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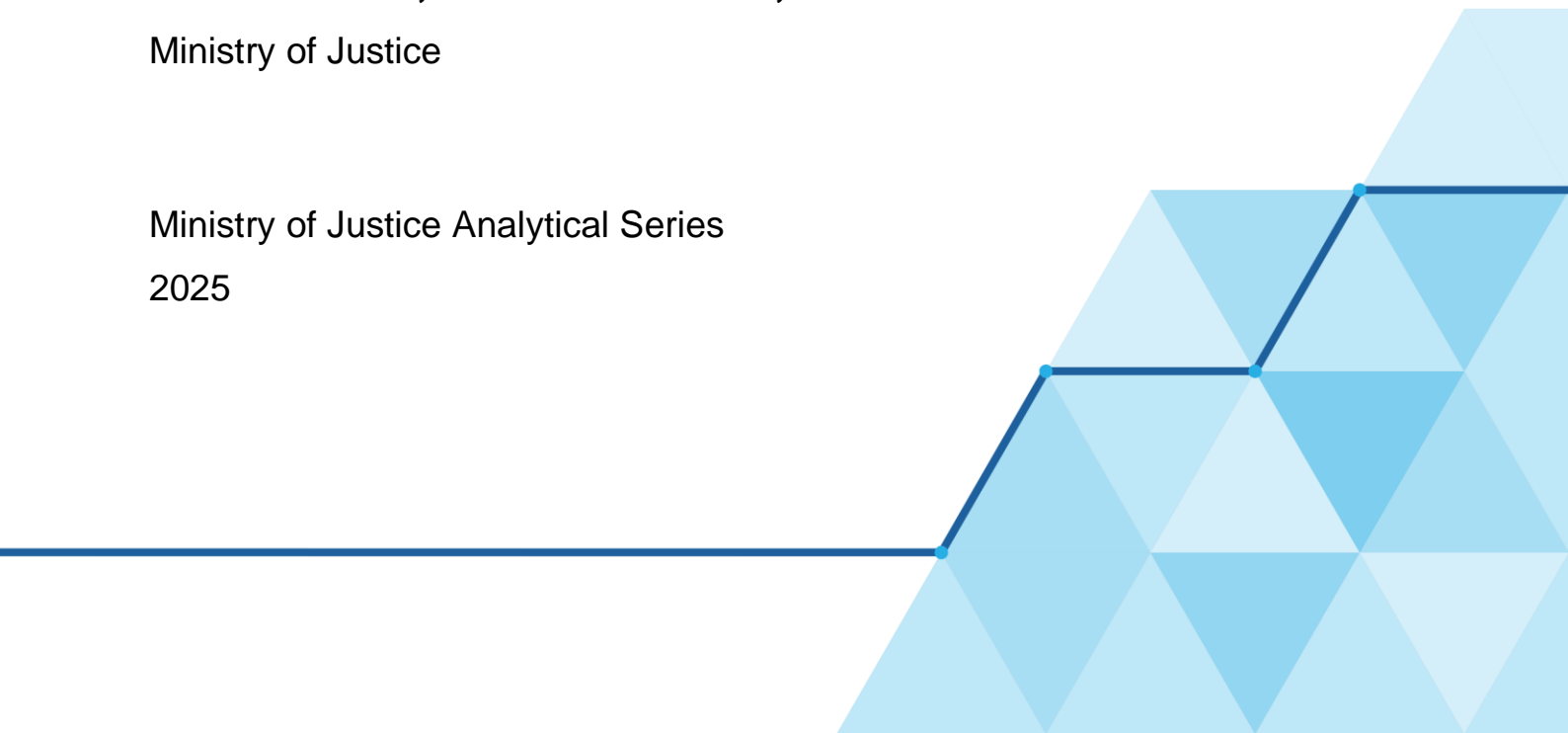
# Impact Evaluation of Pre-recorded Cross Examination for Vulnerable & Intimidated Witnesses

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# 1. Executive Summary

Section 28 (s.28) of the Youth Justice and Criminal Evidence Act 1999 (YJCEA) is a special measure that allows vulnerable or intimidated witnesses to pre-record their cross-examination ahead of the trial. This means that witnesses do not need to give their evidence live during the trial as the recorded evidence and cross-examination can be played back at the court.

