the patients of fundholding practices had lower waiting times for elective care (Propper et al, 2002). From (3) we have $h_{aw} = -r[b^o + q + z]\exp(-rw)$ < 0 and so lower waiting times lead to higher admissions. The difference in waiting times for patients of fundholders in our data is about 5%. The literature on waiting times suggests that the elasticity of admissions with respect to waiting times is of the order of -0.3 to -0.2) (Gravelle et al, 2002; Gravelle et al, 2003; Martin and Smith, 1999). It is seems unlikely therefore that the reduction in waiting times could reverse the price effect for fundholders. We include practice waiting times in our estimated admission models so that the estimated effect of the fundholding regime allows for any difference in waiting times and therefore reflects the incentive effects of the explicit prices faced by fundholders.

2.2 Measuring the effect of budgetary regimes with observational data

2.2.1 Identification

Although it seems plausible that the fundholding regime led fundholders to reduce admission rates compared with the non-fundholding and PCT regimes, establishing the magnitude of the effect from observational data requires some care because of potential selection bias and transitional effects. Suppose that there are two GP preference types with $\underline{q} < \bar{q}$. Those practices with $\underline{q}$ have optimal choices a_{NF}, a_F,

[5] Fundholders also received an additional expense allowance to cover some of the administrative costs of fundholding, such as the hiring of practice managers. Such a reduction in the transaction costs of fundholding will increase the likelihood that a practice will become a fundholder.

and a_T under the non-fundholding, fundholding and PCT regimes and choose to become fundholders in 1997/8 and 1998/9: $u^*(p_F, y_F, \underline{q}, z) > u^*(p_{NF}, y_{NF}, \underline{q}, z)$. In Figure 1 they choose a_F in 1997/8 and 1998/9 as fundholders and a_T in 1999/2000 and 2000/1 under the PCT regime. We do not observe them choosing a_{NF}.

Practices with $\bar{q}$ have indifference curves (not shown) which are steeper negatively than type $\underline{q}$ practices and choose higher admission rates a'_{NF}, a'_T under the non-fundholding and PCT regimes. Under the fundholding regime they are offered a nominal budget line through a'_{NF}. They would have transaction costs of $\bar{q}k_F$ under fundholding and would choose a'_F on their real fundholding budget line. Type $\bar{q}$ did not choose to become fundholders: $u^*(p_F, y_F, \bar{q}, z) < u^*(p_{NF}, y_{NF}, \bar{q}, z)$. Hence we do not observe a'_F.

Selection bias means that a cross sectional comparison of fundholders and non-fundholders is flawed because it confounds differences in incentives and in tastes. The observed difference between the admission rates of fundholders and non-fundholders $(a_F - a'_{NF})$ overstates the unobserved effect of fundholding on the behaviour of practices which chose to become fundholders: $(a_F - a_{NF})$.

A temporal comparison of the admission rates of fundholders before and after the abolition of fundholding can potentially identify the effect on fundholding practices of changing the budgetary regime from fundholding to PCTs. Thus in Figure 1, where we assumed that z was constant over time, we could examine $a_T - a_F$. But in general a before and after comparison of fundholders may be confounded by changes in other factors affecting admission rates. Examples of such factors include government policies to increase the supply of elective surgery to reduce waiting times (Department of Health, 1998), and initiatives like NHS Direct (George, 2002) and Walk In Centres (Salisbury et al, 2002) to improve access to care. The before and after temporal comparison of fundholding practices $a^*(p_T, \underline{q}, z_A) - a^*(p_F, \underline{q}, z_B)$ confounds the effect of the budgetary regime on the fundholding practices with the effects of the change in z before and after the budgetary regime change.

To remove the bias from selection and from temporal factors we compare the change in the admission rates of fundholders and non-fundholders before and after the abolition of fundholding:

$$\Delta = \left[a^{*}(p_T, \underline{q}, z_A) - a^{*}(p_F, \underline{q}, z_B) \right] - \left[a^{*}(p_T, \bar{q}, z_A) - a^{*}(p_{NF}, \bar{q}, z_B) \right] \qquad (4)$$

If, as in our model in section 2.1, the temporal shift parameter has the same additive effect on both types of practice

$$a^{*}(p_j, q, z) = a^{**}(p_j, q) + z \qquad (5)$$

then

$$\Delta = \left[a^{**}(p_T, \underline{q}) - a^{**}(p_F, \underline{q}) \right] - \left[a^{**}(p_T, \bar{q}) - a^{**}(p_{NF}, \bar{q}) \right] \qquad (6)$$

Difference in differences is usually employed to identify the effect of treatment on the treated when there are only two regimes (treated, not treated) (Blundell and Costa Dias, 2000). Here there are three regimes since both the fundholders and non-fundholders have regime changes after the abolition of fundholding. Thus the standard assumption of additive and identical temporal effects is insufficient to identify the effect on fundholders of fundholding compared either with the PCT regime or with the non-fundholding regime.

If we also assume that the difference between the non-fundholder and PCT budgetary regimes has a negligible effect on the type $\bar{q}$ who chose not to become fundholders:

$$a^{**}(p_{NF}, \bar{q}) = a^{**}(p_T, \bar{q}) . \qquad (7)$$

then

$$\Delta = \left[a^{**}(p_T, \underline{q}) - a^{**}(p_F, \underline{q}) \right] \qquad (8)$$

identifies the effect of the change from fundholding to PCT regimes on type $\underline{q}$ who chose to become fundholders.

Alternatively, we can assume, as in our model, that the difference between the non-fundholder and PCT budgetary regimes for type $\underline{q}$ who become fundholders is the same as the difference between those regimes for type $\bar{q}$ who do not become fundholders:

$$a^{**}(p_{NF}, \underline{q}) - a^{**}(p_T, \underline{q}) = a^{**}(p_{NF}, \bar{q}) - a^{**}(p_T, \bar{q}) \qquad (9)$$

If (5) and (9) hold then difference in differences identifies the effect on fundholders of fundholding compared to non-fundholding:

$$\Delta = a^{**}(p_{NF}, \underline{q}) - a^{**}(p_F, \underline{q}) \tag{10}$$

The implicit cost borne by a practice when it has an additional patient admitted under the PCT or non-fundholding regimes is greater the smaller the number of practices in the PCT or the HA. Although we do not observe $a^{**}(p_{NF}, \underline{q})$, we examine the effect of the number of practices in the PCT on the admission rates of ex-fundholders and ex-nonfundholders to test whether (7) or (9) hold. If the number of practices has no effect on the admission rates of ex-nonfundholders then (7) holds. If nonfundholders are price responsive and have a lower admission rate when the implicit price increases then the difference in differences will overestimate the effect on fundholders of the effect of fundholding relative to the PCT regime.[6]

If the number of practices in the PCT has the same effect on the admission rates of both types of practice then (9) holds. If ex-fundholders are more price responsive than ex-nonfundholders, then the difference in differences will be an underestimate of the effect on fundholders of the fundholding regime relative to the nonfundholding regime.[7]

2.2.2 Transitional effects

We estimate a linear difference in differences model of the admission rate of practice *i* in period *s* as

$$a_{is} = a_i + \mathbf{ß'x}_{is} + \sum_{s=2}^{4} w_s D_s + d F_i + \sum_{s=2}^{4} g_s D_s F_i + e_{is} \tag{11}$$

where F_i = 1 if practice *i* is a fundholder (zero otherwise) , D_t = 1 if $s = t$ (zero otherwise), and **x** is a vector of covariates (from waiting times to practice population characteristics). We have four years of data: years 1 and 2 are the pre-PCT period (1997/8, 1998/9) and years 3 and 4 are the PCT period (1999/2000, 2000/1). Data and

[6] Letting $a^{**}(p_{NF}, \bar{q}) - a^{**}(p_T, \bar{q}) = f_1$ we have $\Delta = a^{**}(p_T, \underline{q}) - a^{**}(p_F, \underline{q}) + f_1$

[7] $a^{**}(p_{NF}, \underline{q}) - a^{**}(p_T, \underline{q}) = a^{**}(p_{NF}, \bar{q}) - a^{**}(p_T, \bar{q}) + f_2$ implies $\Delta = a^{**}(p_{NF}, \underline{q}) - a^{**}(p_F, \underline{q}) - f_2$

estimation methods are described in section 3. Here we use (11) to discuss the testing of the implications of the theoretical model with our panel of observational data.

We expect that estimated $d < 0$ but cannot interpret it as the effect of the difference between fundholding and non-fundholding regimes because of the unobservable taste variables incorporated in e_{is} which affect referral decisions and the decision to become a fundholder. The estimated difference in difference parameters g_2, g_3, g_4 are not affected by selection bias and do provide information about of the effect of change from the fundholding to PCT regimes on practices which were fundholders. With instantaneous adjustment to regime changes the model predicts

$$d < 0; < g_2 = 0 < g_3 = g_4 \tag{12}$$

However, there are likely to be transitional effects which alter the predictions about the difference in difference parameters g_2, g_3, g_4.

