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The Impact of Emergency Birth Control on Teen Pregnancy and STIs

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Abstract

We use panel data from local authorities in England between 1998 and 2004 to examine the differential impact of increased access for teenagers to emergency birth control (EBC) at pharmacies on teenage pregnancies and sexually transmitted infections (STIs). We estimate both difference-in-difference (DD) and the more robust difference-in-difference-in-differences (DDD) models. The DD estimates provide some evidence that pharmacy EBC schemes are associated with higher teenage conception rates, but this result is not upheld in the DDD models. In contrast both the DD and DDD models provide consistent evidence that pharmacy EBC schemes are associated with higher teenage STI rates.

Keywords: emergency birth control; teenage pregnancy; sexually transmitted infections

JEL Classifications: C21, I18, J13.

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The Impact of Emergency Birth Control on Teen Pregnancy and STIs

1. Introduction

The use of modern econometric techniques in the evaluation of policy aimed at achieving improvements in sexual health amongst adolescents has been the subject of an increasing amount of attention by economists over recent years, in part due to recognition of the inherent limitations of evidence gained from randomised control trials.

A specific issue that continues to generate a large amount of controversy in both the US and Europe concerns measures aimed at increasing access to emergency birth control (EBC, sometimes known as the morning after pill or emergency contraception) for adolescents. Although a number of authors have examined the impact of such measures on adolescent abortion and pregnancy rates, to date there has been little evidence of a significant impact.

There are at least two possible explanations for these null results. The first is that increased access to EBC for adolescents does in fact result in reductions in teenage pregnancy, but that the effects are too small for the statistical tests used to reveal them. A second explanation is that, as might be predicted by standard microeconomic theory, increased access to EBC induces at least some adolescents to increase their level of risk-taking sexual behaviour and that the reduction in pregnancies from greater use of EBC is being countered by additional pregnancies resulting from this behaviour change.

Girma and Paton (2006) argue that a potential way of distinguishing between these alternatives is "to examine the differential impact of EBC schemes on teenage pregnancy and other outcomes of risky sexual behaviour, in particular, sexually transmitted infections." (p. 1030). It is this challenge that we seek to take up in this paper. We do so by exploiting recent policy changes in England which allowed access by adolescents to EBC free of charge from pharmacies in some areas but not in others.

Using annual data from English local authorities, we test whether increased access for young people to EBC had an impact either on conception rates or on rates of diagnoses of Sexually Transmitted Infections (STIs) amongst adolescents. In order to isolate the effects of the EBC programme, we need to control for any systematic shocks to the outcomes of the treated groups (adolescents) that are correlated with, but not caused by, the programme. This is achieved through the difference-in-differences (DD) approach which measures the change in outcome in treated areas (i.e. those adopting the EBC scheme) relative to the change in non-treated areas (the control group).

A concern with the DD strategy is that it may be picking up different trends in outcome between different age groups within treated areas. To deal with this concern and make our analysis more robust, we exploit the fact that older groups are not directly affected by the programme and employ a difference-in-difference-in-differences (DDD) approach. This identification strategy uses an older cohort as an additional control group and measures the causal impact of the programme as the difference in the younger groups' outcome vis-à-vis that of the older cohort in treatment areas *relative* to the same difference in areas that did not adopt the programme.

In the next section of the paper we examine the limited evidence on the impact of access to EBC on pregnancy and STI amongst adolescents. In sections three and four respectively we introduce the empirical methodology and discuss our data. The results of our analysis are reported in section five. Finally we make some concluding remarks.

2. Evaluating the Impact of Access to Emergency Birth Control

There is a long tradition dating back to Becker (1963) of economists pointing out how policy in the areas of sexual activity, conception and pregnancy resolution can cause moral hazard effects that lead to interventions having unintended consequences. Several authors (for example Akerlof, Yellen and Katz, 1996; Paton, 2002) have analysed the way in which easier access to contraception may lower the perceived costs of underage or extra-marital sexual activity and, as a result, can have an ambiguous effect on unwanted pregnancy or abortion rates.

Levine and Staiger (2002) and Levine (2004) consider how legalized abortion can be seen as a form of insurance against unwanted outcomes (in this case pregnancy) that may result from sexual activity. Within such a model, relaxation of abortion restrictions aimed at teenagers should lead to an increase in risky sexual activity, abortions and also the total number of unwanted conceptions. Further, given that actual decisions on abortion once a teenager is pregnant may be different to those which the teenager expected to take when deciding to engage in risky sexual activity, the easing of abortion restrictions has an ambiguous impact on the number of adolescent births.

Interventions aimed at providing easier access to EBC can be viewed as an additional form of insurance against contraceptive failure or non-use. As a result, we should expect such interventions also to be associated with increases in risky sexual activity. As with abortion, actual take-up of EBC in the event of, say, condom failure, may be different from what was envisaged at the time of the decision to have sex. Even in the event that EBC is

effective in preventing pregnancy in individual cases, the overall impact of the policy intervention on teenage pregnancy rates is still ambiguous.

