The Effect of Spending Cuts on Teen Pregnancy

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Abstract: In recent years, English local authorities have been forced to make significant cuts to devolved expenditure. In this paper, we examine the impact of reductions in local expenditure on one particular public health target: reducing rates of teen pregnancy. Contrary to predictions made at the time of the cuts, panel data estimates provide no evidence that areas which reduced expenditure the most have experienced relative increases in teenage pregnancy rates. Rather, expenditure cuts are associated with small reductions in teen pregnancy rates, a result which is robust to a number of alternative specifications and tests for causality. Underlying socio-economic factors such as education outcomes and alcohol consumption are found to be significant predictors of teen pregnancy.

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1. Introduction

The global financial crisis in 2008 led many countries to introduce fiscal tightening in an attempt to reduce public expenditure and borrowing. In England and Wales, a major source of expenditure cuts has been through reductions of grants disbursed by central government to local authorities. Given heavy restrictions on local tax-raising powers, this has forced public sector managers to implement significant cuts in discretionary expenditures at a local level (Hood and Dixon, 2013). As budget changes are potentially exogenous, the recent programme of cuts provides useful opportunities to evaluate the effectiveness of local authority spending. In this paper, we examine the impact of cuts to expenditure on a discrete area of targeted policy - reducing rates of teen pregnancies.

High teen pregnancy rates have long been seen as a significant public health concern in many countries. This is particularly the case in the U.K. which has historically had one of the highest teen pregnancy rates in the world. To tackle the problem, in 1999, the U.K. Government launched the Teenage Pregnancy Strategy, a major programme aimed at halving the under-18 conception rate in England by the year 2010 (Social Exclusion Unit, 1999).[1] The cornerstones of the Strategy were expanding access to sexual and relationships education (SRE) and contraception for young people.

On the face of it these measures might be expected to lower pregnancy rates. Teenage pregnancy services have been a significant victim of recent local government expenditure cuts (Billingsley, 2011) and it has been suggested that this would have an adverse effect on teenage pregnancy rates. However, the empirical and theoretical literature suggests a more ambiguous prognosis. For instance, increased contraceptive use has been argued to increase sexual-risk taking (Akerlof et al, 1996; Paton, 2002) while a recent Cochrane review of trials of school-based SRE concluded that "there was no apparent effect on the number of young women who were pregnant" (Mason-Jones et al, 2016, p.24). Our aim in this paper is to assess what effect cuts to Teenage Pregnancy Strategy-related services have actually had on teenage pregnancy rates.

In the next section, we provide some background for our analysis. In section 3, we describe our empirical methodology. In section 4, we introduce and describe the data. In section 5, we report results of formal panel-data regressions which seek to identify the statistical relationship between expenditure cuts and changes in teenage pregnancy rates. In

[1] Responsibility for public health is devolved to the home nations in the U.K.

the final section we discuss the strengths and weaknesses of our approach and the implications of our findings for policy.

2. Background

During the 1980s and 1990s, teenage pregnancy rates in the U.K. were double those typical in western Europe (Social Exclusion Unit, 1999). This problem drew attention from commentators and policy makers and led to several policy interventions, culminating in the 1999 English Teenage Pregnancy Strategy. Although the Strategy involved some co-ordination and expenditure at a national level (e.g. an awareness campaign carried out in national media), delivery of the programme was largely devolved to Tier One local authorities. Central government allocated substantial sums of money to local authorities in the form of annual Local Implementation Grants (LIGs), the size of which was determined mainly by the number of female teenagers in each area, local costs and pre-existing under-18 conception rates.[2] Public health managers within local authorities were given considerable discretion as to how these grants were spent, though guidance was provided by a central Teenage Pregnancy Unit. Local authorities also had the ability to supplement the LIGs with funds from other sources, if they so wished.

Strategy measures focussed on increasing young people's access both to sex and relationships education (SRE) and to contraception, though specific activity varied considerably across local authorities. Typical projects included employing local teenage pregnancy co-ordinators, opening sexual health clinics aimed at young people (often based in schools), and increasing SRE provision within schools. In some cases, local authorities also provided emergency contraception to young people without prescription, free of charge at pharmacies (Girma and Paton, 2014). Legal rights to reproductive health services were not affected by the Strategy. For example, abortions continued to be available free of charge to teenagers via the National Health Service (NHS).

Local Implementation Grants were initially ring-fenced for teenage-pregnancy related expenditure, but in 2008-09 this restriction was removed completely.[3] LIGs were last disbursed in 2010-11, and after this point, funding for public health projects was included in a general grant from central government. These changes coincided with the financial crash of

[2] The size of the grants did not always change year-on-year. For instance, the amounts given to each local authority were the same from 2004/5 to 2010/11.

[3] Ring-fencing was removed somewhat earlier for a sub-set of high-performing local authorities.

2008 and with the fiscal tightening which followed. Post-crash, total grants to local authorities from central Government were reduced significantly, forcing areas to review and cut expenditure. As a result, most local authorities implemented cuts to teenage pregnancy-related projects, but there was considerable heterogeneity in timing and extent. For example, in 2011, the Guardian newspaper reported that many local areas (e.g. Walsall and Tameside) had removed their teenage pregnancy co-ordinators whilst others (e.g. Liverpool and Enfield) had retained them.[4] The newspaper also highlighted that many areas were cutting sexual health advice services (including no-cost contraception) amongst other projects. Typically, this has meant fewer places where teenagers can access contraception, reduced opening hours, less advertising of services, less training for sex education in schools, and so on.

Politicians (Education Select Committee, 2010) and organisations working in the field (Billingsley, 2011) put out strong warnings that these cuts would put at risk the reductions in teenage pregnancy rates that had already been observed. The argument was that cuts to services would increase the barriers to teens accessing birth control which would consequently increase the number of teenage pregnancies.

Despite these warnings, there are arguments to suggest that the impact on teenage pregnancy may not be as bad as feared and, indeed, that spending on projects relating to teenage pregnancy may even be counterproductive. Wiggins et al. (2009) evaluate a comprehensive intervention given to at-risk youths (including SRE and access to family planning services) which is typical of measures recommended by the Teenage Pregnancy Unit. They find significantly higher rates of teenage pregnancy among the intervention group, as well as increases in a number of other adverse outcomes. This adds to a large body of research showing that teenage pregnancy initiatives may have unintended effects on risky sexual behaviour which could counteract or even outweigh positive effects on teenage pregnancy. For example, a number of authors have found that access to birth control has only limited effects on most measures of fertility (Raymond, Trussell and Polis, 2007; Girma and Paton, 2006; 2011; 2014; Paton, 2002).

Akerlof et al. (1996) provide a theoretical basis for these outcomes: easier access to birth control may induce increases in sexual activity amongst teens. As methods of birth control are not fail-safe (particularly so amongst teenagers [Kost et al, 2008]), the net impact on pregnancy rates is ambiguous. Put simply, birth control will reduce the risk of pregnancy for sex acts which would have occurred anyway but may increase the risk amongst teenagers

[4] www.theguardian.com/society/2011/aug/09/cuts-undo-progress-teenage-pregnancies

who are induced by easier access to birth control either to start having sex or to have sex more frequently. This has been tested empirically by numerous authors using data on sexually transmitted infections (infections should rise if risk-taking is greater). Girma and Paton (2011), Durrance (2013) and Mulligan (2016) all find that greater access to emergency birth control can induce an increase in sexual risk taking. Similarly, Klick and Stratmann (2007; 2003) and Klick, Neelson and Stratmann (2012) find that access to abortion is associated with an increase in risk taking behaviour, although Coleman, Dee and Joyce (2013) argue that these effects are partly due to misreporting of data.