The key aims of s.28 were for the cross-examination to happen earlier in the process than if cross-examination occurred at trial, and to improve the quality of the evidence provided by the witness. It was envisaged that this would be achieved by:

- Making it easier for vulnerable/intimidated witnesses to recall/recount events clearly by reducing the length of time to cross-examination.
- Improving the experience for witnesses (e.g. less stressful/traumatic/accessing support and therapy more easily (Ward et al., 2023)).

S.28 was piloted for vulnerable witnesses and children under 16 years of age in three Crown Courts in 2013, with the pilot extended to children under 18 years of age in 2017, an additional six courts commencing the provision in 2019, and rollout to all Crown Courts completed in November 2020.

For complainants of sexual and modern slavery offences, it was piloted in three Crown Courts in 2019, and has been available in all Crown Courts since September 2022. This followed a phased rollout, with additional courts adding the provision over 2021 and 2022. Whilst it is offered to all eligible victims and witnesses, all applications must be approved by the judge.

Our research set out to understand the effect of s.28 on conviction rates, court operations, and the timeliness of cases.

When analysing convictions and court operations, we used a form of analysis that allows us to identify the effect of s.28 across the whole caseload, including the phases of the rollout to compare courts with, and without, expanded s.28 provision.

We found that for the sample period January 2019 to September 2022:

- Considering conviction rate, s.28 does not appear to have an effect on a jury's decision to acquit, or convict, in a case, when controlling for court, case mix and trends over time.
- When examining court functioning, s.28 does not seem to have a long-term effect on the percentage of ineffective trials in the court. However, during a short period after s.28 implementation in each group of courts there did appear to be an increase in the percentage of ineffective trials in the courts that had just implemented s.28; this suggests there may have been some short-term negative effects as the policy was rolled out in each court.
- With regard to timeliness, on average, s.28 cross-examinations tend to occur 150 days earlier than a standard cross-examination, and s.28 trials take place 30 days later than a standard trial.

Our analysis suggests that s.28 has reduced the length of time to cross-examination with no discernible impacts on case outcomes or court functioning.

However, analysis was constrained by the operational decisions and data collection at the time increased s.28 eligibility was rolled out. The roll-out also coincided with two major disruptions to the Crown Courts (the COVID-19 pandemic, and industrial action by the Criminal Bar Association).

Specific issues encountered include:

- Data on specific characteristics of s.28 witnesses and cases was limited, which prevents direct comparisons between s.28 and non-s.28 cases. We did not have data for witness characteristics, so it was not possible to distinguish between vulnerable and intimidated witnesses. We have had to assume that any impact of s.28 on vulnerable and intimidated witnesses (and the impact of using s.28 for vulnerable and intimidated witness) was equivalent, or that it could be explained by factors we do have data for (such as the different rates of Adult and Child Sex Offences). If there were both substantial differences between the impact of s.28 on vulnerable and intimidated witnesses, and substantial differences in the

proportion of vulnerable and intimidated witnesses within a given court (not explained by the roll-out phase), this could have biased our findings.

- This analysis could not control for decisions prior to the trial, such as witness attrition, plea behaviour, charging decisions or etc. Therefore, it was unable to account for cases that were added to or removed from the caseload because of these impacts (for example, cases that would otherwise have been lost to attrition, or cases that would have had a not guilty plea if s.28 hadn't been used). If these cases had different characteristics than the general caseload, this may have biased our findings.
- Data on use of other special measures was limited, which limited the ability to distinguish the impacts of s.28 from other measures, especially pre-recorded evidence-in-chief (known as Achieving Best Evidence (ABE), or s.27 interview). All s.28 cases were also using s.27, but cases with s.27 may not have always used s.28. While no other special measures were rolled out during the trial period, a substantial regional or local difference, or a significant upward or downward trend in uptake or delivery of non-s.28 special measures, might have biased our results.
- Analysis of case timescales and outcomes requires cases to have reached a trial or verdict, which for many cases will be three years or longer (MoJ, 2023b). This means that some cases received near the end of the rollout had not had a trial or verdict when we stopped collecting data. To avoid biasing our results by only including short trials, we excluded some of these cases from our analysis of verdicts.
- To meet operational needs, well-prepared courts with judges previously experienced in s.28 were chosen as early adopters when eligibility was extended to intimidated witnesses. This meant that more of our data for s.28 cases came from these well-prepared courts, which were not necessarily representative of the whole Crown Court estate. This may have biased our findings if the impact of s.28 in those courts was not equivalent to the impact in other areas.
- The roll-out of s.28 happened over a period of substantial disruption in normal court processes, with the COVID-19 pandemic (leading to court closures, reduced court activity, and a backlog of cases in Crown Courts), and the industrial action