Empirical studies to date suggest that schemes to increase access to EBC on unwanted pregnancy or abortion rates have failed to result in observable decreases in unwanted pregnancy rates. Raymond et al (2007) review 23 such studies concluding that "None of the included studies found clinically or statistically significant differences between intervention and control groups in pregnancy or abortion rates" (p. 184). In a population level study not covered by this review, Girma and Paton (2006) use matching estimators and find that areas introducing over-the-counter EBC at pharmacies in England and Wales experienced similar changes in teenage pregnancy rates to areas that did not introduce such schemes. One exception to this literature is Durrance (2007) who uses county-level data from Washington State and finds that access to EBC is associated with lower abortion rates for some cohorts.

Although the literature has found little impact on unwanted pregnancy rates, there is considerable evidence that EBC schemes are successful in their primary aim of increasing uptake of EBC amongst the target group (Raymond et al, 2007; Glasier, 2006).

Several papers have used STI rates as proxying for risky sexual behaviour in the context of abortion legislation (Klick and Stratmann, 2003, 2008; Sen, 2003). There are sound reasons for expecting that a scheme to increase access to EBC will have a differential impact on, say, rates of STIs amongst adolescents compared to pregnancy rates, if only because use of EBC reduces the likelihood of pregnancy but has no impact on the probability of contracting an STI from an infected partner.

Consider the impact of schemes to provide EBC to adolescents at pharmacies free of charge and without a prescription. The schemes lower the effective cost of EBC (and, hence, increase uptake) but also induce an increase in risk-taking sexual behaviour. This second effect may lead to an increase in pregnancies that partially or fully counters the reduction in pregnancies from greater use of EBC. Empirical support for this hypothesis has come recently from Raymond and Weaver (2008) who conclude from a randomised trial (amongst women of all ages) that access to EBC "increased the frequency of coital acts with the potential to lead to pregnancy" (p.333).

Irrespective of the net impact on pregnancy rates, we should expect easier access to EBC to lead to an increase in STI rates. The reason for this is that EBC offers no protection against STIs. This has two implications. Those adolescents who switch from abstention to sexual activity are inevitably at greater risk of contracting an STI than before. At the same time, by reducing the cost of EBC relative to other forms of family planning, marginal

adolescents may switch from using condoms, which provide at least some protection from some STIs, to reliance on EBC which affords no protection.

To summarise, to the extent to which at least some adolescents respond in a rational manner to easier access to EBC, the overall impact on teenage pregnancy rates is ambiguous, whilst STI rates amongst teenagers should increase.

There is a paucity of empirical evidence on this question, particularly relating to teenagers. Raine et al (2005) and Raymond et al (2006) both use a randomised control trial design and find no statistically significant increase in STI rates in groups subject to an EBC intervention. However, as Girma and Paton (2006) point out, the results of RCT studies do not permit an easy evaluation of the likely impact of easier access to EBC on the population as a whole. Apart from the fact that it is difficult to achieve a large enough sample size to ensure that the subsequent statistical tests are of high power, there are inherent sample selection biases in the RCT studies.

First, most RCT-based studies restrict subjects to existing users of family planning services who are, by definition, already sexually active (see, e.g. Glasier and Baird, 1998 and Raine et al, 2005). This makes it impossible for these studies to pick up any impact of increased access in increasing the number of young people who are sexually active. Hence, estimates of the impact of access to EBC on STI rates using such a design represent lower bounds at best. Second, existing users of family planning services may exhibit a different response to changes in EBC access to that of other sexually active young people.

For these reasons, population-based studies should provide an important complement to RCTs in this area and the Durrance (2007) study noted above concludes that pharmacy access to EBC is associated with higher rates of Chlamydia amongst both younger and older cohorts in Washington State. England represents a particularly useful policy environment within which to conduct similar research. Increased access to EBC has been an important component of the Government's Teenage Pregnancy Strategy which was introduced in 1999 and which aimed to cut teenage pregnancy rates by 50% by the year 2010. Importantly though, policy funding and implementation is largely devolved to local level and local authorities (LAs) have had discretion on the extent to which funds are used to increase access to EBC. Specifically, since the start of 2000, English LAs have been encouraged to introduce schemes in which EBC is offered free of charge and over the counter at pharmacies to adolescents including those below the age of 16. Although an increasing number of LAs have introduced such schemes, they have done so at different times, whilst a significant minority have declined to use this policy tool at all.

A further point to note is that EBC has been available over-the-counter from pharmacies in all parts of England from 2001 but at a cost and only to those aged over 16. Hence, we can identify two effects of the pharmacy EBC schemes: improved access to EBC for under-16s and a reduction in the price of EBC for those aged over 16[1]. This raises the possibility of a differential impact on those aged under-16 relative to older teenagers. We explore this below by presenting results for all teenagers and for under-16s.

These developments provide us with time and cross-sectional variation in the policy intervention that can help to identify the effects on outcomes such as pregnancy and STI rates. However, even in the context of fixed-effects panel data, it may be that any association between the EBC scheme and teenage conceptions (or STIs) is due to unobservable trends in sexual activity. We seek to get around this problem by estimating the effect of pharmacy EBC schemes amongst an older cohort not directly affected by the scheme. If the policy variable is associated with conceptions (or STIs) amongst affected age groups but not by the (older) unaffected cohort, it is much more reasonable to interpret the association as being causal.

3. Identification strategies

In order to quantify the causal effects of EBC on the rate of STI and teenage pregnancy we use two related identification strategies: difference-in-differences (DD) and difference-in-difference-in differences (DDD).[2]

Suppose there are two time periods, a and b, with a < b; and the EBC scheme has taken place between a and b. Define r_i = 1 if a local authority (LA) has participated in the pharmacy EBC scheme and 0 otherwise; and $\tau_t = 1$ if t = b and 0 otherwise. Receipt of treatment is then defined as $d_{it} = r_i \tau_t$.