Uncertain waiting times. The relevant price for admissions is the price ruling at the date of admission, not the date of referral. The mean wait for elective admission in our data set is 110 days. Hence some of the admissions in one year are the results of decisions in the previous year which were based on beliefs about the budgetary regime which would prevail when the referred patient was admitted. If patient waiting times were certain and practices could forecast the future budgetary regime, patient admissions in a year would be determined by the budgetary regime in that year. But patient waiting times are highly variable within practices. The probability that a referral in one year would be admitted in the following year increases through the year. The expected price $E\tilde{p}$ for fundholders in the last year of fundholding was less than p but more than $p_T = p/n_T$ and declined throughout the year. Fundholders would increase their referral rates in 1998/9, the last year of fundholding, especially towards the end of the year. Some of the referrals in 1997/8 would be admitted in 1998/9, so that the admission rate in the last year of fundholding would be greater than in the penultimate year 1997/8.

The admission rate in the first post fundholding year 1999/2000 reflects both decisions taken in the last year of fundholding when fundholders were faced with expected prices $E\tilde{p} \in (p/n_T, p)$, and decisions taken in the first post fundholding year when faced with p/n_T. The admission rate for ex-fundholders in the first post-fundholding year would be greater than in the last year of fundholding but less than in the second post fundholding year. We expect

$$d < 0 < g_2 < g_3 < g_4 \qquad (13)$$

A comparison of the last year of fundholding and the first year of PCTs would tend to understate the effect of the regime change on fundholders. The full effect of the regime change on fundholders requires a comparison of the penultimate year of fundholding and the second year of the PCT regime and is given by g_4

Deferred referrals. The anticipated end of fundholding gave fundholders an incentive to delay referrals so that the patient would be admitted in the post fundholding period at the much lower post-fundholding price.[8] Referrals in the last fundholding year would be reduced and those in the first post fundholding year would be increased. Admissions in the last fundholding year would therefore fall relative to the previous fundholding year. The deferred patients would increase admissions in the first post-fundholding year. Hence, ignoring the waiting time effect discussed above

$$d < 0, g_2 < 0 < g_4 < g_3 \qquad (14)$$

Fundholder balances. After the end of fundholding in April 1999 ex-fundholding practices were allowed to keep their accumulated fundholding surpluses to be spent over the following four years. Thus even in the last two years of fundholding fundholders still had an incentive to reduce elective admissions in order to accumulate surpluses. The anticipated ending of the fundholding regime increased the marginal opportunity cost of admissions for fundholders in the last year of fundholding. Hence fundholding practices would reduce admissions in the last year of fundholding compared with previous years. If this was the only transitional effect

$$d < 0, g_2 < 0 < g_3 = g_4 \qquad (15)$$

[8] A suggestion by Barry McCormick.

Early wave fundholders were funded more generously and would have had more time to accumulate balances than late wave fundholders. Hence the ending of fundholding is likely to lead to a bigger reduction in admissions from 1997/8 to 1998/9 by late wave fundholders compared with early wave fundholders.

If all three transitional factors operated the prediction is that

$$d < 0, g_2 < 0 \text{ or } > 0, g_3 > 0, g_4 > 0 \qquad (16)$$

Comparisons of the last year of fundholding and the first post fundholding year will not identify the equilibrium effect of fundholding on fundholder admission rates because of the transitional incentives created by the anticipated end of fundholding. To achieve this we need to focus on the comparison of 1997/8 with 2000/01. Pairwise comparisons between 1997/8, 1998/9 and 1999/00 are, however, not without interest in that they may show whether and how fundholders responded to the transitional incentive effects.

3 Data and Methods

3.1 Data

Data were collated from three main sources: Hospital Episodes Statistics for admissions, General Medical Statistics for practice characteristics and the database assembled for the AREA project (Sutton et al, 2002) for socio-economic characteristics and provider characteristics. Detail are in the Appendix. Table 1 gives summary statistics for the set of variables included in the final models. Table 2 shows the distribution of practices by fundholding wave and their admission rates for the four years 1997/8 to 2000/1.

3.2 Dependent variable

The dependent variable was the crude practice admission rate: total practice admissions divided by practice population. Demographic effects were allowed for by including the age and sex proportions of the practice population as explanatory variables. The procedure is more flexible than direct or indirect standardisation and does not require recomputation of the dependent variable when the observation set changes. We included the total practice population as a regressor in an attempt to allow for measurement errors in the denominator of the dependent variable.

3.3 Variable selection

By including covariates we increase the precision of the estimates of the effect of fundholding since the decision to become a fundholder was correlated with observable characteristics of the practice as well as unobservables. Many of the potential covariates are highly correlated and estimates of the fundholding effects were insensitive to the choice of covariates. The selection of covariates was made taking into account an initial factor analysis of the explanatory variables, variance inflation factors (Fox, 1997), and results from the AREA project cross section analysis of the determinants of 2000/1 ward level admission rates (Sutton et al, 2002).

3.4 Estimation procedures

We used STATA Version 7 to estimate a variety of panel data models: pooled OLS, fixed effects, random effects and population averaged. The pooled OLS, population-averaged and fixed effects estimators were all estimated with robust standard errors. The pooled OLS estimates and the fixed effects estimates allow for within-group (GP practice) correlation of the errors over time using the 'cluster' command. The estimated population averaged and the random effects models assume that the within group correlation in the error term is a scalar that is identical across groups and constant over time. The population averaged (PA) estimator is a general linear model for panel data (Liang and Zeger, 1986) and is asymptotically equivalent to the random effects estimator (Stata, 2001). It produces coefficient estimates very close to those from random effects models. The PA estimator yields robust standard errors which do not rely on the assumption of homoskedasticity in the RE estimator.

The OLS models include HA effects as a means of allowing for unobserved HA level effects which could arise either from the possibility that supply variables may be endogenous (Gravelle et al, 2003b) or because of HA level variations in the quality of population and admission data. We also separately include provider effects (captured by a dummy variable for a practice's main provider) to allow for unobserved provider effects arising from provider admission policies or from provider level data entry quality variations.

The fixed effects model does not yield estimates of the effects of the many time invariant explanatory variables. We therefore also estimate a within and between population averaged model that uses the within practice four year mean and the yearly deviations from the within practice four year mean for the time varying variables (waiting time, practice population and list size per GP) instead of their yearly observations. The procedure produces coefficients on the time varying variables which are identical to those from the fixed effects estimator and estimates of the time invariant coefficients which are consistent without the need for the assumption underlying the random effects estimator that unobserved heterogeneity across groups is uncorrelated with the explanatory variables.

Initial functional form tests using Box-Cox transformations of the dependent variable and the PE test of the linear versus log linear models (Greene, 2000) led us to choose a linear model. We report results for the pooled OLS, fixed effects and population averaged procedures since the random effects and population averaged models gave very similar results and the random effects models failed Hausman tests, suggesting that explanatory variables were correlated with unobservable practice effects.

3.5 Testing the identifying assumptions

3.5.1 Differential time trends: elective and non-elective admission rates

The difference in differences method rests on the assumption that unobserved temporal factors have the same effects on fundholding and non-fundholding practices. We attempted to test for differential trends by comparing difference in differences models for elective and non-elective (emergency) admissions. Non-elective admissions for fundholding procedures were not charged to fundholders. Comparing the differences in differences for emergency admissions against those for electives gives a version of the "difference in difference in differences" procedure (Blundell and Costa Dias, 2000). If some of the temporal factors affected fundholders and non-fundholders differentially but had the same effect on fundholder elective and non-elective admissions then the difference in difference in differences will identify the incentive effect of the fundholding regime on the fundholders. We therefore estimated a model of non-elective admissions and compared the difference in differences coefficients with those from the elective model. More formally, we also estimated a seemingly unrelated regressions (SUR) model of elective and emergency admissions and tested the restrictions that the difference in difference coefficients were the same in both equations.