Depending on the extent to which unintended consequences occur in practice, it is possible that spending by local public sector managers aimed at reducing teenage pregnancy will not achieve the desired outcome. Further, enforced spending cuts may provide managers with the opportunity to exercise more discretion to fund only those projects which have a stronger evidence base. Thus it is possible that, at the margin, spending cuts do not have the effect of increasing teenage pregnancy rates and, if unintended behavioural effects are very strong, could even lead to decreases in rates.

Research on the links between general expenditure (rather than specific measures) and teenage pregnancy outcomes is very limited. Wilkinson et al. (2006) find that local authority expenditure in the early years of the Teenage Pregnancy Strategy was significantly correlated with reductions in conception rates, although improvements in specific measures such as contraception services and sex education were not found to have reduced conception rates at a local level. Blackman (2013) finds that local "dedicated planning to tackle high teenage conception rates appears to make things worse" (p.69), though does not use data on monetary expenditure.

More recently, Wellings et al. (2016) find that the total amount received in LIGs over the Teenage Pregnancy Strategy is associated with overall reductions in conceptions rates over that period. However, inferring a causal relationship from this is difficult. In the first place, LIGs were largely determined by pre-existing teenage pregnancy rates and, given the very limited of variation over time in LIGs, it is impossible to separate out a causal effect of funding from other contemporaneous changes. Second, the size of the grant received does not equate to actual expenditure, particularly in the latter period of the Strategy when grants were no longer ring-fenced. A key advantage of the approach in this paper is the use of local data on actual spending related to teenage pregnancy. Differences in the level and timing of cuts to this expenditure provides a key source of identification of a causal effect of spending on pregnancy rates.

Figure 1 illustrates annual LIG allocations and, from 2009, local expenditure on teenage pregnancy initiatives, as well as annual rates of under-18 conceptions and abortions from 1998 until 2014. From a peak in 2004, local expenditure decreased steadily until 2011 and more dramatically thereafter. Between 1999, when the Teenage Pregnancy Strategy was announced, and 2007 (just before the financial crash) there was very little change in conception or abortion rates. From 2008, both series show a rapid downward trend, one that has continued even as expenditure has also decreased.

Table 1a provides some more information on the local expenditure series available between 2009 and 2014. Comparing the 2012-2014 period with 2009-11, 10% of authorities reported increased spending on teenage pregnancy projects. The rest all reported decreased spending, many by a very significant amount. For example, over 30% of authorities reported cuts to expenditure in excess of 80%.

The absence of any obvious correlation at an aggregate level does not necessarily mean that cuts in expenditure had no impact on pregnancy rates. It is well-established that socio-economic factors such as poverty, economic welfare and education are significant determinants of teen conception rates and changes in such factors may be obscuring the impact of expenditure cuts at an aggregate level. For example, Girma and Paton (2014) demonstrate that improvements in school education played a significant role in the fall in conception rates up to 2012, whilst Blackman (2013) highlights the important role of race. For this reason, the question we address in this paper is whether those local authorities which made bigger cuts to expenditure on sexual health services have experienced bigger or smaller reductions in conception rates. To this end, the main focus of the paper is an empirical analysis of the effect of cuts in local teenage pregnancy expenditure on teenage pregnancy rates. By using annual data across a range of local authorities, we are able to control for factors which are time-specific and for factors which are specific to local authorities. We are also able to control for a range of other factors, such as education and race, which may obscure the relationship between expenditure and conceptions.

3. Empirical Methodology

The basic empirical approach we adopt is to estimate a fixed effects panel data regression model. Our starting point is the following equation:

$$Y_{it} = \beta_0 + \beta_1 EXP_{it} + \gamma X_{it} + D_t + \mu_i + \varepsilon_{it} \qquad (1)$$

where i and t represent an index of local authorities and years, respectively. In equation (1), Y_{it} is under-18 conception (or abortion or birth) rate, EXP_{it} is the annual expenditure per 15-17 year old female spent by the local authority on teenage pregnancy services, X_{it} is a vector of socioeconomic control variables, D_t is a vector of year dummies, u_i is a vector of local authority specific dummies, and ε_{it} is the residual error term. We estimate standard errors robust to heteroskedasticity, to contemporaneous cross-sectional correlation and to first order serial correlation.

The incorporation of year and area dummies controls for average unobservable effects on conception rates which are specific to particular years and to particular local authorities, respectively. This approach helps to get around two problems which might induce a spurious correlation between expenditure and pregnancy rates. First, we may observe 'policy endogeneity' in that areas with high conception rates are precisely those in which expenditure on teenage pregnancy are also likely to be higher. Second, both expenditure and pregnancy rates may move in the same direction due to other, unobservable factors. In the fixed effects model presented in equation (1), a negative value for β_1 would indicate that local authorities which cut expenditure more (less) than average saw smaller (bigger) decreases in teenage pregnancy than average.

Even in the fixed effects model, we need to be careful before attributing causality to any significant correlation found between expenditure and conception rates. In the first place, there may be unobservable trends over time which are correlated with both variables and which induce spurious correlation. To deal with this possibility, we estimate models using conceptions (and births and abortions) to older women as a control. We estimate the effect of expenditure on under-18 conceptions, relative to any effect on the older group. If expenditure has a causal effect on conceptions, we would expect to observe a significant effect for under-18s but not for older women. We use women aged over 24 as our control group to avoid contamination from any teenage pregnancy services which are also open to young people over the age of 18.[5] To estimate the relative effects, we run regressions on pooled data with under-18 and older conception rates for each local authority. These regressions include fixed effects for year/age group and authority/age group combinations. Each variable is included along with an interaction effect with a dummy variable for under-

[5] Although all local authorities had the under-18 conception rate as a key target, the age limit for accessing services aimed at young people varies considerably across (and sometimes within) areas. For example, Brook, who run many projects funded under the Teenage Pregnancy Strategy, advertise their services to any young person under the age of 25.

18s. The coefficients on the interaction terms provide us with the relative effects for each variable.

Next, we consider the possibility of policy endogeneity in which decisions over funding cuts are correlated with factors which also affect changes in teenage conceptions. Decisions over the extent and timing of cuts to teenage pregnancy services are likely to have been affected by the level of overall cuts imposed by central Government (which varied by local authority), the perceived effectiveness of such expenditure and the political will of local elected representatives who eventually sign off on expenditure. For any effect of expenditure to be causal, these factors must have been largely exogenous to conception rates. We conduct a number of experiments to explore this.

Taking first the influence of party politics, Labour and Liberal Democrat controlled authorities are generally thought less likely to target teen contraceptive services for cuts than Conservative controlled authorities.[6] If the former are areas which for other reasons are less likely to experience cuts in conception rates, then this could induce a spurious correlation between spending and conceptions. We allow for this firstly by including dummy variables for political control in our set of control variables. More substantively, we also test whether the coefficient on expenditure varies according the controlling party.