Now letting y denote the outcome of interest (rates of STI or teenage pregnancy), the DD estimator of the average treatment effect for local author authorities (LAs) receiving the treatment is defined as

$$DD = E(y_b - y_a | r = 1) - E(y_b - y_a | r = 0) \quad (1)$$

In practice we condition the expectations in the above equation on a vector x of covariates that explain differences in y across time and local authorities. These include the number of clinic-based youth family planning sessions provided per person (*Clinic*); the

[1] We are grateful to a referee for raising this point.
[2] For an excellent exposition of DD and DDD method see Lee (2005, chapter 4) on which some of the notations in this section is based.

number of GP practices per person (*Practice*); annual proportion of children aged 15-17 in local authority care (*Care*) and the percentage of school pupils with no qualifications at age 16, measured as a three-year rolling average (*Noqual*) and the full set of time dummies.[3]

The DD approach outlined above assumes implicitly that all individuals within treated LAs received the treatment at time b. In actual fact the EBC scheme is mainly designed for teenagers (under-20s), implying that only a fraction of the population in the treated LAs are qualified for the treatment. This information can be used to refine the definition of treatment group and use a more robust identification strategy.

Let g_i = 1 if the group is treatment qualified (i.e. under-20s) and 0 otherwise (i.e. older people), then receipt of treatment is defined as $d_{it} = g_i r_i \tau_t$. The advantage of this formulation is that one can use the g = 0 group as a control group in addition to the r = 0 group. This helps make DD-based analysis more convincing by eschewing the *same time effect condition*. To see how this can be achieved define the following two DD estimators:

$$DD1 = E(y_b - y_a | g = 1, r = 1) - E(y_b - y_a | g = 1, r = 0) \quad (2)$$

and

$$DD2 = E(y_b - y_a | g = 0, r = 1) - E(y_b - y_a | g = 0, r = 0). \quad (3)$$

The DD1 estimator compares the outcome of the under-20s in treatment adopting LAs to that in non-treatment LAs, whereas DD2 is doing the same for the outcome of the older age cohort.

Now instead of the potentially restrictive *same time effect condition*, suppose we make the much weaker indentifying assumption that there is no contemporaneous shock to EBC scheme that affects the outcome of the under-20s *relative* to that of the older age cohort in the same LA-years. Then the DDD estimator which is defined as

$$DDD = DD1 - DD2 \quad (4)$$

is a consistent estimator of the effect of the EBC scheme for the under-20s in participating LAs. Effectively, the DDD estimator calculates the change in outcome of the under-20s in treatment adopting LAs relative to that of the older cohort in the same LAs, and measures this relative to the equivalent change in the non-treatment LAs.

We further allow for differing trends amongst treatment and control groups by including in our robustness checks, specifications with area-specific time trends. Throughout we report standard errors which allow for heteroscedasticity, serial correlation within panels

[3] We define the variables and provide summary statistics in the Appendix.

and contemporaneous correlation across panels following the contributions of Bertrand et al (2004) and Hansen (2007).

4. Data

We have two key outcome measures, one to measure teenage STIs and one to measure teenage pregnancies. Both are measured on an annual basis between 1999 and 2004 for all 147 higher tier local authorities in England.

Pregnancy data in England is of high quality relative to many other countries. There are legal requirements for the reporting of live births and abortions. The Office of National Statistics estimate the time of conception in each case to arrive at annual conception rates for each local authority in the country. Here we use conception rates amongst all teenagers (using the female population aged 15-19 as the deflator) and for two sub-groups, under-16s (using population aged 13-15 as the deflator) and under-18s (using population aged 15-17 as the deflator).[4]

For the DDD estimates, we use conception rates amongst women aged over-24, a group that should be unaffected by the EBC schemes. Note that a few local authorities extend access to their pharmacy EBC schemes to those aged 20-24. Unfortunately there are no consistent data on which authorities have extended the scheme in this way. Even in those cases where access is extended, we would expect any impact on 20-24 year olds to be less than that for younger teenagers as cost considerations are likely to be less of a barrier to access to EBC for the older cohort.[5] However, in the empirical section, we report DD and DDD estimates using those aged 20-24 as an alternative intervention group.

To measure STIs, we use data on the number of diagnoses of the main STIs at genitourinary medicine (GUM) clinics, provided by the Health Protection Agency (HPA). We use the same age groups as for conceptions with the exception of under-18s for which STI data are not available.

A potentially important issue is that we are only able to observe diagnoses of STIs and not actual infections. Diagnoses will underestimate infections for at least two reasons. The first is that some STIs are largely asymptomatic and may go unreported (Fenton *et al*., 2001). The second reason is that services are rationed at many GUM clinics. The rapid

[4] The Government's Teenage Pregnancy Strategy specifies targets for reductions in conception rates for under-18 and under-16. However, the pharmacy-EBC schemes are generally open to all teenagers.
[5] Indeed there is evidence from the few authorities where data are available that take-up amongst 20-24 year olds under the scheme is much lower than for teenagers.

increase in demand for GUM services in the past few years has forced some clinics to impose long delays between when a patient first contacts the clinic and the time when the patient actually sees a health professional at the clinic (Foley et al, 2001). In these cases, there will be a supply-induced constraint on the number of diagnoses reported by the clinic. This will be a problem only if these constraints vary systematically with entry into the EBC scheme. Even in this event, the constraints are likely to have an impact across all age groups, not just adolescents. For this reason, DDD models in which we test for a differential impact of the scheme on adolescents with the impact on older people will be particularly important for the STI models.