3.5.2 Responses to implicit prices

We argued in section 2.2 that the difference in differences coefficients would identify the effect on fundholders of the fundholding regime relative to the nonfundholding regime if the difference in the implicit price of elective admissions between the nonfundholding and PCT regimes had the same effect on the admission rates of fundholders and nonfundholders. Alternatively, and less plausibly, if nonfundholders are not affected by the difference in the implicit prices between the nonfundholder and PCT regimes, then the difference in difference methodology identifies the effect on fundholders of the fundholding regime relative to the PCT regime. We also suggested that the implicit price would be inversely proportional to the number of practices in the primary care organisation (HA in 1997/8 and 1998/9, PCT in 1999/2000 and 2000/1) since the costs of additional admissions would be spread over all practices under the nonfundholding and PCT regimes. Accordingly, we attempt to test if the identifying assumptions hold by including the reciprocal of the number of practices and its interaction with a fundholding dummy in cross section OLS models of the admission rates in the penultimate year of fundholding and the second year of the PCT regime. These years are unlikely to be affected by transitional effects and by estimating single cross sections for each year we are able to allow for clustering by including HA effects in the model for 2000/1 and PCT effects in the model for 1997/8. Including HA effects in the model for 1997/8 which also includes the reciprocal of the number of practices in the HA will lead to perfect multicollinearity and similarly for the inclusion of PCT effects in the model for 2000/1.

3.6 Robustness to data quality

3.6.1 Observation selection

After linking the data we had an initial unbalanced four year panel of 30422 observations from 7606 practices. Figure 2 is a scatter plot of elective admissions against practice populations using all four years of observations. It is clear that there are some peculiar observations arising from problems with the HES data on admissions and the patient registration system count of practice populations. We cumulatively dropped (a) 101 practices with less than 1000 patients in any year (the median practice population is over 5900; (b) 740 practices with less than 30 elective surgical admissions in any year; (c) 14 practices with crude elective admission rates over 200 per 1000 in any year (the median admission rate was about 60 per 1000). This step greatly reduced both the skewness of the distribution of admission rates (from 2.91 to 0.41) and the kurtosis (from 48.78 to 4.40); (d) we wanted to include the Low Income Scheme Index (LISI) score as an explanatory variable as it is one of the few socio-economic variables measured at practice level without attribution. Although the LISI is only measured for practices with populations of 1000, not all such practices had a LISI score. Restricting the sample to practices with a LISI score led to a further 46 practices being dropped and yielded the balanced panel of 28420 observations on 7105 practices used for most of the estimations.

3.6.2 Admission data

Although there are known to be inaccuracies in the HES data (Audit Commission, 2002; Department of Health 2003c) the main issue for our investigation is whether they are systematically related to the budgetary regime. Thus suppose that providers make random errors in attributing admissions to practices. Fundholding practices which checked their recorded admissions had an incentive to query overcounts but not undercounts. Under the non-fundholding regime practices had no incentive to check their recorded admissions and if they had done so they had no more incentive to query overcounts than undercounts. Hence in 19987/9 and 1998/9 fundholders may have had their admission rates undercounted relative to non-fundholders. Under the PCT regime after fundholding was abolished ex-fundholders have no more incentive to check recorded admissions than ex-nonfundholders and hence data error will not lead to an undercount for ex-fundholders relative to ex-nonfundholders. It is therefore possible that some of the differences, and some of the difference in differences, between fundholder and non-fundholder admission rates is the result of biased measurement rather than the effect of the budgetary regimes on admissions.

The incentive for bias in the reporting of admissions ceased immediately when fundholding was abolished. Thus we would expect to see an immediate increase in the recorded admission rates of ex-fundholders in the first post fundholding year 1999/2000. One test for whether fundholding had an effect on admissions rather than on recorded admissions is to compare the difference in difference parameter between 2000/1 and 1997/8 (g_3) with that between 1999/2000 and 1997/8 (g_4). We argued in section 2.2.1 that effects of the abolition of fundholding on admission rates were likely to be only fully observed by the second post-fundholding year 2000/1. Hence if $0 < g_3 < g_4$ at least part of the effect of fundholding on recorded admission is due to its effect on admissions rather than its effect on recording.

We also attempted to allow for biased measurement more directly. The quality of provider HES data inputting varies across providers (Department of Health, 2000) suggesting that any measurement bias will also vary across providers. First, we included a set of provider dummies in the regressions to capture unobservable data quality. Second, we measured the data quality of each provider as the proportion of its HES surgical admissions records which contained missing or invalid entries, weighted the proportion by the share of a practice's admissions accounted for by the provider and summed over all providers for each practice to get a practice level measure of provider data quality. The practice level data quality variable was then entered in the admission equation. Third, we divided our sample of practices on the basis of whether the above measure was below 20% in any year and estimated the difference in difference model separately on the two sub samples.

3.6.3 Practice population data

We had practice population data for all four years derived from practice patient lists. It suffer from list inflation: the total population on practice lists exceeds the best estimates of the English population by about 4% (Sutton et al, 2002). Most of the models reported in the paper are based on the time varying uncorrected practice populations but also include Health Authority dummy variables to capture some of the effects of differential list inflation. We also had list inflation corrected population data for one year (2000/1) which deflated practice populations by applying separate age, sex and local authority specific correction factors to practice populations. We used the data to estimate models with the admission rate calculated from constant but list inflation adjusted populations.

4 Results

4.1 Effects of budgetary regimes

Table 3 reports a variety of OLS, FE and population averaged difference in differences models using our preferred set of observations. Figure 3 plots the predicted (conditional mean) differences between fundholders and non-fundholders from the coefficients of the within and between population averaged model in Table 3, together with 95% confidence intervals.

The estimated effects of fundholding are very similar across the different estimation methods and there is no obvious pattern to differences across estimators:

- fundholders had lower admission rates than non-fundholders in 1997/8 (between 2.53 and 3.50 per 1000 compared with a mean rate over all practices over 4 years of about 61.5);
- they further reduced their admission rates, relative to non-fundholders, in the last year of fundholding (1998/9), though the difference in differences is quite small (-0.44 to -0.66) and is only significant at the 5% level in the OLS and PA models which include Health Authority effects;
- fundholders significantly increased their admission rates relative to non-fundholders in the first PCT year (difference in differences vary from 1.09 to 1.27);
- fundholders further increased their admission rates relative to non-fundholders in the second PCT year (difference in differences vary from 1.78 to 2.05) .

Fundholders had lower admission rates than non-fundholders in 1997/8 though not all of the difference can be attributed to the incentive effects of fundholding. Adding the difference in difference coefficient for 2000/01 relative to 1997/8 to the 1997/8 difference between fundholders and non-fundholders provides an estimate of the difference between fundholder and non-fundholders in 2000/01. All models suggest that ex-fundholders had lower admission rates than ex-nonfundholders in 2000/01 when both types were faced with the same budgetary regime.

Using the estimates from the within and between PA model we see that about 43% (3.4399 – 1.9573)/3.4399) of the difference between the elective admission rates of fundholders and non-fundholders under fundholding regime in 1997/8 due to selection and 57% to incentives. The incentive effect, the difference between admission rates in 2000/1 and 1997/8 divided by the four year mean fundholder admission rate, was 3.19%.

The time pattern of the difference in difference coefficients suggests that comparison of the last year of fundholding and the first year of the PCT regime does not yield an estimate of the long run effects of the change in the budgetary regime. The estimated pattern suggests that all three of the transitional effects discussed in section 2.2.2 were operating. The reduction in fundholder admission relative to nonfundholders in the last year of fundholding (1998/9) is evidence against uncertain waiting times being the only cause of transitional effects since this would imply that fundholder admission rates would be increased relative to those of nonfundholders because of the reduction in the expected price of admissions in 1998/9 relative to 1997/8. Hence either the deferral or fundholder balance transitional effects must also have been operating.

The fact that the difference in difference for 1999/2000 relative to 1997/8 was smaller than that for 2000/1 relative to 1997/8 suggests that the deferral of admissions to the first post fundholding year cannot be the whole explanation. It is suggests that though fundholders may have wanted to reduce admissions in the last year of fundholding in order to accumulate balances they were unable to predict precisely when referred patients would be admitted. The pattern of difference in difference coefficients is best explained as a combination of the three transitional effects.

Previous studies found evidence that early and late wave fundholders differed in their observed and unobserved characteristics (Baines and Whynes, 1996; Whynes et al, 1999) and responded differently to budgetary regimes (Gravelle et al, 2002). Table 4 has the fundholder effects for the seven waves of fundholders from a within and between PA model (not reported). For all fundholder waves, except the last (1997/8) wave, the difference in difference coefficients shows that fundholders increased their admission rates relative to non-fundholders between the penultimate year of fundholding and the second year of PCTs. However, the incentive effects of fundholding, defined as the difference in admission rates between 2000/1 and 1997/8 divided by the four year mean admission rate, were larger for the first four waves.

Fundholding led to a weighted average reduction of 6.15% in elective admissions for wave 1 to 4 fundholders compared with a reduction of 1.20% for wave 5 to 7 fundholders. The overall weighted average fundholder incentive effect when waves are distinguished is 3.30%, compared with the estimated incentive effect when waves are not distinguished of 3.19% (Table 3, model 5).