by the Criminal Bar Association both heavily impacting the courts, with a particular impact on trial effectiveness. This type of disruption makes other factors of court functioning, including potential disruptive impacts from s.28 and the timelines of s.28 delivery, harder to estimate.

## 2. Introduction

The YJCEA introduced a range of special measures to support vulnerable and intimidated victims and witnesses to give their best evidence and reduce the anxiety associated with giving evidence in court. Measures include changes to how the witness and defendant are presented in court (e.g. screening the witness stand from the defendant, giving evidence from another location via live link), assistance for the witness (via an intermediary or physical aid to communication), or changes to court procedure (e.g. removal of wigs). Relevant special measures can be combined.

Section 27 of the YJCEA (s.27) allows for eligible witnesses to submit pre-recorded evidence-in-chief (also known as Achieving Best Evidence or ABE) before the trial. This recording is then played at the trial.

Section 28 of the YJCEA (s.28) takes this further, allowing eligible witnesses who have pre-recorded their evidence-in-chief to additionally record their cross-examination ahead of the trial. Witnesses using s.28 therefore typically do not need to be present in person at the trial. S.28 is the last of the victim and witness measures in YJCEA to be implemented.

In general, cross-examination is recognised as potentially stressful or traumatic for vulnerable or intimidated witnesses, (Cossins, 2009), and this can reduce the quality of the evidence given. The key aims of s.28 are for the cross-examination to happen earlier in the process than if cross-examination occurred at trial, and to improve the quality of the evidence provided by the witness. It is envisaged that this is achieved by:

- Improving the memory recall of vulnerable or intimidated witnesses by reducing the length of time between events and cross-examination
- Improving the experience for witnesses (e.g. less stressful/traumatic/ accessing support and therapy more easily (Ward et al., 2023))

Two cohorts of witness are currently automatically eligible for s.28:

- Vulnerable witnesses: those under the age of 18 or those whose quality of evidence would be diminished by a mental or physical disorder or impairment; and
- Intimidated witnesses: for the purposes of s.28, the law defines these as adult complainants of sexual or modern slavery offences.

S.28 was piloted for vulnerable witnesses and children under 16 years of age in three Crown Courts in 2013, with the pilot extended to children under 18 years of age in 2017. An additional six courts commenced the provision in 2019, and rollout to all Crown Courts completed in November 2020.

For complainants of sexual and modern slavery offences, it was piloted in three Crown Courts in 2019 and has been available in all Crown Courts since September 2022, following a phased rollout with additional courts adding the provision over 2021 and 2022. Whilst it is offered to all eligible victims and witnesses, all applications must be approved by the judge.

## 2.1 Existing Evidence

Studies on the impact of s.28 (or more generally on special measures or use of pre-recording of witnesses) have tended to fall into one of three main types:

- quantitative studies of administrative datasets;
- interview and survey-led evaluations of service users and practitioners (sometimes with supplementary analysis of a very small dataset); and
- simulation-driven research using some form of ‘mock trial’ allowing analysis of juror reactions and responses.

The Ministry of Justice (Ward et al., 2023) published a process evaluation of s.28 in April 2023. It aimed to understand if the provision was working as intended for complainants of sexual and modern slavery offences. Analysis was based on interviews with criminal justice practitioners and witnesses with experience of s.28. Key findings of the process evaluation included:

1. Both practitioners and witnesses felt that s.28 improved the witness experience – with key benefits including physical separation from the defendant(s) and the ability to seek therapy at an earlier point in time.
2. Witnesses were not always provided with timely, clear, and consistent information about the s.28 process by the police or CPS.
3. Some practitioners were concerned about whether witnesses were making an informed choice to use s.28, rather than being influenced by the police.
4. Some advocates and court staff believed s.28 to have a negative impact on scheduling and court listings. This was seen as due to the additional hearing, replaying the cross-examination at trial, and the requirement (since removed) for the same judge and advocates to attend all hearings.
5. Several practitioners were concerned about the quality of technology supporting s.28 cross-examinations and courtroom availability.