The perceived effectiveness of spending is likely to be influenced by recent trends in conception rates. For example, it may be harder to defend against cuts to teen pregnancy projects in areas in which conception rates have already decreased significantly. If conception rates were in fact decreasing due to some unconnected but ongoing trend (related to, say, demographic change), we might observe a relatively big cut in spending but also continued decreases in conception rates. To control for this, we test whether the expenditure coefficient varies for areas with relatively big recent decreases in conception rates. We also test whether the effect of expenditure varies for authorities whose LIG budgets were released from the ring-fence only from the start of our sample on the grounds that the ring-fence may have caused a differential pattern of spending cuts.

More formally, we also estimate an instrumental variable regression in which *Exp* is treated as endogenous and instrumented by a number of potential instruments, including total budget size.

[6] However, it should be noted that ideological differences between political parties in the U.K. on policies relating to sex education and abortion are much less significant than, for example, in the U.S.

A further issue with equation (1) is the possibility of teenagers accessing services across local authority borders. Our estimates implicitly assume that the effect of teenage pregnancy service expenditures is confined to pregnancy rates within the local authority. This is unlikely if the cost of travel between local authorities is low. As a result, cuts in one local authority may appear to have little effect on pregnancy rates if young people can avail themselves of teenage pregnancy services in neighbouring authorities (Girma and Paton, 2014). We consider several experiments aimed at dealing with this issue.

We also undertake a series of additional robustness checks including: using dynamic panel data estimates which include the lagged dependent variable (suitably instrumented using the Arellano and Bond, 1991, approach) to control for the possibility that expenditure decisions are affected by previous years' conception rates; including area- or region-specific trends; allowing for multi-way clustering as suggested by Cameron, Gelbach and Miller (2010); using a log-linear specification; and using the random effects estimator which utilises variation across local authorities to identify results and, hence, may be better able to pick up effects of co-variates which have limited 'within' variation.

4. Data

Our units of observation are 149 higher tier local authorities in England. The primary outcome used in our empirical work is the under-18 conception rate among residents in each upper tier local authority (*U18CR*). Pregnancy data in England are of high quality relative to many other countries as there are legal requirements for the reporting of live births and abortions. The Office of National Statistics (ONS) provides estimates of the time of conception in order to generate annual conception rates for each local authority in the country by age at conception. The ONS also break down the data into conceptions ending in abortion and those ending in live births.[7]

The data are available by calendar year. Conceptions rates are calculated as the number of pregnancies ending in live birth, stillbirth or induced abortion occurring to any woman aged under-18 at conception per 1,000 women aged 15-17 and resident in each local authority. As there is an interval between a conception and its recorded outcome, data on

[7] The number of unrecorded illegal abortions can be assumed to be negligible as abortions are provided confidentially and for free to under-18s in England. However, one limitation with the data is that miscarriages are excluded and there is some evidence that economic conditions can affect the rate of miscarriages (Bejenariu and Mitrut, 2013). We rely on our socio-economic measures such as unemployment to control for such effects.

conception rates are only released about fourteen months after the end of a calendar year. This means that our sample ends in 2014. Given this, we have 894 observations.

We also estimate models using under-18 abortion and birth rates as the outcome variables (*U18AR* and *U18BR*). As Paton (2002) notes, abortion rates can be considered a good proxy for the overall rate of unintended pregnancies as it is assumed that the majority of planned teenage pregnancies will lead to birth. Given a major focus of the Teenage Pregnancy Strategy was on reducing the likelihood of accidental pregnancy through advocating better contraceptive use, it is thus interesting to see the effect teenage pregnancy expenditures had on abortion rates. Data for under-18 abortion and birth rates are available from the ONS, again based on estimated age and year at conception.

Data on teenage pregnancy service-related expenditure (*Exp*) are provided by the Department for Education from the Outturn data series of local authority expenditure. These are available from 2008-09 (the first year the Local Implementation Grant was non-ring fenced for all authorities). We measure expenditure as a rate per thousand female residents aged 13-17 and deflate using the ONS's Consumer Price Index (2014=100).[8] Unlike conception and abortion data, teenage pregnancy expenditures are only recorded by financial year which runs from 1 April to 31 March. It is likely that the full effect of expenditure on conceptions will not be immediate. For instance, providing long acting reversible contraceptives may prevent pregnancies that would have only occurred over the medium-term. Hence, we allocate expenditure to the second year in which it is reported – i.e. expenditure in 2008-09 is coded as 2009. Our results are robust to using alternative lag specifications such as including both current and lagged expenditure figures, using weighted averages to estimate calendar year expenditure and including a lagged dependent variable.

A particular issue with the expenditure data is the presence of a significant number of zero entries. Although the Department for Education confirm that zeros represent true reported values[9], we do not exclude the possibility that some instances represent accounting errors. For this reason, we include in our robustness checks alternative ways of dealing with zero values. The first is to omit any observations in which expenditure is recorded as zero in one year but followed by a positive value in a subsequent year. The rationale for this approach is that spending cuts over time mean zero reported values in later years are much more likely to indicate true zero expenditure rather than accounting errors. The second

[8] The results below are robust to using alternative deflators, e.g. 15-17 or 15-19 year olds.
[9] Personal correspondence, 4 April 2015.

approach is to omit local authorities with *any* zero expenditure values over the period. It is possible that re-coding true zeros to missing will bias the results in a different way to including zeros which are actually reporting errors. Reassuringly, however, the key results are robust to these alternative approaches.

To control for possible confounding trends, we add a number of variables as controls in the econometric models. In the first place, we include local authority expenditure on all services aimed at young people (*Exp young*) and also specifically on alcohol and drug prevention services for young people (*Exp alcohol*). The former will help to control for, amongst other things, access to youth workers and recreational services which have been found to be correlated with teenage pregnancy (Akers, Muhammed and Corbie-Smith, 2011). The latter represents another area of public health which has also been targeted for cuts over the period under study. Including these measures may control for any spurious correlation between teenage pregnancy expenditure and conceptions which is, in reality, due to more general cuts caused by the economic downturn.

Trends in education, ethnic change, and alcohol consumption may explain changes in pregnancy rates. We measure education using a three-year rolling average of the percentage of students in the final year at state-funded secondary schools who attain five GCSEs at grade A*-C including English and Mathematics (*GCSE*). This is one of the main indicators of performance at the standard school leaving age in England. We use three-year rolling averages to minimise random fluctuation and better represent attainment by 17 year olds, who make up approximately 50% of all under-18 pregnancies.

As census-based data on race or ethnicity are not available on any consistent basis, we measure the ethnic make-up in a local authority using the percentage of final year pupils at state-funded secondary schools who are classified as non-white (*Non-White*).

Although alcohol is known to be a significant factor affecting adolescent decision-making, there is little or no data available at the local authority level which directly measures alcohol consumption by teenagers. Here we include data from Public Health England on the rate of under-18s admitted to hospital with alcohol-specific conditions per 100,000 of the resident population (*Alcohol*). This should act as a proxy for types of alcohol consumption – in particular, binge drinking – that are particularly associated with reduced inhibitions and myopic behaviour and which are likely to be behind any association between alcohol and teenage pregnancy (Rashad and Kaestner 2004).

In line with findings from other papers studying the determinants of teenage pregnancy, we also include in the econometric models controls for the level of deprivation,

unemployment rate and the level of family breakdown.[10] We measure unemployment as the proportion of 16-24-year old females who claim joblessness-related benefits (*Fem Unem*). We measure family breakdown as the annual proportion of children of each age group in local authority care (*Care*). To measure deprivation we use the percentage of children in a Local Authority who live in workless households (*Workless*). Children placed in care are known to be at an enhanced risk of adverse outcomes in a range of areas including low educational attainment, substance abuse, early sexual activity and teenage pregnancy (Social Exclusion Unit, 1999). High levels of youth unemployment and deprivation imply fewer workplace and other opportunities and, consequently, a lower opportunity cost of early pregnancy. As a result, we expect all three measures to be positively related to teenage pregnancy rates.