4.2 Other factors affecting admission rates

The coefficients on the other covariates in Tables 3 are generally plausible. Elective admission rates are reduced by *Waiting time*, *Access NHS* and by distance to substitute private providers (*Distance private*). The elasticity of admissions with respect to waiting time (calculated from the fixed effect model at the variable means) is -0.103 which is comparable, though somewhat smaller, to other studies (Gravelle et al, 2002; Gravelle et al, 2003a; Martin and Smith, 1999). Only *Consultants* has a counter-intuitive (negative) coefficient, though in the OLS model with provider effects, which may allow for possibly confounding effects of provider admission policies, the coefficient has a more plausible positive sign.

All models have a negative association of total *practice population* on admission rates. Practice population enters the denominator of the dependent variable and we interpret the negative coefficient as evidence of measurement error in practice populations rather than a genuine effect of practice size. The negative coefficient on the *Single handed GP* dummy variable is perhaps a better indicator of possible size effects on admission rates.

Practices with younger GPs or more UK qualified GPs have higher admission rates. Practices with more minor surgery or which are training practices have lower admission rates. The latter effect may be picking up an effect of practice quality on practice admission thresholds. The negative effect of practice dispensing may be in part due to the fact that one criterion for GPs to be allowed to dispense medicines is that patients live more than a certain distance from their surgery. Thus the variable may be an indirect indicator of rurality and access to secondary providers.

The practice population deprivation variables (*Low Birthweight*, *LISI, Disability Allowance, Education Deprivation*) have positive effects on admissions, probably reflecting both higher morbidity amongst the poor and the greater use of private health care amongst the more affluent and better educated. The negative coefficient on the deprivation measure *Job seekers* may be due to the concentration of such allowances amongst the younger, and hence generally healthier, population. The negative association between the *Non white* proportion and admission rates is in line with other studies and probably reflects lower use at given levels of morbidity (Sutton et al, 2002; Gravelle et al, 2003b) . The negative coefficient on *In Migration* is evidence of a selection effect operating at area level: the healthy are more mobile. The negative coefficient on *Rural patients* may reflect the effects of poorer access in rural areas.

4.3 Testing identifying assumptions

4.3.1 Differential time trends: elective and non-elective admissions

There were no financial incentives for fundholders to reduce admission rates for non-elective procedures since they were not charged for them. Indeed it has been suggested that fundholders attempted to have patients on the waiting list for elective fundholding procedures admitted as emergencies (Paton, 1995), though the evidence is contradictory (Goodwin, 1998). If fundholders had been able and willing to reduce elective admissions by increasing non-electives then we would expect to find that the temporal pattern of difference in difference coefficients for non-elective admissions was the opposite of that for elective admissions. In particular the difference in difference for 2000/1 relative to 1997/8 for non-electives should be negative.

Table 5 reports a variety of models of non-elective admissions in the same specialities and procedures as the elective models. There are fewer significant explanatory variables in the non-elective admission rate models but the pattern of non budgetary regime coefficients is very similar. Elective waiting times have a significant negative effect but the magnitude of the effect is very much smaller than for the elective admissions whereas the other supply variables have effects which are of comparable magnitude for electives and non-electives.

The results for the pooled OLS, fixed effects and population averaged models are very similar: the fundholding effect and the difference in difference coefficients are all very small (the mean non-elective admission rate over the four years is 23.97 per 1000) and insignificant. Figure 3 plots the estimated differences between the admission rates for fundholders and nonfundholders over the four years from a between and within population averaged model (not reported here).

We also estimated a seemingly unrelated difference in differences regression model for elective and non-elective admissions. The results are in the last two columns of Table 5. The coefficients in the elective admissions model are similar to those for the OLS model in Table 3. In particular there is a significant negative effect of fundholding in 1997/8 and significant difference in difference coefficients for 1998/9 (negative), 1999/2000 (positive) and 2000/1 (positive).

We compared the difference in differences coefficients between the elective and non-elective models as a form of difference in difference in differences test for the possibility that the significant difference in difference effects in the elective model are due to differential time trends between fundholders and nonfundholders. In the SUR model the restriction that there is no difference between elective and non-elective difference in differences coefficients is not rejected for 1998/9 ($\chi^2(1) = 1.83$, p = 0.176) but is rejected for 1999/2000 (($\chi^2(1) = 6.03$, p = 0.014) and for 2000/1 ($\chi^2(1) = 19.79$, p = 0.000). Figure 3 also shows that there is essentially no change in the difference between fundholder and nonfundholder non-elective admission rates over time, in contrast to the clear reduction in the difference for elective admission rates after the end of fundholding. Thus the difference in differences for elective admissions do not appear to be due to time trends having the same effect on a practice's elective and non-elective admission rates but differing effects between fundholder and nonfundholder practices. The figure also does not provide any evidence that fundholders responded to the financial incentive to substitute emergencies for electives.

4.3.2 Implicit prices

Table 6 reports attempts to test the identifying assumptions (7) and (9) by estimating cross section models including the reciprocal of the number of practices in the HA or the PCT. Since fundholders will generally not exceed their fundholding budgets the theoretical model predicts that the number of practices in their HA should have no effect on their admissions whilst nonfundholders' admission rates would be smaller in HAs with fewer practices. Thus in the model for 1997/8 we expect a negative coefficient on the reciprocal of the number of practices and a positive coefficient on its interaction with the fundholding dummy. Both coefficients are negative in 1997/8 but their *t* statistics are very small so that we cannot reject the null hypothesis that the number of practices had no effect on admissions in 1997/8.

In 2000/1 the implicit price facing all practices, whether ex-fundholders or ex-nonfundholders, is inversely related to the number of practices in their PCT so that the theoretical model predicts that both types of practices will have lower admission rates in PCTs with fewer practices. The results for 2000/1 are compatible with the prediction in that both the coefficient on the reciprocal of the number of practices and that on its interaction with the fundholding dummy are negative: the implicit price variable has a larger negative effect on ex-fundholders than fundholders. This is not implausible but if the coefficient on the implicit price variable was significant it would suggest that identifying assumption (7) is violated. If the coefficient on the interaction term was significant then we could reject the identifying assumption (9). Neither coefficient is significant though that on the implicit price variable is considerably larger (–1.39). than that on the interaction (–0.44). We believe that the difference in difference coefficient for 2000/1 relative to 1997/8 in the models reported earlier are identifying the effect on fundholders of the fundholding regime relative to the non-fundholding regime and that, if anything, the effect is underestimated by the difference in difference.

4.4 Robustness to data quality

4.4.1 Exclusion of observations

Table 7 shows the effect of the cumulative exclusion of observations in a fixed effect model of elective admissions. The pattern of difference in difference coefficients is similar across all five observation sets, especially for the final year of fundholding (1998/9) and the second year of the PCT regime (2000/1). After dropping practices with populations under 1000 the results suggest that fundholders significantly increased their admission between the penultimate year of fundholding and the second year of PCTs.

There were no obvious differences, apart from the exclusion criteria and the proportion of fundholders, between included and dropped practices. Fundholders accounted for 41% of dropped practices, compared with 49% in the final sample. This was to be expected since one of the criteria for dropping practices was a recorded practice size of less than 1000 and the minimum size requirement for fundholders never fell below 5000. The fact that 16% of the 55 practices dropped for this reason were fundholders is further evidence of inaccuracies in the population data and supports the omission of small practices from the analysis.

4.4.2 Admission data quality

In Table 8 the first two sets of results are from OLS and FE regressions with the data quality variable. Practices using providers with better quality data have lower admission rates, which is to be expected since data quality is defined in terms of missing entries (see section 3.6.2). However, the effects of fundholding are very similar to those in the Table 3. We also divided practices according to whether their providers had good or poor data and estimated FE models on each set of practices. The pattern of difference in differences coefficients are both very similar to those in Table 3, though the coefficients estimated on the much smaller set of practices with poor data quality providers are not significant.

4.4.3 Alternative practice population estimates

We estimated the difference in difference models using the alternative population measure which has the advantage of being adjusted for list inflation but the disadvantage of being available for only one year so that the same population had to be applied to four years of admissions to produce admission rates. As the OLS and FE results in the last four models in Table 8 show, the population adjustment makes very little difference to the results.

5 Conclusions

Using a large sample of most English general practices and adopting a difference in difference methodology to address selection bias, this study offers clear evidence that the policy of GP fundholding exerted downward pressure on secondary care admissions for elective surgery. Our estimates suggest that elective surgical admissions amongst the practices that chose to become fundholders was about 3.3% lower than they would have been in the absence of fundholding.