Practitioners have also raised concerns about the impact of s.28 on juror decision making, and subsequent court outcomes. They suggest that s.28 can play against witnesses' perceived authenticity in the minds of jurors (Ward et al., 2023). They also reported that practitioners acknowledged that they had no evidence of these effects (*ibid.*).

In addition to the 2023 Process Evaluation, the MoJ also published an earlier Process Evaluation of the initial s.28 pilot in 2016. This evaluation also found that practitioners (including court staff, CPS lawyers, advocates and police officers) had some concerns around the impact of s.28, or pre-recording and special measures in general. They suggested that they would result in delayed or cancelled trials and reduced jury impact from witness testimony potentially leading to reduced convictions (Baverstock, 2016). There was also a suggestion that the protocols around cross-examination in s.28 cases reduced the defence's ability to react to new information and could undermine the defence case (*ibid.*).

On the impact of court functioning, the 2016 Process Evaluation generally found that while there were perceived additional challenges, they were manageable within existing frameworks (*ibid.*).

Other interview and survey-based work saw concerns raised about the effect of physical removal from the court room on the impact of testimony (Victims Commissioner, 2021), or the overall quality of evidence being degraded by poor-quality recording and playback (*ibid.*).<sup>1</sup>

Following the Process Evaluation, in June 2023 the Justice Select Committee launched an enquiry into ‘the use of pre-recorded cross-examination under Section 28 of the Youth Justice and Criminal Evidence Act 1999’. They requested evidence on all aspects of s.28 including ‘the effect of the use of section 28 on ... juries’ and ‘the effect of the use of section 28 on the number of cases brought to trial, and on the outcome of cases’ (JSC, 2023).

Other than our analysis, the main large-scale quantitative study of the impact of s.28 was written evidence submitted by Prof. Cheryl Thomas, published via the Justice Select Committee (JSC) in response to their enquiry in December 2023 (Thomas, 2023a).

Prof. Thomas’s main reported finding was a 10% reduction in conviction rates for s.28 charges, with some variation between different offence types (*ibid.*). Prof. Thomas’s report utilised data from June 2016 to June 2023 (Thomas, 2023b). However, the analysis does not appear to account for any characteristic of the cases other than the offence (including the date or location the case is tried). As the analysis is based on charges rather than cases, the results are not directly comparable to this analysis.

Simulation-based studies have not focused on pre-recorded cross-examination specifically, rather, they compare ‘in person’ evidence to a live video-link and all-pre-recorded evidence (Ellison & Munro, 2014; Taylor & Larsen, 2005). In these cases, they found that the influence on juror decisions was variable between individual jurors, with different jurors reporting witnesses as more, or less, credible and sympathetic, and finding that there was no evidence of an overall impact on outcomes (Taylor & Larsen, 2005).

There have also been evidence reviews assessing a wider range of sources on the topics of pre-recorded evidence in general (Fairclough, 2020) and the wider suite of special

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<sup>1</sup> (an issue for ABE that is also relevant in s.28).

measures available to vulnerable and intimidated witnesses (Munro, 2018), which have tended to conclude that there is no evidence of pre-recorded evidence impacting outcomes.

## **2.2 Research Questions**

In response to the recent research and lack of quantitative evidence in this area, this analysis aims to address the following three research questions:

1. Does the use of s.28 impact the likelihood of a conviction, including in offence types with high levels of eligibility for s.28?
2. Does the use of s.28 impact the ability of the courts to function effectively, particularly whether it causes trials to be cancelled and rescheduled (i.e. the rate of ineffective trials)?
3. And how long before the main trial are s.28 pre-recorded cross-examinations recorded?