Our treatment variable (*Pharm*) is an indicator dummy taking the value one if there is a pharmacy-EBC scheme operating in a particular year-area combination. We register a value of zero for any years in which the scheme operated for less than 3 months and for any years in which a very small scale pilot scheme was operating.

5. Results

5.1 Baseline Results

In Figure 1, we illustrate national trends in conception rates amongst teenagers and older women along with the rate of participation by local authorities in the EBC scheme between 1996 and 2004. Pharmacy EBC schemes started in 2000 and their prevalence increased rapidly until 2004 when they covered approximately 50% of local authorities. Teenage conception rates show a downward trend from a peak in 1998 (two years before the pharmacy EBC scheme started and a year before the start of the Teenage Pregnancy Strategy) until 2003 followed by a small increase in 2004, just as the EBC scheme reached its peak. Amongst older women, there is a downward trend from 1997 but this reverses from 2001. So although there is no evidence that pharmacy EBC had an impact on the absolute level of teenage conception rates, there is some evidence that rates decreased relative to the older cohort just as the EBC scheme was taking off.

Similar series for STI diagnosis rates are reported in Figure 2. Rates for teenagers and older people both consistently increase throughout the period. However, rates amongst teenagers increase at a faster rate and there is some evidence that the gap with rates amongst older people widens as the EBC scheme progresses.

So the national trends for conception and STI diagnosis rates are clearly different and in a way that is consistent with a differential impact of the pharmacy EBC scheme. However, there are also differences in trends prior to the establishment of the scheme making it difficult

to draw firm conclusions from the descriptive data. Hence, we now turn to the local authority level analysis to discern whether or not we can identify any link between those authorities adopting the pharmacy scheme and either conception or STI rates.

We report the baseline estimates of the impact of pharmacy EBC schemes on teenage conception and STI rates in Tables 1 and 2 respectively. For conceptions, we report estimates for three intervention groups, all teenagers (U20), under-18s (U18) and under-16s and for our control group, over-24s (O24). We report the STI results for the same age groups with the exception of U18s for which STI data are not available. The DD and DDD effects for conceptions and STIs are summarised in the first row of Tables 3 and 4 respectively.

Rates are measured in natural logarithms meaning the coefficient on the policy variable represents the percentage change associated with the intervention. Using logs allows a consistent comparison across age groups but does mean that a few observations with zero values (particularly for STIs amongst under-16s) are omitted. For that reason, we also report estimates in levels as one of our robustness checks.

The DD estimate of the impact of EBC schemes on teenage conception rates is positive for all three age groups, and weakly significant for U20 and U18s, implying that EBC schemes may be associated with a modest increase in teenage conceptions. However, the coefficient on *Pharm* is also positive and statistically significant for the control group, suggesting that the weak positive association for teenage conceptions should not be interpreted as a causal effect. For each of the teenage age groups, the point DDD estimates are very close to zero and in each case we cannot reject the null hypothesis that the EBC scheme had no impact on conception rates.

Looking at the control variables, the availability of adolescent family planning clinic sessions appears to have a somewhat ambiguous impact on conception rates. Although at first sight this may seem surprising, existing evidence is also equivocal on the impact of access to family planning services on teenage pregnancy rates (see, e.g., Paton, 2002; Kearney and Levine, 2009). The socio-economic variables are generally of the expected sign with both children in care and the proportion with no educational qualifications being associated with higher teenage conception rates.

In contrast, the results reported in Table 2 provide more consistent evidence that EBC schemes are associated with an increase in the rate of STI diagnoses amongst teenagers. The baseline DD estimate of the *Pharm* coefficient for the U20 cohort is 0.049 which is statistically significant at the 5% level. This implies that, on average, the presence of a pharmacy EBC scheme in a local authority is associated with an increase in the rate of STI

diagnoses amongst teenagers of about 5%. The equivalent figure for U16s is even larger at 12%.

The estimated impact for the control group is negative but small and statistically insignificant. As a result, the DDD estimates (reported in the first row of Table 4) are slightly higher than the DD estimates and also statistically significant. In other words, there is evidence that pharmacy EBC schemes led to an increase in STI rates both relative to areas without such schemes and relative to the older cohort that should not have been affected by the scheme.

The coefficient on family planning clinic sessions in the STI models is relatively small and generally lacking in explanatory power. However, note that the impact of clinic sessions may operate in several contradictory ways. In the first place, promotion of some forms of family planning may help to protect sexually active youngsters against some STIs. However, some clinics also offer tests for some STIs subsequent to which, young people may be directed to STI clinics where diagnoses will be picked up. Reassuringly, the estimates on *Pharm* are robust whether or not this control is included.

5.2 Robustness Checks

Robustness checks for conceptions and STI rates are reported in Tables 3 and 4 respectively. In these tables, we report only the DD and DDD estimates for each age group, although we continue to include the same set of control variables and fixed effects as before.