There was considerable heterogeneity in the response to the opportunities offered by fundholding amongst general practices. Over the seven years in which it was possible to become a fundholder nearly half (49%) of our practices chose not to do so. Fundholders differed from non-fundholders in both observed and unobserved characteristics. After allowing for a wide range of observable practice characteristics, the conditional mean elective admission rate for fundholders in 1997/8 was 5.6% less than for non-fundholders. 57% of this difference was due the incentive effects of fundholding but 43% arose from unobserved differences between the characteristics of fundholding and non-fundholding practices.

There was also considerable heterogeneity amongst practices that chose to become fundholders with the incentive effect of fundholding leading to a reduction in admission rates of 6.2% for wave 1 to 4 fundholders compared with a reduction of only 1.2 % for wave 5 to 7 fundholders.

We found no evidence that fundholding led to the substitution of emergency admissions for electives. There were no significant differences between the admission rates of fundholders and nonfundholders nor between the changes in their admission rates.

We believe that we have produced a robust answer to one of the major questions in an assessment of the fundholding experiment and have shown that gatekeeping practices will reduce their elective admissions by modest but not negligible amounts when confronted with a budget and explicit prices. The results have implications for the new budgetary arrangements in the NHS and, given that gatekeeping is widespread in health care systems, may also be relevant for many other settings.

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Data Appendix

Admissions

Annual data on admissions at NHS Trusts were taken from Hospital Episode Statistics for 1997/8 to 2000/1 for consultant episodes which finished in each year. There were about 11.7M episodes in total in 2000/1. We used the episodes for all procedures in a number of main specialities: General Surgery, Urology, Trauma and Orthopaedics, Ear, Nose and Throat, Ophthalmology, Oral surgery, Restorative dentistry, Orthodontics, Gynaecology. In these specialities most procedures were covered by the fundholding scheme. We distinguish between elective or booked admissions (which would have been charged to the fundholder for fundholding procedures) and non-elective or emergency admissions, which would not have been charged to fundholders. The 3M elective and 1.25M non-elective surgical episodes per year in the specialities were aggregated to practice level.

Populations

Data on practice populations (total and by age and sex groups) were taken from the PCT database at the National Primary Care Research and Development Centre (http://www.primary-care-db.org.uk/) for each of the four years and used to calculate crude admission rates for each practice for elective and non-elective surgery. Because of delays in removing patients who die or move from lists these data suffer from list inflation. We also had list inflation corrected population data for one year (2000/1) from the AREA database (Sutton et al, 2002) which deflated practice populations by applying separate age, sex and local authority specific correction factors to practice populations.

Waiting times

The waiting time (the difference between the date of the elective procedure and the date the patient was placed on the elective waiting list) was available from HES for each patient admitted as an elective patient. The waiting time for each practice for each year was calculated as the mean elective wait for its patients admitted as electives in the year (mean and median waiting times were very similar).

Practice characteristics

The fundholding status and wave of fundholding for each practice were derived from lookup tables from the NHS Operational Codes Service. We had data on practice characteristics for 1999, based on the Department of Health's General Medical Statistics, from the NPCRDC website. They included GP age, sex, country of qualification, numbers of GPs, whether GPs were approved trainers, whether the practice was in receipt of quality payments, and whether the practice offered different types of clinics.

Practice patient characteristics

In addition to the age and sex composition of the practice populations, we also had information on their socio-economic characteristics. The main sources of socio-economic data were the 1991 Census and the components of the Index of Multiple Deprivation (DETR 2000) which uses information on Social Security payments in 1998 and 1999. The data are available at small area (frozen 1998 electoral ward) level. They were attributed to practices by taking weighted averages based on the proportion of practice populations resident in each ward (from the Department of Health's Attribution Data Set used to calculate the 2000/1 funding allocations to HAs). Some socio-economic data, such as the Low Income Scheme Index (the proportion of prescriptions from a practice which were dispensed without charge because the patient was exempt on grounds of low income), related directly to the practice.

Supply factors

We used data on supply factors from the AREA project (Sutton et al, 2002) including distance from practice populations to NHS Trusts, private hospitals, residential and nursing homes, numbers of beds and consultants at NHS Trusts.

Figure 1. Budget constraints for fundholders, non fundholders and for practices after fundholding.

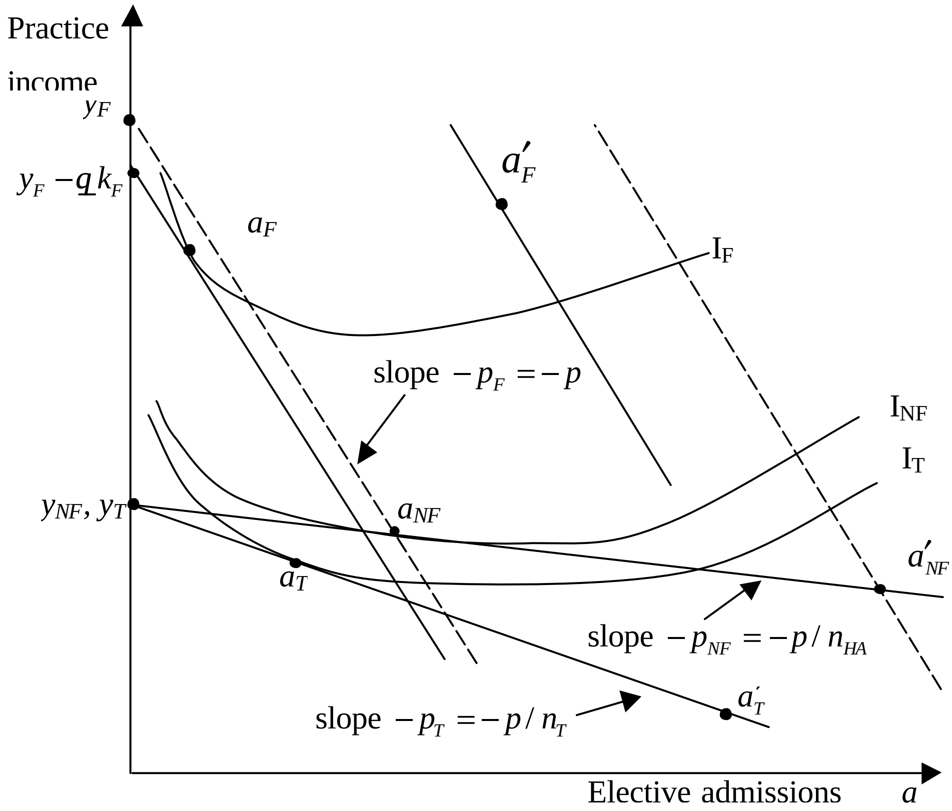
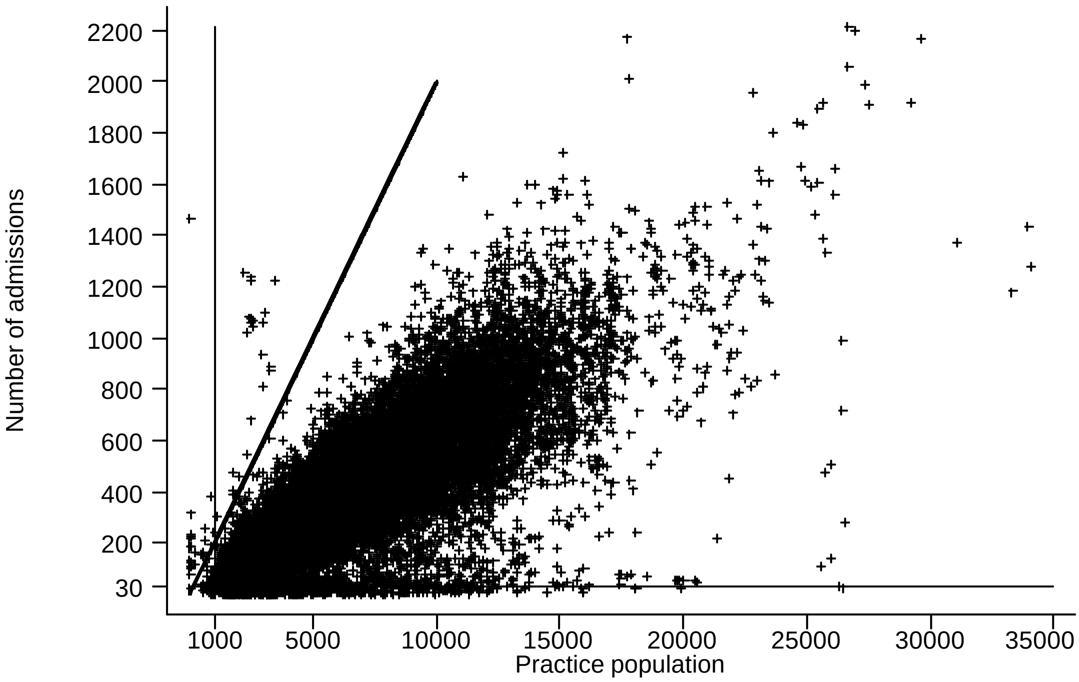
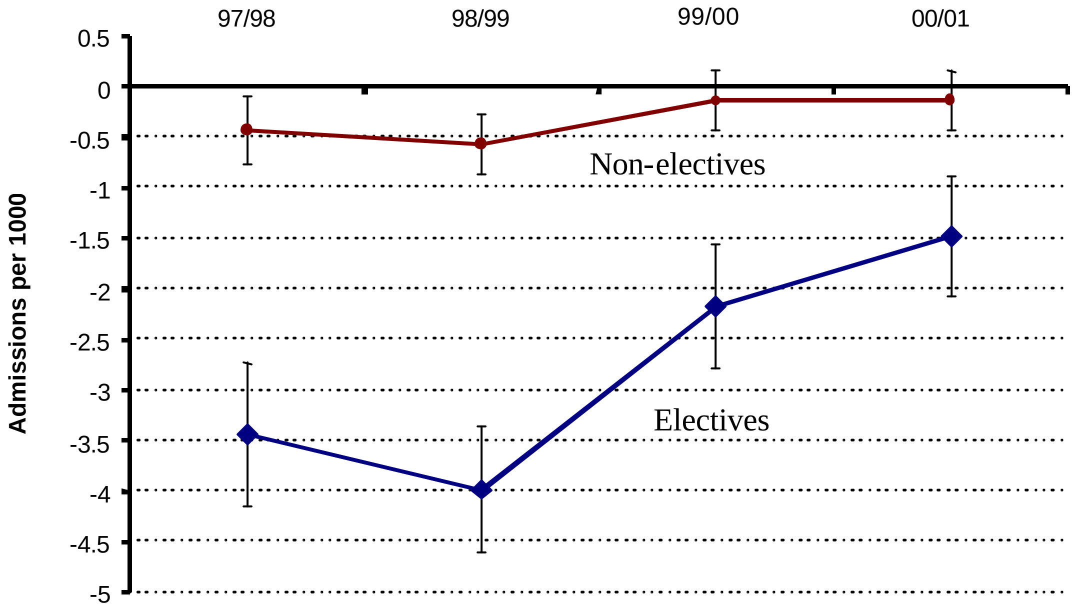