## 3. Methodological Approach

### Introduction

At the time s.28 was introduced for intimidated witnesses and was extended nationally for vulnerable witnesses, operational and legal constraints meant no arrangements could be made for a controlled trial or other formal experimental structure that would allow the impact of s.28 to be evaluated. The introduction of s.28 for vulnerable witnesses had a small-scale quantitative impact evaluation done as part of the process evaluation, but this was acknowledged as not being generalisable if s.28 was extended beyond the pilot courts (Baverstock, 2016).

Therefore, given the difficulty of collecting additional data, our analysis has been designed around the existing administrative datasets. The core of this is data on receipts and disposals, also used in published National Statistics (Ministry of Justice, 2023b). This was combined with granular data for individual Crown Court cases, and additional monitoring data used to manage the s.28 recording process.

S.28 cases form a relatively small proportion of the overall caseload, so we sought to include as much data as possible. As well as allowing the largest possible sample to be analysed, administrative data has the advantages of being collected from systems with a high degree of automation and validation, minimising the risk of errors introduced by mis-recording; subject to a high degree of scrutiny and verification so that issues that do occur can be identified and rectified; and having a large volume of curated, pre-processed fields that avoid the need for recalculation.

Unfortunately, as the data was collected for administrative purposes, key details that would have been useful in our analysis are not present. We were not able to obtain data on witness numbers or characteristics at a case level, or any means of evaluating whether a given case that did not use s.28 might have been eligible. Despite the high degree of automation and validation, as with any large administrative dataset, there is the possibility of recording errors.

Crown Court administrative data does not contain explicit markers for whether a case or trial includes a vulnerable or intimidated witness. While it is possible to infer that some classes of case (for example, those containing sex offences where the victim was a child) are likely to contain at least one vulnerable witness, there are a number of reasons why s.28 provisions might not be used in these cases (e.g. the witness might have chosen not to use special measures or to use alternative special measures, such as screens or a live link; or the ABE interview for that witness might be unusable).

As such, we are not reliably able to distinguish cases that used s.28 for vulnerable or intimidated witnesses, or cases that included a vulnerable or intimidated witness but did not use s.28.

A significant complicating factor in the analysis of the impacts of s.28 provisions was that cases that make use of s.28 do not have the same characteristics as cases that do not make use of s.28. Cases making use of s.28 typically include a sexual offence, or more rarely a violent offence. Cases including sexual offences typically take longer and some have lower conviction rates. All cases that use s.28 are using at least one other special measure (section 27 pre-recorded evidence-in-chief is a pre-requisite for s.28). It is likely that many cases are using a combination of other special measures.

Additionally, cases that are eligible for s.28 (including being in a court where s.28 is available at that point in time) but do not use s.28 may have further differences from s.28 eligible cases that do use it. Factors such as the age and development of child witnesses or the amount of trauma experienced when the offence took place or the specific advice received from police and CPS may influence the decision to make use of s.28.

Many of these factors could not be accounted for using the data available to this study, or via any administrative data regularly collected. These factors may lead to s.28 eligible cases having longer average times to trial, higher impact on court functioning or different conviction rates than non-s.28 eligible cases.

When a cross-examination was recorded for s.28 purposes, the recording was stored in a separate court computer system. We accessed monitoring data from this system containing information about when and where the recording was made, which trial the

recording was to be played in, and which witness in that trial was cross-examined. We removed all personally identifiable information from this data.

These records could then be linked to the trials they were associated with or the Crown Court that the recording was associated with.

Different methods and samples were used to address each research question; for details see Appendix 1: Data Processing, and Appendix 2: Methodology.

### **3.1 Conviction Rates**

We analysed case conviction rates, controlling for case mix (rates of Adult and Child Sexual Offences), time, and court-level effects.

Measuring whether a change in court procedure alters juries' reactions to evidence directly is difficult, due to a combination of ethical, operational, and legal constraints. In addition, there are significant barriers to arranging any sort of randomised trial structure within court processes.

How evidence is presented to the jury is important. If evidence is presented poorly, then it may lead to juries not being convinced by the strength of the prosecution's evidence and find the defendant not guilty. We are unable to examine jury perceptions directly, therefore we decided to examine whether the use of s.28 changes the likelihood that, in cases where a trial by jury has taken place, the defendant is found guilty. Whether a case made use of s.28, whether it went for trial, and what the outcome was are generally well-recorded and accessible data-points across the Crown Court.