Our key results are robust to the inclusion or omission of the various control variables. Summary statistics for each variable are presented in Table 1b. Full descriptions and sources are in the Appendix.

5. Empirical Results

5.1 Baseline Estimates

The baseline panel fixed effects data results are reported in Table 2. In the first half of the table, we report estimates of the effect of expenditure on each of conception, abortion and birth rates for under-18s. The coefficients on expenditure are positive and statistically significant. In other words, controlling for a range of other factors including unobservable area and time effects, areas which have put in place relatively large cuts to teenage pregnancy expenditure have seen significantly bigger reductions in teenage conception, abortion and birth rates. The estimated effects are fairly small. Evaluated at the mean of the variables, a 10% reduction in expenditure is associated with a decrease of about 0.25% in the under-18 conception rate, 0.19% in the abortion rate and 0.32% in the birth rate.[11]

Looking at the control variables, better school performance is associated with significantly lower conception, birth and abortion rates amongst teens. In contrast to local authority spending, the estimated impact is large: a 10% improvement in GCSE results (again

[10] For evidence on all three factors on teenage pregnancy rates see Imamura et al. (2007). Due to a lack of local authority level data, we are unable to add a number of other controls which may have been significant or related to teenage pregnancy expenditures. For instance, it would also have been useful to control for media consumption patterns (see Collins et al. 2004) and, more speculatively, internet usage (Girma and Paton 2014).

[11] This is calculated as follows. A 10% reduction spending at the mean is £1.956 per capita. Using the marginal effect of 0.039 gives an estimated increase in the conception rate of 0.0752 (= 0.039*1.956). In turn this represents an increase of 0.25% from the mean conception rate of 30.93 per thousand girls.

estimated at the mean) is associated with a reduction in the under-18 conception rate of over 10%. As expected, higher alcohol use is generally associated with higher conception rates. Regarding political control, Labour controlled authorities appear to have (conditionally) lower conception and abortion rates than authorities controlled by other parties, although this result is not especially robust to some of the alternative specifications.

In the second half of the table, we report the effect on conception, abortion and birth rates for under-18s relative to older women (over-24). This specification helps to controls for unobservable trends which may lead to a spurious correlation between expenditure and teenage pregnancy. There are changes in statistical significance of some of the control variables (e.g. *GCSE*). However, the estimated effects on conceptions, abortions and births continue to be strongly statistically significant and, indeed, a little larger in magnitude than in the baseline model.

5.2 Robustness Checks

We go on to consider a series of alternative specifications to the fixed effects model to explore how robust are the estimates of the effect of expenditure cuts. For reasons of space, we only report the results for conceptions and for the under-18s only model. In general, the results of the robustness checks are very similar for abortions and births and for the pooled model.[12]

In Table 3, we report on attempts to control for policy endogeneity as discussed in section 3 above. In column (1), we include interaction terms between expenditure and the political party in control of the local authority. In column (2), we allow for a differential effect between areas which have seen relatively large and small decreases in conception rates (compared with the median) in the years prior to the start of our sample. In column (3), we allow for different effects for those areas where teenage pregnancy spending was ring-fenced until 2008-9. In each experiment, the baseline effect of expenditure on conception rates holds up well. Expenditure has a positive effect on conceptions whichever political party is in control. The effect is strongly significant for Conservatives, Liberal Democrat and Labour controlled authorities. The effect is estimated to be insignificant for authorities with no overall control, though we should note this estimate is based on relatively few such cases.

[12] In a few cases, the effect of expenditures in the specifications for abortion or births alone are reduced in significance. The full results are available from the authors on request.

We find no significant difference in the effect of expenditure for areas which have seen a relatively large prior decrease in under-18 conceptions or for those areas whose expenditure was ring-fenced up to 2007-8.

In the final column (4) of Table 3, we report instrumental variable GMM estimates in which expenditure is treated as endogenous to conceptions. We use as instruments previous changes in conception rates, total expenditure on all services by the local authority and the proportion in the population aged over-65. The rationale for using previous changes in conceptions is that areas which have already achieved large reductions in teenage pregnancy may be more prepared subsequently to cut expenditure. The proportion of older people in the population is included as a significant proportion of statutory local authority spending is related to care of the elderly. Relative increases in the elderly population are likely to put additional pressure on discretionary areas of expenditure such as on teenage pregnancy. Similarly, total expenditure identifies local authorities with bigger external constraints (typically imposed by central government) on all spending and, as such, is likely to be an exogenous determinant of teenage pregnancy expenditure.

In this regression, the coefficient on expenditure remains positive and strongly statistically significant. Diagnostic tests suggest that the model is identified and the instruments valid, albeit somewhat weak. The Anderson-Rubin test, which is robust to the presence of weak instruments, continues to indicate that the coefficient on expenditure is statistically significant.[13]

In Table 4, we further explore the influence of unobserved trend effects on our key relationship. We do this in two ways. In column (1), we include separate linear trends for each local authority. This approach controls for changes in other factors over time which are specific to a local authority but which could help to explain changes in conception rates. An example might be a trend for inward migration by particular groups characterised with higher or lower teenage pregnancy rates and which affects some areas more than others. With this specification, the coefficient on expenditure, although still positive, is only significant at the 10% level. The implied magnitude of the effect is also lower than in the baseline, with a 10% reduction expenditure now estimated to cut conception rates by only 0.09%.

Because this approach involves including 149 different trend terms, it is very demanding of the data and tests may suffer from low statistical power. For this reason, in

[13] The effect on expenditure is robust to using different specifications, for example using the political control dummies as instruments rather than independent co-variates. However, the diagnostic tests suggest that weak instrumentation is even more of a problem.

column (2) we report a less-demanding approach which includes region-specific trends. Another way of controlling for pre-existing trends is to include lagged changes in conception rates as an additional regressor and we report this approach in column (3). The variable measuring lagged changes (*Pre-trend*) is defined as the three-year moving average of lagged growth rates in teen conceptions where the most recent period (t-1) is given a weight of 1/2 with periods t-2 and t-3 having weights of 1/3 and 1/6 respectively. In each of these alternative specifications the coefficient on expenditure is very similar to that in the baseline model.

In column (4), we report dynamic panel data estimates which incorporate a lagged dependent variable to control for dynamic effects of variables on pregnancy rates. We employ the Arellano and Bond (1991) estimator which treats the lagged dependent variable as endogenous. The size of the estimated effect of expenditure on pregnancy rates is very similar to that in the baseline models and still strongly significant.

We further explore dynamics by including both the lag and lead of expenditure. These results are reported in column (5) of Table 4. All three coefficients are positive but only the coefficient on current expenditure is statistically significant.

In the final column of Table 4, we use standard errors which allow for multiway clustering (in this case, by both year and area) as suggested by Cameron, Gelbach and Miller (2010). The results are very similar to the baseline, although the statistical significance of the expenditure variable is somewhat reduced.