Figure 2. Elective admissions and practice populations 1997/8 to 2000/1



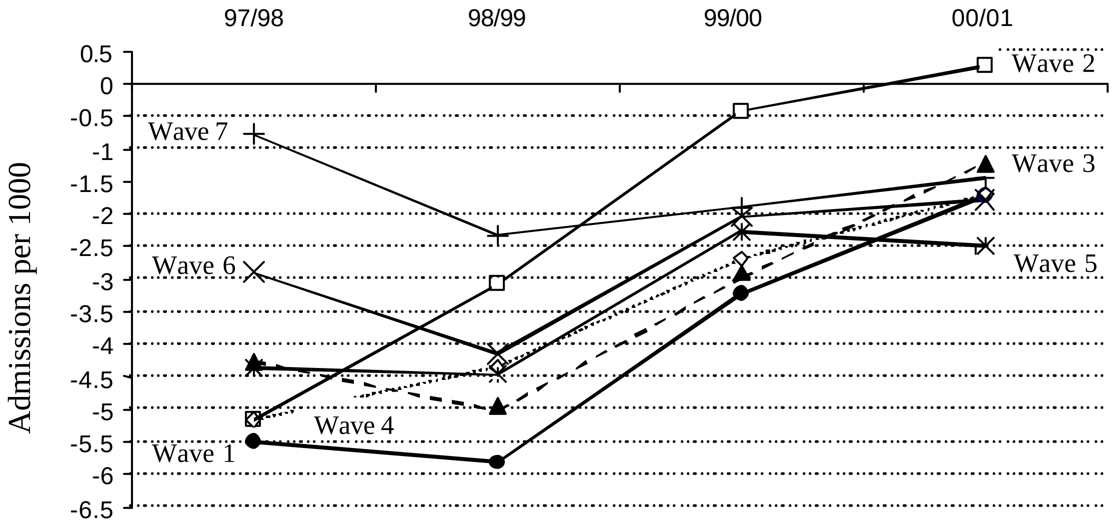
Note: Plot of initial sample of 30422 observations. Lines show practices excluded because (a) population under 1000, (b) admissions under 30 and (c) admission rate under 200

Figure 3. Annual differences between fundholder and non-fundholder admission rates



Note. Plots coefficients from population averaged within and between models. Error bars are 95% confidence intervals. For 1998/9, 1999/2000 and 2000/1 they are derived from the joint distribution of the sum of the fundholding and difference in difference coefficients.

Figure 4. Annual difference between fundholder and non-fundholder elective admission rates by wave of fundholder



Note: derived from within and between population averaged estimates in Table 4.

Table 1. Variable definitions and summary statistics

Variable	Definition	Years	Source	Mean	Std. Dev.	Max	Min
Admission rate*	Elective admission rate	1997/8 – 2000/01	HES/GMS	60.334	19.847	192.481	2.002
Fundholder	GP fundholding status in 1997/8	1997/8	OCS	0.513			
Waiting time*	Average inpatient waiting time (days)	1997/8 – 2000/01	HES	110.177	27.224	264.063	33.409
Practice pop*	List size of practice	1997/8 – 2000/01	GMS/ NPRDC	6089.470	3706.128	34044	1005
List per GP*	List size per GP	1997/8 – 2000/01	GMS/ NPRDC	1985.671	565.392	6918	515.750
Male GPs	Proportion of male GPs	1999/00	GMS/NPRDC	0.714	0.272	1	0
UK qualified GPs	Proportion GPs qualified in UK	1999/00	GMS/NPRDC	0.699	0.412	1	0
GPs over 50	Proportion of GPs aged over 50	1999/00	GMS/NPRDC	0.375	0.383	1	0
Minor surgery GPs	Proportion of GPs performing minor surgery	1999/00	GMS/NPRDC	0.750	0.372	1	0
Single GP practice	Practice had one GP	1999/00	GMS/NPRDC	0.243			
Training practice	Practice has approved training status	1999/00	GMS/NPRDC	0.268			
Dispensing practice	Practice can dispense pharmaceuticals	1999/00	GMS/NPRDC	0.144			
Access NHS	Beds weighted distance to secondary care	2000	OCS/AREA	2.801	0.828	5.527	0.355
Distance private	Distance from practice to nearest 5 private hospitals	2000	OCS/AREA	22.891	10.116	98.989	10.458
Consultants	Number of consultants at main acute provider	2000	OCS/AREA	131.139	57.787	391.350	38.262
Low birthweight	Percentage of babies who were low birthweight	1993-1997	ID/AREA	7.506	1.475	12.973	2.523
Over 75 alone	Proportion of over 75s living alone	1991	Census/AREA	0.480	0.048	0.645	0.127
Non white	Proportion from minority ethnic group	1991	Census/AREA	0.079	0.118	0.688	0.000
Rural patients	Proportion of rural patients	1999/00	GMS/NPRDC	0.085	0.174	1	0
In migration	Proportion migrants from outside Local Authority	1991	Census/AREA	0.040	0.020	0.237	0.007
Disability allowance	Incapacity/Severe disability allowance claimants	1999	ID/AREA	99.783	51.464	434.157	13.772
Education deprivation	Index of education deprivation	2000	ID/AREA	0.253	0.758	2.792	-2.285
Job seekers	Job seekers allowance	1998	ID/AREA	4.848	3.279	20.550	0.285
Low income	Percentage of prescriptions dispensed with low income exemption (LISI score)	2000	PSU/NPRDC	11.352	7.825	56.455	0.380

* Denotes data available for all four years 1997/8 – 2000/1. All other data is time invariant. For time varying data summary statistics are calculated over four years. Data refers either to financial year (e.g. 1997/98) or calendar year (e.g. 2000) and is for the final sample of 7105 practices.

HES - Hospital Episode Statistics, OCS - Organisational Codes Service, GMS - General Medical Statistics, NPRDC - National Primary Care Research and Development Centre, PSU - Prescribing Support Unit, AREA – Allocation of Resources to English Areas Report, ID - Indices of Deprivation.