#### **Data collection**

We sampled all cases that were received into the Crown Courts between 1<sup>st</sup> January 2019 and 8<sup>th</sup> September 2022 that had a trial by jury, and that had received a disposal. Cases received in this period that were still open at the conclusion of data collection (in May 2024) were not included.

Courts with very small numbers of trials were excluded from the study as the volume would be too low to effectively calculate the per-court impact. Where a court closed early

in the study period (before roll-out of s.28 to that court), it was also excluded. Volumes of excluded cases can be found in Table 2 and Table 3.

Some cases have multiple offences each with a different result in terms of conviction or acquittal for each offence. We have considered any case to be a conviction where at least one offence trialled in that case was found guilty by a jury. This was because we have no data to match s.28 recordings to individual offences, and therefore could not associate any element below case level to any effects from s.28.

Cases were considered to be s.28 cases where there was at least one s.28 recording associated with that case.

### **Stepped Wedge Design**

The analytical methodology that we used to examine conviction rates was a stepped wedge pseudo-experimental design. In a standard stepped wedge experimental design, participating cohorts (typically the delivery sites for the intervention) are all initially in the control group, and over the course of the experiment random cohorts move to the treatment group (i.e. begin to use whatever intervention is being tested), with the final measurements in the experiment taking place once all cohorts are in the treatment group. While no cohort can effectively be compared against itself under the same conditions, a stepped wedge experiment allows for comparison between each individual cohorts before and after the intervention was applied, and between different cohorts with and without the intervention at the same point in time. Because of the large number of individual comparisons, a stepped wedge experiment also allows for higher confidence to be obtained from a smaller sample of treated cases (Woertman et al., 2016).

**Table 1: Structure of stepped wedge design**

Group	Pre-rollout	Rollout					Post-rollout
6 +25 courts							83 courts using s.28
5 +21 courts						58 courts using s.28	
4 +25 courts					37 courts using s.28		
3 +5 courts				12 courts using s.28			
2 +4 courts			7 courts using s.28				
1 +3 courts		3 courts using s.28					
<b>Wave</b>	<b>Wave 0</b>	<b>Wave 1</b>	<b>Wave 2</b>	<b>Wave 3</b>	<b>Wave 4</b>	<b>Wave 5</b>	<b>Wave 6</b>
<b>Dates</b>	January to June 2019	3 June 2019 to 29 Sept 2021	20 Sept 2021 to 30 March 2022	31 March 2022 to 11 May 2022	12 May 2022 to 28 June 2022	29 June 2022 to 8 Sept 2022	9 Sept 2022 to 31 August 2023
<b>Days in wave</b>	152	823	182	42	48	72	356

Table 1 shows the control (light blue) and treatment (dark blue) steps of the stepped wedge design, with the number of courts added to and using s.28 provisions at each step, as well as the dates and length of each step.

For a full list of Crown Courts in each wave see Appendix 1.

Fortunately, while this was not an intended part of the design for the roll-out of s.28, the model of introducing s.28 to one group of Crown Courts at a time closely follows the design for a stepped wedge experiment. In addition, the lower sample size requirements of a stepped wedge are a good fit for the small overall number of cases making use of s.28 provisions.

Under ideal experimental conditions, which group of courts would begin treatment in each wave should be randomised. The order in which courts went live was decided on an operational basis; other than special provisions for the first and last groups, assignment to waves was broadly geographical and not linked to any other court characteristics. Following independent expert advice, we decided that a stepped wedge still allows the best possible chance of detecting relevant impacts from the roll-out of s.28 of intimidated witnesses.

As the rollout was completed in September 2022, our ability to compare groups with different levels of s.28 provision effectively ended with the final group of courts adopting s.28 for intimidated witnesses; from this date differences in s.28 provision reflect different local circumstances, and further comparisons would require additional data and a different analytical approach.