In the first row of these tables, we report the baseline DD and DDD estimates as discussed above. In the row below this, we report a falsification test in which the policy variable is coded as having occurred two years later than its actual introduction. Logically, the schemes should not have had any impact on conception or STI rates before their introduction and so, any significant effect in this specification would cast doubt that the baseline results can be interpreted as causative. For conceptions, all of the DD and DDD estimated effects are negative and, with the exception of one DD estimate, are not statistically significant. When the recoded pharmacy variable is used in the STI models (row 2 of Table 4), the DD and DDD effects vary in sign but are small in magnitude and statistically insignificant.

The next check involves the inclusion of area-specific trends. We do this in two ways: first by including a time trend for each of the 9 main regions in England and second by including a separate time trend for every local authority. We continue to include fixed effects for local authorities and for years (the results are robust to excluding the year fixed effects).

Clearly the inclusion of the full set of local authority-specific trends is quite demanding of the data and there is the danger of losing residual variation, and hence, our tests having low statistical power. In fact the results are reasonably robust to either specification. The main difference is that when the local authority-specific trends are included, the estimates for the U16 group are much smaller than before (although still positive) and statistically insignificant.

The next check is to re-specify the pharmacy EBC variable as the number of pharmacies in the EBC scheme divided by the population aged 15-19. We would expect any impact on conceptions or STIs to be larger in areas with a greater number of pharmacies per head of population. Unfortunately data on the number of pharmacies in the scheme in some local authorities are not available for each year and some values had to be imputed. With that caveat, the results with this specification are similar to the baseline regressions, with the exception that the estimated effect on STIs amongst U16s, although positive, is no longer significant at conventional levels.

We next experiment by including an additional variable measuring the percentage of bordering local authorities that have a pharmacy EBC scheme in operation during the relevant year. This approach is an attempt to control for adolescents resident within one local authority taking advantage of services provided within another authority. In all cases, the point estimates and significance levels on the EBC variable are similar to in the baseline models.

A further check is to measure the outcome variables in levels (rather than logarithms). The DDD estimates are awkward to interpret in this case as the estimates represent changes in levels rather than percentages.[6] However, the patterns of results is similar in that the estimated impact of access to EBC on STI rates is consistently positive and, for the DD estimates at least, statistically significant.

The next check is to estimate the model using abortion rather than total conception rates. The motivation for this is two-fold. First, it is possible that EBC may have a differential impact on abortions and births. To the extent that EBC prevents unwanted pregnancies, we might expect the EBC scheme to have a greater effect on adolescent abortion rates. On the other hand, there is some evidence (Lee et al, 2004) that sexual health services in general are associated with an increase in the proportion of pregnancy adolescents who

[6] To take an example, the mean level of U20 conception rates is several times higher than that for U16 rates. A small percentage change in the U20 rate may translate into a large difference in the level relative to the U16 rate. In this case, the DDD estimate would not accurately measure the relative impact of the policy change on the two groups.

choose the abortion option. The second motivation is that the control group may be more valid when comparing abortions than conceptions. The reason for this is that the proportion of planned (and 'wanted' pregnancies) is likely to be relatively high amongst older cohorts. Focusing purely on abortions allows us to get closer to a measure of unwanted pregnancies that may be more consistent across age groups.

In fact, the results (reported in the penultimate row of Table 3) are not qualitatively different to those for conceptions. The EBC policy variable attracts a positive and significant coefficient for all three of the teenage groups. However, the coefficient on the variable in the model for over-24s is also positive, casting doubt on any interpretation that the EBC policy has a causal impact on teenage abortions. These results are in contrast to the findings in Durrance (2007) that easier access to pharmacy EBC in the U.S. is associated with lower abortion rates. It may be that the differences are due to the differences in social and cultural trends between the U.S. and the U.K. and, in particular, greater promotion of and public acceptance of abortion over the time period in question. However, we note that the impact of EBC on abortion rates reported by Durrance (2007) is not robust to alternative specifications and, further, that other research in the U.S. (e.g. Raymond, 2007) finds that access to EBC has little or no effect on abortion rates.

Finally, we report DD and DDD estimates using conception and STI rates for 20-24 year olds. As we note above, some local authorities have extended the EBC schemes to include this age group. Our expectation is that we should observe little impact of the schemes amongst this cohort, but in the absence of comprehensive data on the extensions, we cannot rule out the possibility. The results are reported in the final row of Tables 3 and 4. As with the U20 group, the DD estimates suggest that the EBC schemes increased conception rates amongst 20-24 year olds, but the result does not hold up in the DDD estimates. For STIs, both the DD and DDD estimates are positive but much smaller than for the younger age groups and statistically insignificant.

6. Conclusions

Most previous research has found evidence that increased access to emergency birth control is generally successful in increasing take-up but does not appear to reduce unwanted pregnancy or abortion rates significantly. Suggested explanations for the apparent contradiction include the possibility that the most common hormonal EBC (Levonorgestrel) has a lower effectiveness than is commonly assumed (Trussell et al, 2008). Alternatively, access to EBC may induce behavioural change such that risk taking sexual behaviour

increases (Raymond and Weaver, 2008). The increase in pregnancy rates from, for example, greater sexual activity may cancel out reductions in pregnancy rates from greater use of EBC.