Table 2. Practices by fundholding wave

Wave (first year as fundholder)	Number of practices	%	Mean elective admission rate per 1000			
			1997/8	1998/9	1999/2000	2000/1
Wave 1 (1991/2)	250	3.52	52.381	57.617	59.788	60.849
Wave 2 (1992/3)	217	3.05	52.493	60.446	62.608	62.722
Wave 3 (1993/4)	544	7.66	55.856	60.730	62.232	63.059
Wave 4 (1994/5)	552	7.77	53.105	59.435	60.741	60.722
Wave 5 (1995/6)	365	5.14	51.928	57.589	59.528	57.713
Wave 6 (1996/7)	1104	15.54	57.005	61.533	63.560	62.390
Wave 7 (1997/8)	615	8.66	56.906	61.366	61.697	60.776
Total Fundholders	3647	51.33	55.133	60.340	61.902	61.411
Non-fundholders	3458	48.67	56.820	62.776	62.868	61.557
All practices	7105	100	55.954	61.526	62.372	61.482

Table 3. Alternative estimates for elective admissions

	OLS (HA effects)	OLS (Provider effects)	FE	PA (HA effects)	PA (within and between)
1998/9	6.6028	6.4020	6.5202	6.4781	6.5202
	[27.96]**	[25.70]**	[25.44]**	[27.76]**	[29.39]**
1999/2000	5.7498	5.8481	5.7556	5.8009	5.7556
	[20.26]**	[20.32]**	[18.70]**	[20.72]**	[21.60]**
2000/1	4.6978	4.8405	5.2726	4.8694	5.2726
	[15.76]**	[16.04]**	[15.12]**	[16.38]**	[17.47]**
Fundholder	-3.0778	-3.3459		-2.5258	-3.4399
	[8.53]**	[9.22]**		[6.99]**	[9.57]**
Diff in diff 98/99 – 97/98	-0.6387	-0.4421	-0.5408	-0.6556	-0.5408
	[2.05]*	[1.38]	[1.59]	[2.12]*	[1.84]
Diff in diff 99/00 – 97/98	1.0915	1.1222	1.2704	1.0426	1.2704
	[2.88]**	[2.95]**	[3.06]**	[2.77]**	[3.53]**
Diff in diff 00/01 – 97/98	1.8441	2.0489	1.9573	1.7777	1.9573
	[4.68]**	[5.22]**	[4.39]**	[4.54]**	[5.08]**
Waiting time	-0.0847	-0.0552	-0.0564	-0.0679	-0.0564
	[13.07]**	[7.59]**	[7.98]**	[12.13]**	[9.22]**
Mean wait over 4 years					-0.1120
					[10.31]**
Practice pop	-0.0003	-0.0003	-0.0069	-0.0007	-0.0069
	[6.08]**	[6.50]**	[18.96]**	[12.98]**	[21.90]**
Mean pop over 4 years					-0.0001
					[3.19]**
List per GP	-0.0007	-0.0009	-0.0015	-0.0016	-0.0015
	[2.74]**	[3.63]**	[3.87]**	[6.11]**	[4.47]**
Mean list per GP over 4 years					-0.0001

				[0.18]
Male GPs	1.6245	1.4862	2.6788	1.0826
	[2.99]**	[2.79]**	[4.80]**	[1.97]*
GPs over 50	-1.3080	-1.4081	-1.2926	-1.3268
	[2.82]**	[3.16]**	[2.75]**	[2.87]**
UK qualified GPs	1.0397	0.9651	1.6225	0.8247
	[2.24]*	[2.12]*	[3.43]**	[1.78]
Minor surgery GPs	-1.3578	-1.5863	-1.3015	-1.3565
	[2.90]**	[3.51]**	[2.74]**	[2.91]**
Single GP practice	-1.1248	-0.9573	-2.1566	-0.9712
	[2.66]**	[2.30]*	[5.02]**	[2.24]*
Training practice	-0.6037	-0.3584	0.2777	-0.8617
	[2.01]*	[1.21]	[0.90]	[2.88]**
Dispensing practice	-1.1069	-0.9900	-1.1981	-1.0925
	[2.20]*	[1.96]*	[2.32]*	[2.20]*
Access NHS	1.2106	2.4463	1.2544	1.2015
	[2.95]**	[5.25]**	[3.06]**	[2.92]**
Distance private	0.0838	0.0211	0.0846	0.0842
	[2.68]**	[0.44]	[2.67]**	[2.71]**
Consultants	-0.0248	0.0209	-0.0254	-0.0241
	[4.62]**	[2.71]**	[4.64]**	[4.51]**
Low birthweight	0.4554	0.3455	0.4728	0.4576
	[2.85]**	[2.04]*	[2.92]**	[2.88]**
Over 75 alone	-10.0816	0.5294	-10.0125	-9.4979
	[2.21]*	[0.12]	[2.17]*	[2.07]*
Non white	-6.3815	-8.1689	-5.9712	-6.5894
	[2.89]**	[3.32]**	[2.66]**	[2.95]**
Rural patients	-6.5350	-4.5867	-7.3383	-6.2325
	[5.69]**	[3.83]**	[6.25]**	[5.47]**

In migration	-105.9429	-68.6248		-110.0454	-105.9887
	[9.76]**	[5.95]**		[9.88]**	[9.75]**
Disability allowance	0.0481	0.0609		0.0493	0.0485
	[6.16]**	[6.93]**		[6.24]**	[6.23]**
Educ Deprivation	3.4510	4.3874		3.2686	3.5582
	[10.82]**	[13.43]**		[10.05]**	[11.18]**
Job seekers	-0.9839	-1.1419		-0.9729	-1.0198
	[8.03]**	[8.62]**		[7.89]**	[8.31]**
Low income	0.5207	0.4548		0.4892	0.5333
	[8.01]**	[7.16]**		[7.40]**	[8.22]**
Low income squared	-0.0130	-0.0099		-0.0130	-0.0130
	[8.51]**	[6.97]**		[8.44]**	[8.52]**
Constant	41.273	17.9952	56.794	8.806	78.9957
	[1.54]	[0.66]	[1.92]	[0.41]	[1.93]
R2	0.57	0.60	0.84		
Observations	7105 x 4	6776 x 4	7105 x 4	7105 x 4	7105 x 4

Dependent variable: crude elective admission rate. Practice age/gender proportions included. All estimators use robust standard errors and clustering within practices.
* significant at 5%; ** significant at 1%

Table 4. Fundholder wave effects

Fundholder wave (first year as fundholder)	Fundholder effect 97/98	Diff in diff 98/99 – 97/98	Diff in diff 99/00 – 97/98	Diff in diff 00/01 – 97/98	Incentive effect %
Wave 1 (1991/2)	-5.4989	-0.3182	2.2695	3.7789	6.55
	[6.04]**	[0.47]	[2.70]**	[4.33]**	
Wave 2 (1992/3)	-5.1619	2.0825	4.7407	5.4399	9.13
	[4.43]**	[2.14]*	[3.98]**	[4.08]**	
Wave 3 (1993/4)	-4.2685	-0.7592	1.2915	3.0507	5.05
	[6.70]**	[1.52]	[2.03]*	[4.25]**	
Wave 4 (1994/5)	-5.1602	0.8112	2.4722	3.4598	5.91
	[7.68]**	[1.44]	[3.66]**	[4.73]**	
Wave 5 (1995/6)	-4.3623	-0.1021	2.0938	1.8739	3.31
	[5.55]**	[0.16]	[2.67]**	[2.19]*	
Wave 6 (1996/7)	-2.9001	-1.2494	0.8525	1.1144	1.82
	[5.61]**	[3.00]**	[1.66]	[2.02]*	
Wave 7 (1997/8)	-0.7745	-1.5449	-1.1161	-0.6742	-1.12
	[1.15]	[2.55]*	[1.62]	[0.95]	

Dependent variable: crude elective admission rate. Within and between population averaged estimates with robust standard errors and clustering. Same set of covariates as model 5, Table 3. Incentive effect: - (diff in diff 00/01-97/98)/(mean elective admission rate 1997/8 to 2000/1 for fundholdersof relevant wave)

* significant at 5%; ** significant at 1%

Table 5. Elective and non-elective admissions

	Non-elective			Non-elective	Elective
	OLS	FE	PA	SUR	SUR
1998/99	1.029	1.142	1.071	0.971	6.516
	[8.27]**	[8.32]**	[8.70]**	[6.55]**	[20.88]**
1999/00	1.751	1.627	1.708	1.623	5.560
	[12.59]**	[10.44]**	[12.39]**	[10.96]**	[17.83]**
2000/01	1.678	1.689	1.650	1.525	4.494
	[11.62]**	[10.21]**	[11.52]**	[10.26]**	[14.37]**
FH – NFH 1997/98	-0.265		-0.077	-0.285	-2.992
	[1.54]		[0.45]	[1.91]	[9.50]**
Diff in diff 98/99 – 97/98	-0.224	-0.139	-0.215	-0.197	-0.682
	[1.38]	[0.78]	[1.33]	[0.96]	[1.57]
Diff in diff 99/00 – 97/98	0.104	0.292	0.134	0.160	1.042
	[0.56]	[1.41]	[0.73]	[0.78]	[2.40]*
Diff in diff 00/01 – 97/98	0.139	0.296	0.157	0.190	1.787
	[0.74]	[1.39]	[0.84]	[0.92]	[4.12]**
Waiting time	-0.009	-0.017	-0.014	-0.010	-0.083
	[2.93]**	[5.36]**	[5.65]**	[4.91]**	[19.22]**
Practice pop	-0.00015	-0.00028	-0.00027	-0.00014	-0.00029
	[8.86]**	[15.92]**	[13.20]**	[12.82]**	[10.45]**
List per GP	-0.00067	-0.00101	-0.00099	-0.00068	-0.00066
	[5.88]**	[3.60]**	[8.42]**	[9.32]**	[4.04]**
Male GPs	0.801		0.992	0.744	1.657
	[3.25]**		[3.96]**	[4.62]**	[4.86]**
GPs over 51					-1.006
					[4.49]**