### **Control variables**

To estimate the impact of s.28 on conviction rates, it was necessary to control for other factors that have an impact on conviction rates (see Appendix 2, section 2A for model specifications). The following were identified as primary elements for which we were able to control:

1. The individual Crown Court. Each Crown Court was assessed as having a different background conviction rate, reflecting local circumstances specific to that court such as differing local demographics impacting juries, differences in local police or court practice, differences between judges etc. Courts were then grouped according to the date of rollout, to control for differences between courts that were rolled out early and those that rolled out later.
2. The rate of sex offence (SO) cases within the cohort. Sex offences have multiple characteristic differences from other types of case, such as a lower rate of guilty pleas (Ministry of Justice, 2023b). Witnesses in SO cases and trials are more likely to be eligible for s.28, so without considering the rate of SO separately it is possible to attribute impacts of high SO rates to s.28. Additionally, due to further differences in s.28 eligibility between cases with Adult and Child SO, the rates of Adult and Child SO were examined separately.

3. The month that the cohort related to (month of case receipt). Our initial analysis suggested that the conviction rate was not static over time. As s.28 uptake increased over the period of the study (both due to new courts rolling out provision and due to increased uptake over the bedding in period for individual courts), it was necessary to separate the background trend in conviction rates from any change driven by s.28.

Because the sample sizes would be too small, no separate analysis was done for any offence type below Adult and Child Sexual Offences.

### **Limitations**

One possible effect of the roll-out of s.28 to a wider group is that some cases that otherwise would not have come to trial could now be tried (Ward et al, 2023). This reduced witness attrition would increase the number of cases coming to trial. Pre-recording of cross-examination may also alter defendant plea behaviour: a defendant may plead differently, or at a different time, than if the cross-examination was still outstanding (Baverstock, 2016).

Unfortunately, the data did not allow us to distinguish these added or removed cases. There is a risk here that these cases have different characteristics to a typical case; they may be weaker 'edge' cases that are less likely to result in conviction, or they may be stronger cases. The data did not allow us to control for these effects, but the total number of cases added or removed this way was likely to be small, given the low volume of s.28 cases overall.

We had limited data on special measures other than s.28. Some special measures can or must be used in conjunction with s.28, while other special measures effectively function as alternatives to s.28. As we were unable to distinguish which cases made use of special measures in general (or were eligible but did not make use of special measures), we have had to assume that there were no substantial changes in uptake or efficacy of special measures over the study period. If there was another change in special measures (for example, greater availability of intermediaries, or a change in quality of ABE recordings), this may have introduced bias into our findings. Although the data that we do have

suggests that the prevalence of interventions other than ABE and s.28 is relatively low, it covers too small a period and geographical area to be considered definitive.

Further, analysis of conviction rates requires cases to have reached a verdict, which for many cases takes three years or longer (MoJ, 2023b). Cases that take longer to reach a trial and verdict are likely to be more complex, and therefore have different characteristics to shorter trials. To avoid this source of bias, cases that were received less than eleven months before the last available outcome data were removed from the sample.

### Summary statistics

449,390 cases were received into Crown Courts in England and Wales over the period 1<sup>st</sup> January 2019 to 8<sup>th</sup> September 2022, of which 71,288 (16%) went on to have a jury trial and verdict in the data that was collected.

Provisions to exclude very small or closed courts removed 380 cases and leaving 70,908, 16% of the original sample.

**Table 2: Conviction rate sample volumes January 2019 to September 2022**

	Number	Percentage of all receipts
All Receipts to Crown Court 01/01/19 to 08/09/22	449,390	-
Cases with a Trial and Verdict	71,288	15.9%
Cases not from a small or closed Crown Court	70,908	15.8%

Our initial sample for s.28 data contained 4,241 unique Crown Court cases where s.28 was used. Of these s.28 records, 92% were linked to a record of the same case in the Crown Court records. Of these cases, 67% went to trial, 55% were cases received in the study period (1<sup>st</sup> January, 2019 to 8<sup>th</sup> September, 2022), and 52% were disposed by the end of the study period. 2,169 (51%) of all s.28 cases were in the final sample.