Further robustness checks are reported in Table 5. The first two columns report results with alternative approaches to dealing with authorities reporting zero expenditure as discussed above. The estimated effect of expenditure in these specifications is stronger than in the baseline, although the implied size of the effects continues to be relatively small. For example, when we only include authorities reporting no zero expenditures, a 10% reduction in expenditure is associated with a cut in the under-18 conception rate of 0.52%.

An important possibility to consider is that adolescents resident in one local authority may be accessing services in a neighbouring borough. In this case, the impact of cuts on residents is likely to be muted. We can think of this as a measurement error problem for our expenditure variable and, if the problem is significant, our estimates may be biased. We explore the importance of this issue in two ways. First (Table 5, column (3)), we adjust the expenditure variable using the number of young people in each area attending secondary

schools in another authority to control for ease of access to cross-border services.[14] Second (Table 5, column (4)), we exploit new data which reports the percentage of teenage family planning clinic users in each local authority who are not residents of that area. We exclude any local authority in which this percentage is greater than 30%. In each case, the coefficient on expenditure is somewhat larger than in the baseline model and still strongly significant.

In Table 5, column (5), we report a log-linear specification which produces very similar results to the linear model. Finally, in column (6) we report random effects estimates. The lack of statistical significance for some of the socio-economic variables in the fixed effects models is not surprising given the limited variation within local authorities over time reported in Table 1b. Using the random effects estimator allows variation in factors across local authorities to identify the estimates. As a result, statistical tests are likely to be more powerful (in the sense of being more likely to pick up true effects as significant), albeit at the risk of inducing bias into the estimates due to not controlling for unobservable local authority-fixed effects. More of the co-variates now have coefficients which are statistically significant and of the expected signs, whilst the coefficient on expenditure is still positive and strongly significant.[15] Hence, the random effects results give us some reassurance that the model is reasonably specified.

Taken together, the various experiments suggest that the baseline results are robust to a range of different samples, econometric approaches and specifications. In every case, reductions in expenditure on teenage pregnancy services are associated with reductions in teenage pregnancies. In the vast majority of specifications, the estimated effect is strongly statistically significant.

6. Discussion and concluding remarks

The results in this paper suggest that, in contrast to predictions by politicians and organisations working in the field, local areas which imposed bigger cuts to projects aimed at

[14] Specifically, we set expenditure on residents of a given local authority equal to the sum of expenditure by each local authority weighted by the proportion of pupils they comprise in each local authority (i.e. proportion of pupils in local authority B from local authority A). This should proxy well for cross-border access of services as many Teenage Pregnancy Strategy projects were delivered in schools and catchment areas for secondary schools in England can cross local authority boundaries.

[15] One apparent anomaly is that female unemployment is estimated to have a negative (albeit insignificant) impact in the fixed effects model but significantly positive one in the random effects model. The difference can be reconciled by noting that the fixed effects model is driven by changes in unemployment over time whilst the random effect model is driven also by average differences in unemployment across local authorities. So the latter result reflects the fact that more deprived areas tend to have relatively high teenage pregnancy rates whilst the former reflects that, a marginal increase in the unemployment rate in any area reduces the resources available to spend on children and, hence, reduces the number of births.

reducing teenage pregnancy have, on average, experienced faster decreases in teenage pregnancy rates.

These findings need to be interpreted cautiously. In the first place, although the effects are statistically significant, the estimated magnitude of the impact on conception rates is small. Further, it should be noted that our empirical approach estimates the *marginal* effect of changes in funding to services which are co-ordinated and targeted at teenagers. If funding cuts are implemented in an efficient manner, then the least effective projects should be cut first. It may still be the case, for example, that if all funding for sexual health services were cut, this would cause teen pregnancy rates to increase. Moreover, our analysis does not directly test the effect of cuts to more general services (e.g. those provided by General Practitioners) which are also available to teenagers.

That said, the results here are consistent with previous work suggesting that changes to service access can induce behaviour change amongst at least some teens. To the extent that more difficult access to contraception caused a reduction in sexual risk taking, this may have alleviated any adverse effects of spending cuts. Another, perhaps complementary explanation is that reduced public subsidies for adolescent services may have led to a switch to alternative services, whether publicly or privately funded.[16] In any case, our results suggest that public sector managers facing budgetary pressures need to consider the possibility that behavioural consequences induced by budget cuts have the potential to ameliorate some or all of the direct negative consequences of the cuts.

The panel data approach used in this research has several benefits. Most importantly it allows us to control for time-specific and area-specific effects. An area which is characterised by relatively high rates of teenage pregnancy may wish to spend more money in trying to alleviate the problem. In this event, a cross-sectional analysis would be likely to report a spurious positive correlation between spending and pregnancy rates. By controlling for area-specific effects, we focus specifically on whether relatively large cuts in expenditure are associated with smaller or larger decreases in pregnancy rates. However, this does not entirely solve potential problems of endogeneity. For example, areas in which pregnancy rates are already decreasing, may be more likely to choose to re-allocate spending to other projects. Similarly, decisions over funding cuts might be determined by factors such as

[16] Ideally, we would test this directly using sexual transmitted infection data in a similar way to Klick, Neelson and Stratmann (2012) who assess whether results for changes in abortions and infections are of consistent sign and magnitude following abortion policy changes. Unfortunately, suitable data on sexually transmitted infections at a local authority level are not available in England.

political control which also effect pregnancy rates. The fact that our results are robust to alternative specifications which control for political control, for pre-existing trends in pregnancy rates and which treat spending outcomes as endogenous gives some reassurance on these points and suggests that, at the margin, local expenditure cuts may have contributed to the continued fall in teenage pregnancy rates. At the very least, our results provide no evidence that cuts in expenditure to date have led to increases in teenage pregnancy rates.

Another caveat to our results is that the dynamics of the effects of spending cuts are difficult to model and that adverse effects of cuts may only be felt in the longer term. However, against this, Arcidiacono et al. (2012) find evidence that contraceptive access is less likely to reduce unwanted pregnancy rates in the long run than the short run due to the differential impact on those who are already in a sexual relationships (who are more likely to carry on engaging in sexual activity but without using contraception) compared to those who are not yet in sexual relationships and who are less likely to make a future transition to sexual activity when contraceptive access is more difficult. A fuller analysis of the long run effects of cuts on teenage pregnancy will be an important task for future research as more data become available.

It is also unclear to what extent the finding that cuts in public expenditure can be achieved without adversely affecting measures of social wellbeing can be generalised. Contrasting evidence is found by Loopstra et al. (2016) who conclude that cuts to local authority expenditure have contributed to an increase in homelessness. The beneficial effects of cuts revealed in our paper may be due to the particular circumstances and decision-making process surrounding teenage fertility. Exploring whether there are more general lessons to be learnt will be a useful exercise for future research.