In this paper, we have provided an implicit test of these alternative explanations by focusing on another outcome of risky sexual behaviour that is not directly affected by use of EBC, namely sexually transmitted infections. Using both DD and DDD estimations on panel data at the level of the local authority between 1998 and 2004, our results suggest that, consistent with most previous research, increased access to emergency birth control at pharmacies for adolescents appears not to have reduced teenage conception rates in England. In contrast, our results provide evidence that these schemes are associated with a higher rate of diagnoses of STIs amongst teenagers. The estimated effect on STI rates amongst under-16s is larger than that for older teenagers. The results pertaining to all teenagers are robust to a number of falsification tests and alternative specifications. The statistical significance of results pertaining to under-16s is somewhat less robust.

The results in this paper should be interpreted cautiously. The finding that EBC schemes are associated with teenage STIs but not pregnancies is consistent with the hypothesis that greater access to EBC induces an increase in adolescent risky sexual behaviour. However, it is important to recall that we are able to observe only the number of STI diagnoses at GUM clinics and not the total number of infections. As we have already noted, diagnoses at GUM clinics may be affected by an increase in awareness of STIs (particularly those infections that can be asymptomatic) but also by restrictions at clinics on the number of patients that can be seen. Our use of an older age cohort, unaffected by EBC schemes, as a control takes account of the latter issue to some extent. We also control for other factors (such as adolescent family planning clinics) which are likely to be associated with an increase in an awareness of STIs. However, given the limitations in the STI data, it is not possible entirely to exclude alternative explanations for the lack of an impact of EBC on adolescent pregnancy rates.

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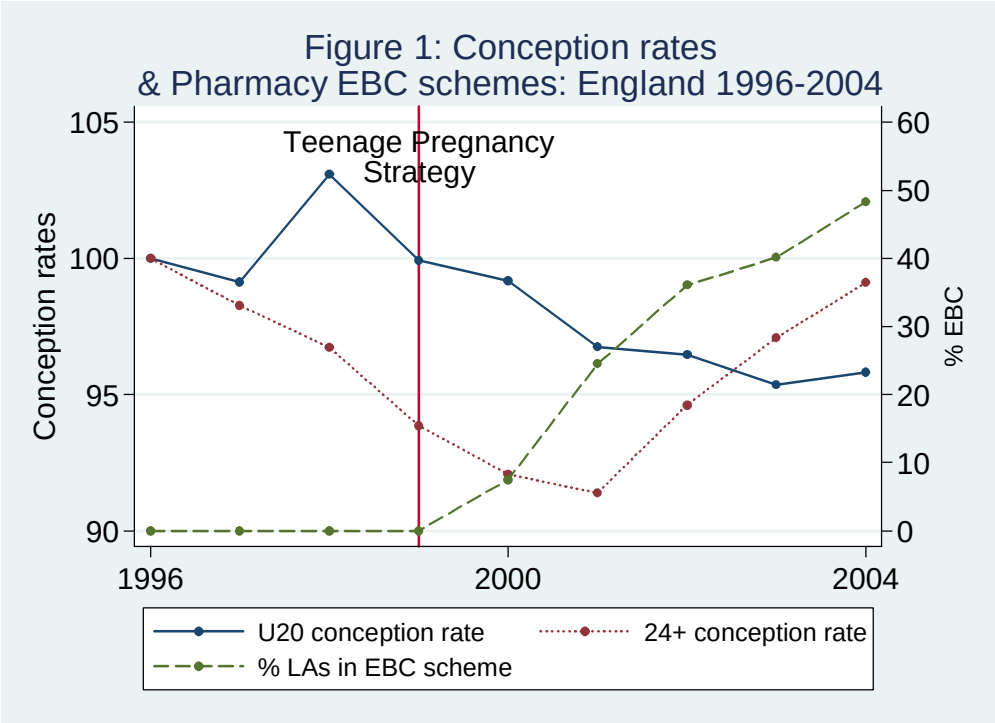
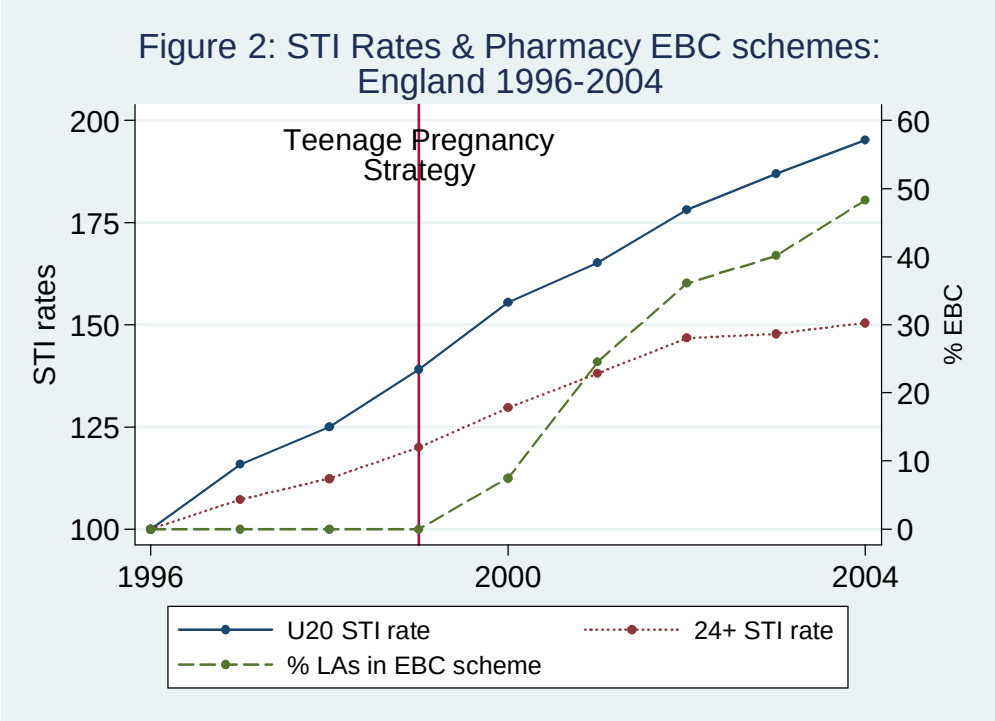