UK qualified GPs					1.670
					[7.16]**
Minor surgery					-1.197
					[5.27]**
Single GP practice					-1.287
					[6.11]**
Training practice					-0.654
					[3.89]**
Dispensing practice					-1.322
					[5.10]**
Access NHS	1.5869		1.6521	1.5413	1.1124
	[9.64]**		[10.07]**	[14.87]**	[4.65]**
Distance private					0.086
					[5.17]**
Consultants	-0.009		-0.009	-0.008	-0.024
	[3.79]**		[3.72]**	[5.01]**	[7.15]**
Non white	-8.261		-8.365	-8.473	-5.918
	[7.53]**		[7.55]**	[11.94]**	[3.92]**
Rural patients					-5.565
					[9.38]**
In migration	-17.039		-18.050	-16.195	-105.815
	[3.64]**		[3.76]**	[4.64]**	[14.32]**
Low birthweight	0.276		0.283	0.285	0.400
	[3.84]**		[3.91]**	[5.99]**	[3.98]**
Over 75 alone					-8.065
					[3.48]**
Disability allowance	0.048		0.048	0.048	0.050
	[12.29]**		[12.33]**	[21.50]**	[10.53]**
Education deprivation	0.901		0.849	0.907	3.357
	[5.94]**		[5.57]**	[9.28]**	[16.27]**

Job seekers	-0.367		-0.367	-0.374	-1.003
	[6.24]**		[6.25]**	[10.55]**	[13.25]**
Low income	0.257		0.257	0.254	0.543
	[8.22]**		[8.08]**	[12.81]**	[12.92]**
LISI income squared	-0.003		-0.004	-0.003	-0.013
	[4.12]**		[4.43]**	[7.05]**	[13.99]**
Constant	11.705	46.864	9.396	9.056	31.010
	[0.92]	[3.13]**	[0.89]	[1.03]	[1.68]
R-squared	0.46	0.78			
Observations	7113 x 4	7113 x 4	7113 x 4	7052 x 4	7052 x 4

Dependent variables: crude practice admission rate. Practice age/gender proportions included. HA effects in OLS models. All estimators use robust standard errors and clustering within practices.
* significant at 5%; ** significant at 1%

Table 6. Effect of number of practices in HA and PCT

	1997/8	2000/1
Fundholder	-2.431	-1.402
	[2.51]*	-1.74
Practices in PCO	-175.5	-18.96
	[0.57]	[-1.39]
Fundholder × Practices in PCO	-76.1	-5.91
	[0.95]	[-0.44]
Constant	68.021	79.640
	[1.18]	[1.67]
R2	0.60	0.681
Observations	6771	6771

Dependent variable: crude practice elective admission rate. Separate OLS cross sections estimated with robust SEs and clustering within HAs (1997/8) or PCTs (2000/1). Models also include all covariates in Table 3.
* significant at 5%; ** significant at 1%

Table 7. Effect of dropping observations

	(1)	(2)	(3)	(4)	(5)
Exclusions:	None	Pop < 1000	(2) + Admissions < 30	(3) + Adm Rate > 200/1000	(4) + no LISI score
1998/9	5.6069	6.4439	6.6330	6.5285	6.5202
	[1.50]	[23.65]**	[25.25]**	[25.46]**	[25.44]**
1999/2000	10.9248	7.3489	5.8261	5.7820	5.7556
	[2.23]*	[19.24]**	[18.46]**	[18.75]**	[18.70]**
2000/1	21.4979	7.1142	5.4267	5.2625	5.2726
	[2.97]**	[16.80]**	[15.28]**	[15.10]**	[15.12]**
Diff in diff 98/99 – 97/98	-0.3338	-0.9327	-0.6905	-0.5841	-0.5408
	[0.09]	[2.51]*	[1.89]	[1.72]	[1.59]
Diff in diff 99/00 – 97/98	3.3086	0.1390	1.0964	1.2014	1.2704
	[0.76]	[0.28]	[2.55]*	[2.89]**	[3.06]**
Diff in diff 00/01 – 97/98	1.6192	1.1388	1.7735	1.9086	1.9573
	[0.36]	[2.11]*	[3.86]**	[4.29]**	[4.39]**
Wait	0.0365	-0.0045	-0.0573	-0.0560	-0.0564
	[1.05]	[0.50]	[7.93]**	[7.98]**	[7.98]**
Practice pop	-0.0156	-0.0082	-0.0084	-0.0066	-0.0069
	[2.92]**	[7.52]**	[7.54]**	[15.67]**	[18.96]**
List per GP	-0.0249	-0.0010	-0.0011	-0.0015	-0.0015
	[2.15]*	[1.52]	[1.81]	[3.93]**	[3.87]**
Constant	2797.5179	48.8785	76.6553	51.9438	56.7940
	[0.96]	[1.37]	[2.34]*	[1.75]	[1.92]
Observations	(7605 x 4) + (1 x 2)	7505 x 4	7165 x 4	7151 x 4	7105 x 4
R2	0.74	0.82	0.85	0.84	0.84

Dependent variable: crude admission rate. Practice age/gender proportions included. Fixed effects with robust standard errors and clustering of errors within practices. *significant at 5%; ** significant at 1%

Table 8. Robustness to quality of admission data and to alternative measures of practice population

	Quality of admission data				Practice population measures			
	OLS	FE	FE poor data	FE good data	OLS time varying	OLS time invariant	FE time varying	FE time invariant
1998/9	6.9176	6.7775	6.6752	7.0714	6.5889	6.9208	6.5308	6.7222
	[28.51]**	[25.97]**	[10.18]**	[20.62]**	[27.82]**	[31.24]**	[25.47]**	[26.18]**
1999/2000	6.4071	6.2508	6.5775	6.0583	5.7540	5.2482	5.7842	5.3839
	[21.40]**	[19.43]**	[7.35]**	[14.97]**	[20.24]**	[20.19]**	[18.76]**	[18.00]**
2000/1	5.4880	5.8919	7.1153	5.5004	4.6901	4.7663	5.2911	4.9127
	[17.31]**	[16.16]**	[5.40]**	[12.07]**	[15.67]**	[16.48]**	[15.15]**	[14.79]**
Fundholder	-3.2261				-3.0935	-2.5271		
	[8.89]**				[8.55]**	[6.20]**		
Diff in diff 98/99 – 97/98	-0.5642	-0.4997	-0.8399	-0.4935	-0.6120	-0.6800	-0.5357	-0.7140
	[1.80]	[1.46]	[1.15]	[1.02]	[1.96]*	[2.31]*	[1.57]	[2.11]*
Diff in diff 99/00 – 97/98	1.2243	1.3503	1.7160	1.2737	1.1051	1.3054	1.2587	1.1960
	[3.20]**	[3.23]**	[1.54]	[2.25]*	[2.91]**	[3.64]**	[3.02]**	[2.89]**
Diff in diff 00/01 – 97/98	2.1266	2.1446	1.8977	1.9864	1.8727	1.8750	1.9587	1.7707
	[5.36]**	[4.79]**	[1.22]	[3.33]**	[4.74]**	[4.73]**	[4.39]**	[3.87]**
Admission data quality	-4.9785	-3.6268						
	[10.03]**	[8.44]**						
Constant	42.5102	55.0930	43.0075	33.5038	42.7762	-312.5612	57.9674	63.2951
	[1.58]	[1.87]	[0.27]	[1.03]	[1.60]	[3.05]**	[1.95]	[79.24]**
R2	0.57	0.84	0.89	0.85	0.57	0.55	0.84	0.85
Observations	7004 x 4	7004 x 4	1620 x 4	5384 x 4	7079 x 4	7079 x 4	7079 x 4	7079 x 4

Dependent variable: crude elective admission rate. All estimates with robust standard errors and clustering. Models also included the same variables as the corresponding models in Table 3. Practice age/sex proportions also included. HA effects in OLS model. See text for definition of data quality index. * significant at 5%; ** significant at 1%

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