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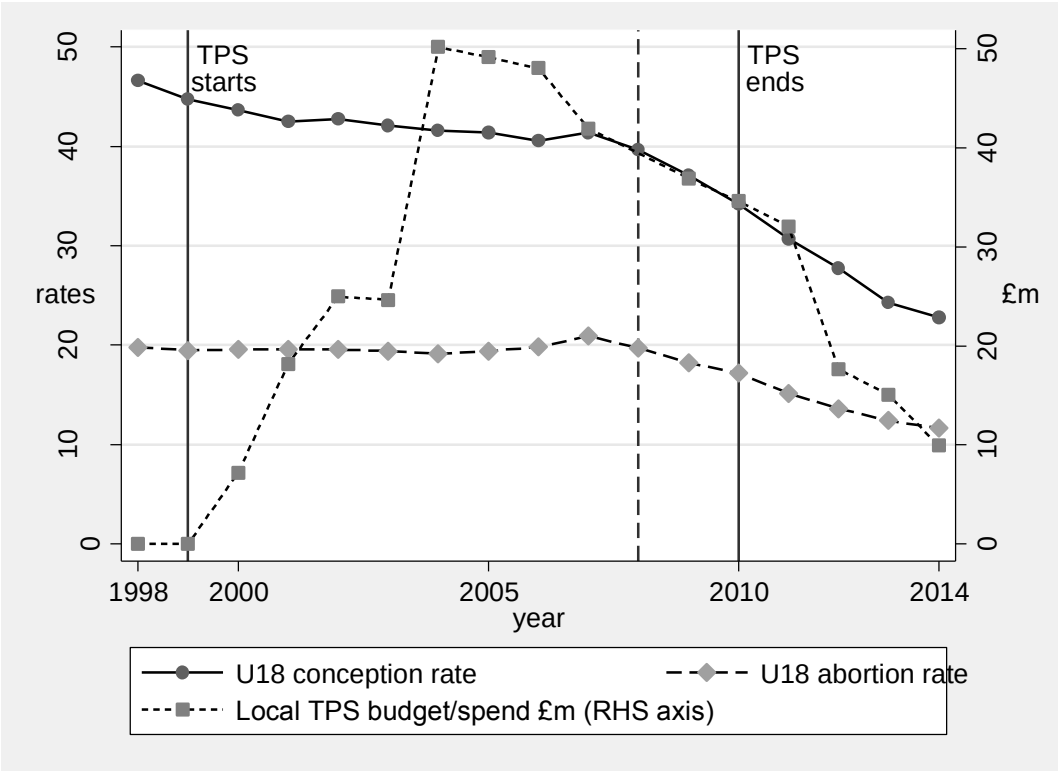
Figure 1: Trends in U18 conception and abortion rates and local budget/spend



Notes:
(i) Local Teenage Pregnancy Strategy (TPS) budget/spend is derived from two series. Up to 2008, it is based on the Local Implementation Grant (LIG) allocated to local authorities. From 2009 on, it is the reported actual expenditure by local authorities. Both series are adjusted for the ONS consumer price index (2014 = 100). The LIG series is adjusted based on the values for both series which are held for 2009. (The dashed vertical line indicates the point where the two series are spliced together.) Budgets/spends are allocated to the calendar year in the second half of the financial year for which figures are reported.
(ii) The solid lines indicate the start and end of the Teenage Pregnancy Strategy (TPS)
(ii) Conception and abortion rates are numbers of girls aged under 18 (based on estimated age at conception) per thousand females aged 15-17.

Table 1a: Summary statistics: per capita teenage pregnancy expenditure by local authority

	Annual expenditure					
	2009	2010	2011	2012	2013	2014
Mean	29.28	27.03	25.35	15.15	12.59	7.96
SD	26.83	24.99	21.62	17.04	18.11	11.80
	Change 2009-11 to 2002-14					
Local authorities:	Increase	cut ≤ 20%	20 < cut ≤ 40%	40 < cut ≤ 60%	60 < cut ≤ 80%	80 < cut ≤ 100%
Number	15	7	21	34	25	47
Percentage	10.7%	5.4%	14.1%	22.8%	16.8%	31.5%

Notes:
(i) Figures are the expenditure (in £) on teenage pregnancy initiatives reported by 149 local authorities per female aged 13-17. Means and standard deviations are unweighted by population.
(ii) Expenditure is adjusted for the ONS consumer price index (2014 = 100).

Table 1b: Summary statistics

Variable	Mean	Standard Deviation			N
		Overall	Between	Within	
U18CR	30.93	10.30	8.19	6.27	894
U18AR	15.48	4.75	3.48	3.25	894
U18BR	15.45	6.99	6.00	3.61	894
Exp	19.56	22.14	16.38	14.95	894
Exp (no early zeros)	21.17	22.28	16.57	14.73	826
Exp (only authorities with no zeros)	24.37	21.29	17.41	12.39	438
GCSE	56.51	6.47	5.62	3.24	894
Fem Unem	3.56	1.46	1.31	0.66	894
Workless	16.11	7.00	6.24	3.20	894
Non-white	22.35	22.23	22.23	1.65	894
Alcohol	626.36	286.38	264.35	111.91	894
Care	44.01	16.97	16.39	4.57	894
Conservative	0.38	0.49	0.44	0.20	894
Labour	0.38	0.49	0.41	0.26	894
Lib Dem	0.03	0.17	0.14	0.11	894
Exp young	2975.1	1250.8	1145.0	510.8	894
Exp alcohol	14.99	19.94	14.79	13.42	894

Notes:
(i) Variable definitions are in Table A1 in the Appendix
(ii) Statistics are calculated for 149 local authorities over 2009-2014. Variations in sample size reflect missing observations.
(iii) **"**Between" indicates the standard deviation across local authorities whilst "Within" indicates standard deviation within each local authority.

Table 2: Determinants of teen conception & abortion rates, 2009-2014

	(1)	(2)	(3)	(4)	(5)	(6)
	Under-18s			Under-18s vs older women		
Outcome:	Conceptions	Abortions	Births	Conceptions	Abortions	Births
Exp	0.0390***	0.0148***	0.0250***	0.0540***	0.0133**	0.0412***
	(0.00853)	(0.00527)	(0.00728)	(0.0142)	(0.00639)	(0.0104)
GCSE	-0.476***	-0.250***	-0.235***	-0.172	-0.185***	0.00688
	(0.0974)	(0.0477)	(0.0518)	(0.111)	(0.0487)	(0.0731)
Fem Unem	-0.494	-0.0838	-0.419	-0.356	0.0355	-0.437
	(0.461)	(0.264)	(0.324)	(0.683)	(0.181)	(0.591)
Workless	0.0461	0.0277	0.0193	0.0338	0.0119	0.0221
	(0.0412)	(0.0252)	(0.0236)	(0.0322)	(0.0185)	(0.0273)
Non-White	-0.130	-0.0325	-0.103*	-0.285**	-0.0673	-0.231**
	(0.0928)	(0.0608)	(0.0548)	(0.133)	(0.0571)	(0.0996)
Alcohol	0.00196**	-0.000541	0.00248***	0.00746***	0.00105	0.00640***
	(0.000764)	(0.000491)	(0.000736)	(0.00229)	(0.000849)	(0.00193)
Care	-0.00229	0.0329**	-0.0349**	-0.0283	0.0208	-0.0504**
	(0.0260)	(0.0137)	(0.0174)	(0.0357)	(0.0163)	(0.0217)
Conservative	0.368	0.243	0.168	0.822	0.373	0.495
	(0.436)	(0.312)	(0.252)	(0.630)	(0.345)	(0.458)
Labour	-0.695**	-0.693***	0.0147	-0.822*	-0.597**	-0.219
	(0.282)	(0.214)	(0.253)	(0.485)	(0.299)	(0.402)
Lib Dem	0.357	0.421	-0.0250	1.450	0.350	1.160
	(1.187)	(0.689)	(0.708)	(1.447)	(0.669)	(0.974)
Exp young	-0.000265	1.15e-05	-0.000312	-0.000767**	-0.000116	-0.000666**
	(0.000417)	(0.000225)	(0.000271)	(0.000365)	(0.000209)	(0.000290)
Exp alcohol	-0.00272	0.00147	-0.00420	-0.0172	-0.000748	-0.0175
	(0.00714)	(0.00379)	(0.00615)	(0.0126)	(0.00452)	(0.0111)
Area effects	YES	YES	YES	area-age	area-age	area-age
Year effects	YES	YES	YES	year-age	year-age	year-age
Area trends	NO	NO	NO	NO	NO	NO
Outcome mean	30.93	15.48	15.45	30.93	15.48	15.45
N	894	894	894	1,788	1,788	1,788
N Authorities	149	149	149	149	149	149