Notes:
(i) Conception rates are normalised to equal 100 in 1996.
(ii) Under-20 rates are per 1000 females aged 15-19. Rates for over-24s are per 1000 aged 24-44.



Notes:
(i) STI rates are rates of diagnoses of key infections at GUM clinics and are normalised to equal 100 in 1996.
(ii) Under-20 rates are per 1000 people aged 15-19. Rates for over-24s are per 1000 aged 24-44.

Table 1: Impact of pharmacy EBC on conception rates (base specification)

	U20	**U18**	**U16**	**O24**
Pharm	0.016*	0.014*	0.009	0.009**
	(0.008)	(0.008)	(0.018)	(0.004)
Clinic	-0.001	0.002**	-0.004	0.001
	(0.001)	(0.001)	(0.002)	(0.001)
Care	4.077***	5.064***	5.697**	1.418***
	(0.957)	(1.376)	(2.811)	(0.304)
Noqual	0.003	0.008**	-0.008	-0.002
	(0.003)	(0.004)	(0.009)	(0.001)
Practice	0.005	0.014***	0.015**	0.003
	(0.004)	(0.005)	(0.007)	(0.003)
Observations	1029	1029	1029	1029

Notes:
(i) Dependent variable is the log of conception rates. Rates are per thousand females aged 15-19 for U20, aged 15-17 for U18, aged 13-15 for U16 and aged 25-44 for O24.
(ii) *** indicates significance at the 1% level, ** at 5%, * at 10%.
(iii) Figures in brackets are standard errors, adjusted for cross-panel heteroscedasticity, contemporaneous correlation across panels and first order serial correlation within panels.
(iv) In each case a full set of year and local authority fixed effects are included.

Table 2: Impact of pharmacy EBC on STI rates (base specification)

	U20	**U16**	**O24**
Pharm	0.049**	0.120**	-0.012
	(0.019)	(0.050)	(0.019)
Clinic	3.0 e-4	0.021*	-0.004
	(0.005)	(0.012)	(0.003)
Care	-5.94	1.052	-5.039**
	(4.114)	(7.635)	(2.162)
Noqual	-0.021***	-0.050***	-0.015***
	(0.007)	(0.014)	(0.005)
Practice	0.010	-0.010	-0.001
	(0.011)	(0.011)	(0.007)
Observations	912	906	912

Notes:
(i) Dependent variable is the log of STI rates. Rates are per thousand people aged 15-19 for U20, aged 13-15 for U16 and 25-44 for O24.
(ii) *** indicates significance at the 1% level, ** at 5%, * at 10%.
(iii) Figures in brackets are standard errors, adjusted for cross-panel heteroscedasticity, contemporaneous correlation across panels and first order serial correlation within panels.
(iv) In each case a full set of year and local authority fixed effects are included.

Table 3: DD and DDD estimates of the impact of pharmacy EBC on conception rates.

	DD estimates			DDD estimates		
	U20	**U18**	**U16**	**U20**	**U18**	**U16**
Baseline	0.016*	0.014*	0.009	0.007	0.005	0.000
	(0.008)	(0.008)	(0.018)	(0.009)	(0.009)	(0.018)
Pharm t+2	-0.010	-0.015	-0.022	-0.007	-0.021*	-0.028
	(0.010)	(0.010)	(0.019)	(0.011)	(0.011)	(0.019)
Regional trends	0.020**	0.016*	0.015	0.011	0.007	0.006
	(0.008)	(0.008)	(0.018)	(0.009)	(0.009)	(0.018)
Local authority trends	0.017*	0.019	-0.001	0.013	0.015	-0.005
	(0.009)	(0.012)	(0.021)	(0.009)	(0.012)	(0.021)
Pharmacy rate	7.353***	3.915***	3.253	3.077*	-0.361	-1.023
	(1.426)	(1.332)	(5.873)	(1.793)	(1.719)	(5.973)
Neighbouring schemes	0.019**	0.018**	0.011	0.007	0.006	-0.001
	(0.008)	(0.009)	(0.018)	(0.009)	(0.010)	(0.018)
Levels	1.082**	0.068	0.570	0.493	-0.521	-0.019
	(0.512)	(0.146)	(0.357)	(0.587)	(0.323)	(0.459)
Abortion rates	0.030***	0.039***	0.036*	0.012	0.021	0.018
	(0.010)	(0.009)	(0.021)	(0.015)	(0.014)	(0.024)
20-24 year olds	0.012**			0.003		
	(0.005)			(0.006)		

Notes:
(i) The DDD estimates are based on the estimators given by equations 2, 3 and 4 in the main text. The control group used to estimate equation 3 is those aged over-24.
(ii) Except for the rows labelled 'Levels' and 'Abortion rates' the dependent variable is the log of conception rates. For 'Levels', the dependent variable is conception rates whilst for 'Abortion rates' it is the log of abortion rates.
(iii) *** indicates significance at the 1% level, ** at 5%, * at 10%.
(iv) Figures in brackets are standard errors, adjusted for cross-panel heteroscedasticity, contemporaneous correlation across panels and first order serial correlation within panels.
(v) In each case a full set of year and local authority fixed effects are included, along with all the control variables as listed in Table 1.