Notes:
(i) *** p<0.01, ** p<0.05, * p<0.1.
(ii) Dependent variables are conception, abortion and birth rates for under-18s (based on estimated age at conception) per thousand females aged 15-17.
(iii) Standard errors (in brackets) are robust to panel-level heteroscedasticity, to contemporaneous cross-sectional correlation and to first order auto-correlation.
(iv) In (4)-(6) the coefficients are the relative effect for under-18s compared to the effect on women aged over 24. Specifically, they are the interaction effect between each variable and a dummy for under-18s in regressions which pool under-18s and over-24 conception rates and which include fixed effects for each year/age group and each authority/age group combination. So the relative effect is the difference between the coefficient on *Exp* for under-18s and that for over-24s.

Table 3: Determinants of U18 conception rates, 2009-2014: robustness (1)

	(1) Spending by political party	(2) Big decrease 1998-2008	(3) Ring-fenced	(4) IV-GMM
Exp		0.0463***	0.0418***	0.315***
		(0.0126)	(0.00981)	(0.1000)
GCSE	-0.477***	-0.480***	-0.478***	-0.324**
	(0.0963)	(0.0964)	(0.0985)	(0.130)
Fem Unem	-0.475	-0.483	-0.492	-0.132
	(0.440)	(0.465)	(0.462)	(0.582)
Workless	0.0441	0.0475	0.0468	0.000160
	(0.0421)	(0.0420)	(0.0414)	(0.0598)
Non-White	-0.129	-0.130	-0.130	0.0470
	(0.0952)	(0.0923)	(0.0926)	(0.151)
Alcohol	0.00181**	0.00187**	0.00200***	0.000146
	(0.000791)	(0.000791)	(0.000748)	(0.00256)
Care	-0.00300	-0.000125	-0.00143	0.100*
	(0.0261)	(0.0254)	(0.0254)	(0.0597)
Conservative	0.318	0.352	0.358	0.327
	(0.419)	(0.434)	(0.429)	(0.654)
Labour	-0.918*	-0.713**	-0.727**	0.242
	(0.525)	(0.282)	(0.292)	(0.782)
Lib Dem	-1.091	0.319	0.387	-1.933
	(1.132)	(1.175)	(1.172)	(2.129)
Exp young	-0.000286	-0.000276	-0.000267	-0.00144*
	(0.000414)	(0.000413)	(0.000416)	(0.000858)
Exp alcohol	-0.00220	-0.00238	-0.00329	-0.0278
	(0.00738)	(0.00708)	(0.00750)	(0.0240)
*Exp*Conservative*	0.0336***			
	(0.0121)			
*Exp*Labour*	0.0416***			
	(0.0140)			
*Exp*Lib Dem*	0.0676***			
	(0.0161)			
*Exp*NOC*	0.0294			
	(0.0191)			
*Exp*big decrease*		-0.0128		
		(0.0144)		
*Exp*ring-fenced*			-0.00831	
			(0.0177)	
Area effects	YES	YES	YES	YES
Year effects	YES	YES	YES	YES
Area trends	NO	NO	NO	NO
Outcome mean	30.93	30.93	30.93	30.93
N	894	894	894	894
Authorities	149	149	149	149

Notes:
(i) *** p<0.01, ** p<0.05, * p<0.1
(ii) Dependent variable is the under-18 conception rate (based on age at conception) per thousand females aged 15-17.
(iii) Standard errors (in brackets) in (1)-(3) are robust to panel-level heteroscedasticity, contemporaneous cross-sectional correlation and first order auto-correlation. Standard errors in (4) are robust to heteroscedasticity and auto-correlation.
(iv) The specification in (2) includes an interaction term between expenditure and those authorities with a (relative) decrease in under-18 conceptions bigger than the median. In (3) the interaction term is between expenditure and those authorities for which expenditure was ring-fenced up until 2008/9.
(v) Instruments for *Exp* in (4*)* are percentage of population over age 65, changes in U18 conceptions between 1999 and 1997 and total budget for all services for each council per 1000 total population, adjusted for the ONS consumer price index.

Table 4: Determinants of U18 conception rates, 2009-2014: robustness (2)

	(1) Area trends	(2) Regional trends	(3) Lagged trends	(4) DPD model	(5) Lag/lead expenditure	(6) CGM standard errors
Exp	0.0142*	0.0364***	0.0395***	0.0325**	0.0429***	0.0390**
	(0.00735)	(0.00840)	(0.00856)	(0.0141)	(0.00777)	(0.0163)
Exp (t-1)					0.00898	
					(0.00548)	
Exp (t+1)					0.00539	
					(0.0128)	
GCSE	-0.0966*	-0.465***	-0.482***	-0.407**	-0.457***	-0.476***
	(0.0512)	(0.101)	(0.0939)	(0.162)	(0.115)	(0.141)
Fem Unem	-0.682**	-0.132	-0.486	-0.506	0.00650	-0.494
	(0.316)	(0.422)	(0.449)	(0.571)	(0.453)	(0.688)
Workless	0.00207	0.0369	0.0484	0.0529	0.0268	0.0461
	(0.0267)	(0.0448)	(0.0415)	(0.0605)	(0.0341)	(0.0640)
Non-White	-0.0741	-0.0939	-0.139	-0.167	0.0337	-0.130
	(0.0960)	(0.0953)	(0.0945)	(0.151)	(0.0927)	(0.204)
Alcohol	-0.00276*	-0.00370***	0.00199***	0.00187	0.00155**	0.00196
	(0.00151)	(0.00119)	(0.000758)	(0.00236)	(0.000773)	(0.00219)
Care	0.115***	0.00279	-0.000830	-0.0533	-0.0157	-0.00227
	(0.0269)	(0.0231)	(0.0248)	(0.0654)	(0.0223)	(0.0441)
Conservative	0.523	0.552	0.314	-0.814	0.308	0.369
	(0.476)	(0.424)	(0.406)	(0.988)	(0.451)	(0.464)
Labour	-0.221	-0.330	-0.806***	-0.400	-0.555	-0.695
	(0.315)	(0.238)	(0.281)	(0.989)	(0.375)	(0.516)
Lib Dem	1.837*	0.00287	0.278	-0.458	2.066*	0.358
	(0.997)	(1.191)	(1.149)	(0.884)	(1.236)	(1.355)
Exp young	0.000169	-9.07e-05	-0.000307	0.000124	-0.000172	-0.000266
	(0.000339)	(0.000343)	(0.000420)	(0.000465)	(0.000417)	(0.000487)
Exp alcohol	-0.00962	-0.00785	-0.00116	0.00287	-0.000681	-0.00272
	(0.00984)	(0.00772)	(0.00748)	(0.0278)	(0.00758)	(0.0124)
Pre-trend			6.609			
			(8.474)			
U18CR (t-1)				0.326***		
				(0.0531)		
Area effects	YES	YES	YES	YES	YES	YES
Year effects	YES	YES	YES	YES	YES	YES
Area trends	YES	REGIONAL	NO	NO	NO	NO
Outcome mean	30.93	30.93	30.93	29.30	29.30	30.93
N	894	894	894	745	745	894
Authorities	149	149	149	149	149	149

Notes:

(i) *** p<0.01, ** p<0.05, * p<0.1.

(ii) Dependent variable is the under-18 conception rate (based on estimated age at conception) per thousand females aged 15-17.

(iii) Standard errors (in brackets) in (1)-(3) & (5) are robust to panel-level heteroscedasticity, to contemporaneous cross-sectional correlation and to first order auto-correlation. In (4), standard errors are robust to heteroscedasticity. In (6), standard errors allow for multi-way clustering by year and local authority following Cameron, Gelbach and Miller (2010).

(iv) (4) uses the Arellano-Bond dynamic panel data estimator with the lagged dependent variable treated as endogenous.

Table 5: Determinants U18 conception rates, 2009-2014: robustness (3)

	(1) No early zero expenditure	(2) No authorities with zero expenditure	(3) *Exp* cross-border	(4) Low cross-border	(5) Log-linear model	(6) Random Effects
Exp	0.0557***	0.0659***		0.0517***	0.000473***	0.0448***
	(0.00983)	(0.0143)		(0.00983)	(0.000176)	(0.00904)
Exp Cross			0.0426***			
			(0.00961)			
GCSE	-0.461***	-0.487***	-0.470***	-0.395***	-0.00624***	-0.491***
	(0.0714)	(0.0957)	(0.0966)	(0.102)	(0.00219)	(0.0530)
Fem Unem	-0.143	-0.367	-0.495	-0.611	-0.00350	1.487***
	(0.321)	(0.610)	(0.457)	(0.454)	(0.0106)	(0.232)
Workless	0.0166	-0.0306	0.0466	0.0620	0.000269	0.122***
	(0.0341)	(0.0437)	(0.0413)	(0.0435)	(0.00117)	(0.0385)
Non-White	-0.0627	-0.206*	-0.128	-0.110	0.00168	-0.0467***
	(0.0874)	(0.124)	(0.0926)	(0.111)	(0.00250)	(0.0171)
Alcohol	0.00271*	0.00406***	0.00202***	0.00482***	-0.000103***	0.00359***
	(0.00159)	(0.00103)	(0.000762)	(0.00160)	(2.25e-05)	(0.00110)
Care	-0.0338	-0.0248	-0.00140	0.0141	0.00132**	0.0791***
	(0.0254)	(0.0310)	(0.0261)	(0.0221)	(0.000608)	(0.0209)
Conservative	0.384	0.419	0.365	0.197	0.0147	-0.565
	(0.426)	(0.546)	(0.437)	(0.470)	(0.0136)	(0.563)
Labour	-0.910*	-1.236***	-0.687**	-0.702**	-0.000417	0.344
	(0.477)	(0.358)	(0.283)	(0.325)	(0.00736)	(0.518)
Lib Dem	0.212	1.090	0.338	0.807	0.0159	0.952
	(0.953)	(1.375)	(1.192)	(1.225)	(0.0267)	(1.076)
Exp young	-0.000417	-0.000793	-0.000278	-0.000545	1.52e-06	0.000360
	(0.000374)	(0.000597)	(0.000416)	(0.000437)	(1.02e-05)	(0.000283)
Exp alcohol	0.00194	-0.00278	-0.00322	-0.00833	-4.37e-05	-0.0116
	(0.00909)	(0.0107)	(0.00705)	(0.00951)	(0.000194)	(0.00896)
Area effects	YES	YES	YES	YES	YES	NO
Year effects	YES	YES	YES	YES	YES	YES
Area trends	NO	NO	NO	NO	NO	NO
Outcome mean	30.68	30.97	30.93	30.46	30.93	30.93
N	826	438	894	720	894	894
Authorities	149	73	149	120	149	149

Notes:

(i) *** p<0.01, ** p<0.05, * p<0.1.

(ii) In (1)-(4) & (6) the dependent variable is the under-18 conception rate (based on estimated age at conception) per thousand females aged 15-17. In (5) the dependent variable is the log of conceptions.

(iii) Standard errors (in brackets) in (1)-(5) are robust to panel-level heteroscedasticity, to contemporaneous cross-sectional correlation and to first order auto-correlation. In (6) they are robust to heteroscedasticity and first order autocorrelation.

(iv) In (1), observations with zero expenditure followed by non-zero in subsequent years are recoded to missing and dropped from the sample. In (2) the sample is restricted to those authorities with no missing or zero values over the full period. In (3), *Exp* is adjusted for cross-border pupil movement. In (4) the sample is restricted to those authorities for which the % of family planning services access cross-border is less than 30%.

Appendix

Table A1: Variable descriptions and sources

Variable	Definition	Source
U18CR	Rate of conceptions ending in maternities or abortion to girls estimated to be aged under-18 at conception and resident in each local authority per 1000 girls aged 15-17. Miscarriages are excluded. Age and year at conception is estimated by the Office of National Statistics (ONS).	ONS
U18AR	Rate of conceptions ending in abortion to girls estimated to be aged under-18 at conception and resident in each local authority per 1000 girls aged 15-17. Miscarriages are excluded. Age and year at conception is estimated by the Office of National Statistics (ONS).	ONS
U18BR	Rate of conceptions ending in birth to girls estimated to be aged under-18 at conception and resident in each local authority per 1000 girls aged 15-17. Miscarriages are excluded. Age and year at conception is estimated by the Office of National Statistics (ONS).	ONS
Exp	Expenditure on Teenage Pregnancy Services per female aged 13-17 by each local authority as reported in the S251 Outturn statistics, deflated by the CPI (2014 = 100).	Department for Education
Exp Cross	Expenditure adjusted for 2015 proportion of young people attending schools in a local authority but resident in another local authority.	Department for Education
GCSE	Three-year moving average of the annual percentage of pupils in each local authority gaining 5 A*-C GCSEs.	Department for Education
Fem Unem	% of women aged 16-24 claiming jobless-related benefits.	ONS
Workless	% of children resident in the local authority living with no adult who is in employment.	ONS
Non-White	Percentage of population of secondary school age that are non-white	Department of Education
Alcohol	Rate of under-18s admitted to hospital with alcohol-specific conditions per 100,000 population.	Public Health England.
Care	Rate of all children aged 15-17 under local authority care per 10,000.	Department of Health
Conservative	Dummy variable = 1 if the Conservative Party has a majority on the local authority council; = 0 otherwise.	The Elections Centre, Plymouth University
Labour	Dummy variable = 1 if the Labour Party has a majority on the local authority council; = 0 otherwise	The Elections Centre, Plymouth University
Lib Dem	Dummy variable = 1 if the Liberal Democrat Party has a majority on the local authority council; = 0 otherwise	The Elections Centre, Plymouth University
Exp young	Expenditure per 1000 population aged 13-17 on all children-related services by each local authority as reported in the S251 Outturn statistics, deflated by the CPI (2015 = 100)	Department for Education

Exp alcohol	Expenditure per 1000 population aged 13-17 on alcohol and drug prevention services by each local authority as reported in the S251 Outturn statistics, deflated by the CPI (2015 = 100)	Department for Education