Table 4: DD and DDD estimates of the impact of pharmacy EBC on STI rates

	DD estimates		**DDD estimates**	
	U20	**U16**	**U20**	**U16**
Baseline	0.049**	0.120**	0.061**	0.132**
	(0.019)	(0.050)	(0.027)	(0.053)
Pharm t+2	-0.001	0.039	-0.009	0.031
	(0.025)	(0.086)	(0.033)	(0.089)
Regional trends	0.057**	0.141**	0.073**	0.157**
	(0.023)	(0.061)	(0.030)	(0.064)
Local authority trends	0.062**	0.017	0.072*	0.027
	(0.027)	(0.065)	(0.043)	(0.073)
Pharmacy rate	14.029***	14.842	18.39***	19.2
	(4.652)	(12.741)	(6.128)	(13.351)
Neighbouring schemes	0.059***	0.145**	0.069***	0.155**
	(0.019)	(0.057)	(0.026)	(0.060)
Levels	1.241***	0.152***	1.214**	0.125
	(0.456)	(0.051)	(0.470)	(0.123)
20-24 year olds	0.003		0.015	
	(0.019)		(0.027)	

Notes:
(i) The DDD estimates are based on the estimators given by equations 2, 3 and 4 in the main text. The control group used to estimate equation 3 is those aged over-24.
(ii) The dependent variable is the log of STI rates except for the 'Levels' row where it is STI rates.
(iii) *** indicates significance at the 1% level, ** at 5%, * at 10%.
(iv) Figures in brackets are standard errors, adjusted for cross-panel heteroscedasticity, contemporaneous correlation across panels and first order serial correlation within panels.
(v) In each case a full set of year and local authority fixed effects are included along with all the control variables listed in Table 2.

Appendix

Table A1: Variable definition and sources

Variable	Definition	Source
Conception rate	Number of conceptions ending in maternities or abortion to the relevant age group resident in each local authority per 1000 females. Miscarriages are excluded. Age at conception is estimated by the Office of National Statistics (ONS). The population deflators are the final mid-year female population estimates published by ONS. The base population for all teenagers is 15-19, for under-18s 15-17, for under-16s 13-15 and for over-24s 24-44.	ONS: supplied to the authors
STI rate	Number of STI diagnoses at GUM clinics in each local authority per 10,000 people. Base population for all teenagers is 15-19, for under-18s 15-17, for under-16s 13-15 and for over-24s 24-44.	Health Protection Agency: supplied to the authors
Abortion rate	Number of conceptions ending in abortion to the relevant age group resident in each local authority per 1000 females. Population deflators are as for conception rates.	ONS: supplied to the authors
Pharm	Indicator variable equalling 1 if pharmacy scheme to provide free EBC to young people is in operation in a local authority in particular year.	Department of Health & Teenage Pregnancy Co-ordinators: supplied to author.
Pharmacy rate	Number of pharmacies in each local authority providing free emergency birth control to young people per 1000 females aged 15-19.	Teenage Pregnancy Co-ordinators: supplied to the author and cross-checked at www.RUThinking.org.uk
Clinic	Annual number of family planning clinic sessions aimed at young people per 1000 females aged 15-19.	Department of Health (DOH): supplied to the authors
Care	Annual rate of all children aged 15-17 under local authority care per 10,000 people.	DOH
Noqual	Three- year moving average of the annual percentage of pupils in each local authority gaining no GCSEs at age 16	Department of Education:
Practice	Annual number of GP practices in each authority per 1000 population aged 15-19	National Database for Primary Care Groups and Trusts

Table A1: Summary statistics

Variable	Mean	SD	Minimum	Maximum
U20 conception rate	65.40	17.76	26.18	136.49
U18 conception rate	46.49	13.89	18.88	103.26
U16 conception rate	8.69	3.22	2.16	28.30
O24 conception rate	68.62	11.73	46.28	118.06
20-24 conception rate	110.66	26.76	49.35	207.70
U20 STI rate	15.61	11.56	0.72	80.81
U16 STI rate	1.39	1.32	0.00	9.30
O24 STI rate	6.38	5.61	0.38	34.95
20-24 STI rate	21.57	13.07	1.13	83.33
U20 abortion rate	26.50	9.31	13.65	75.41
U18 abortion rate	21.14	7.29	9.64	61.46
U16 abortion rate	4.82	1.93	1.00	17.25
O24 abortion rate	12.50	6.08	5.75	36.85
Pharm	0.22	0.42	0	1
Pharmacy rate	0.0006	0.0016	0	0.01
Clinic	4.03	3.62	0	31.61
Care	0.020	0.010	0.006	0.067
Noqual	5.76	2.31	1	15.85
Practice	10.53	3.64	2.97	35.73