**Table 3: Sample S.28 cases January 2019 to August 2023**

	Number	Percentage of all s.28 cases
Total s.28 cases	4,241	-
S.28 cases not linked to Crown Court data	342	8.1%

	<b>Number</b>	<b>Percentage of all s.28 cases</b>
S.28 cases linked to Crown Court cases without a Trial	1,048	24.7%
S.28 cases linked to Crown Court cases received outside the study period	526	12.4%
S.28 cases linked to Crown Court cases without an outcome	110	2.6%
S.28 cases falling between rollout groups	46	1.1%
S.28 cases in final sample	2,169	51.1%

Full details of the sample can be found in Appendix 1.

### 3.2 Trial Ineffectiveness

To test whether s.28 had a negative impact on scheduling and court listings (as set out in the introduction) the analysis considered changes to the rate of ineffective trials in Crown Courts, controlling for caseload mix, court-level trends, and (where possible) external pressures that differ over time.

As with jury perceptions of a witness, disruption to court processes from s.28 is not measured directly. The process of scheduling court time and resources to maximise efficient use while also managing appropriate prioritisation is complex and affected by a wide range of factors. While forecasting methods can be used to determine the expected additional time commitment in a case using s.28, it cannot effectively predict disruptions that are the result of individual practitioners being unavailable for other work at short notice, which is a noted effect of s.28 (Ward et al., 2023).

#### Data collection

There was no manual collection of data on court disruption during the roll-out of s.28 to intimidated witnesses, as it would have presented operational challenges to collect data at the scale required for robust analysis.

The impact of s.28 on court efficiency is likely to be felt by non-s.28 cases as well as s.28 cases; given that cases involving sexual offences and vulnerable witnesses are likely to be prioritised in the case of conflicts, non-s.28 cases may be more impacted. If this type of

disruption occurs more frequently with more s.28 usage, and is not effectively accommodated by the court process, then this increased disruption should be measurable as disruption across the work of the whole court even if s.28 cases are not individually more likely to suffer disruption. Therefore, data had to be collected at a whole-court level.

Total volumes of court work are subject to substantial, often long-term fluctuations (for example as capacity at a given court is increased or decreased); many of these changes will not represent disruption but planned changes to capacity. In addition, there is no single well-understood definition of total court work as a single measure. As such, measuring the impact of s.28 on total court work is not feasible.

### **Trial Ineffectiveness**

Published statistics report the efficiency of courts using a set of measures around trial effectiveness, which is a measure of the proportion of planned trials that go ahead.

Planned trials are classed as either effective (i.e. going ahead at their scheduled time), cracked (i.e. becoming unnecessary at their scheduled time, principally due to late guilty pleas), or ineffective (i.e. still required at their scheduled time, but unable to go ahead usually for reasons of availability or readiness).

Correspondingly, process evaluations of s.28 have described disruptions caused by practitioners being required to attend an s.28 trial on short notice or in place of another trial appointment (Ward et al., 2023). Therefore, we believe the rate of ineffective trials (the proportion of trials scheduled for a given day that were not able to go ahead) is a suitable metric to analyse whether s.28 has impacted whole-court operations. Unlike the analysis of conviction rates, we did not require the cases associated with trials to pertain to cases received in a particular window, or to have reached a verdict. Vacated trials (i.e. trials that were cancelled or rescheduled ahead of time) were not included in the count of trials, but otherwise all trials scheduled to be heard between 1<sup>st</sup> January, 2019 and 31<sup>st</sup> August, 2023 were included in the sample.

To reflect the volume of additional work and therefore potential for amount of disruption created by s.28 cases, the analysis of trial ineffectiveness used the number of witnesses recording their cross-examination in each month in each court. All witness records that

linked to a valid Crown Court were included in the sample, regardless of whether the s.28 record could be definitively linked to a specific case record at that court.

As with the conviction rates analysis, courts with very small numbers of trials, or where the court closed early in the study period, were excluded from the study; see Table 4 and Table 5.

### S.28 Ratio

We expected that the level of disruption experienced from s.28 work would scale with the court size. To reflect this, the volume of s.28 work undertaken in a given court was expressed as the 's.28 ratio'. This was calculated as the number of s.28 witnesses making at least one recording in that court in that month, over the expected number of trials held in that court in that month (the 's.28 Ratio'). The expected number of trials was given by a smoothed rolling average.

**Figure 1: S.28 ratio distributions post roll-out (highest and lowest four courts)**

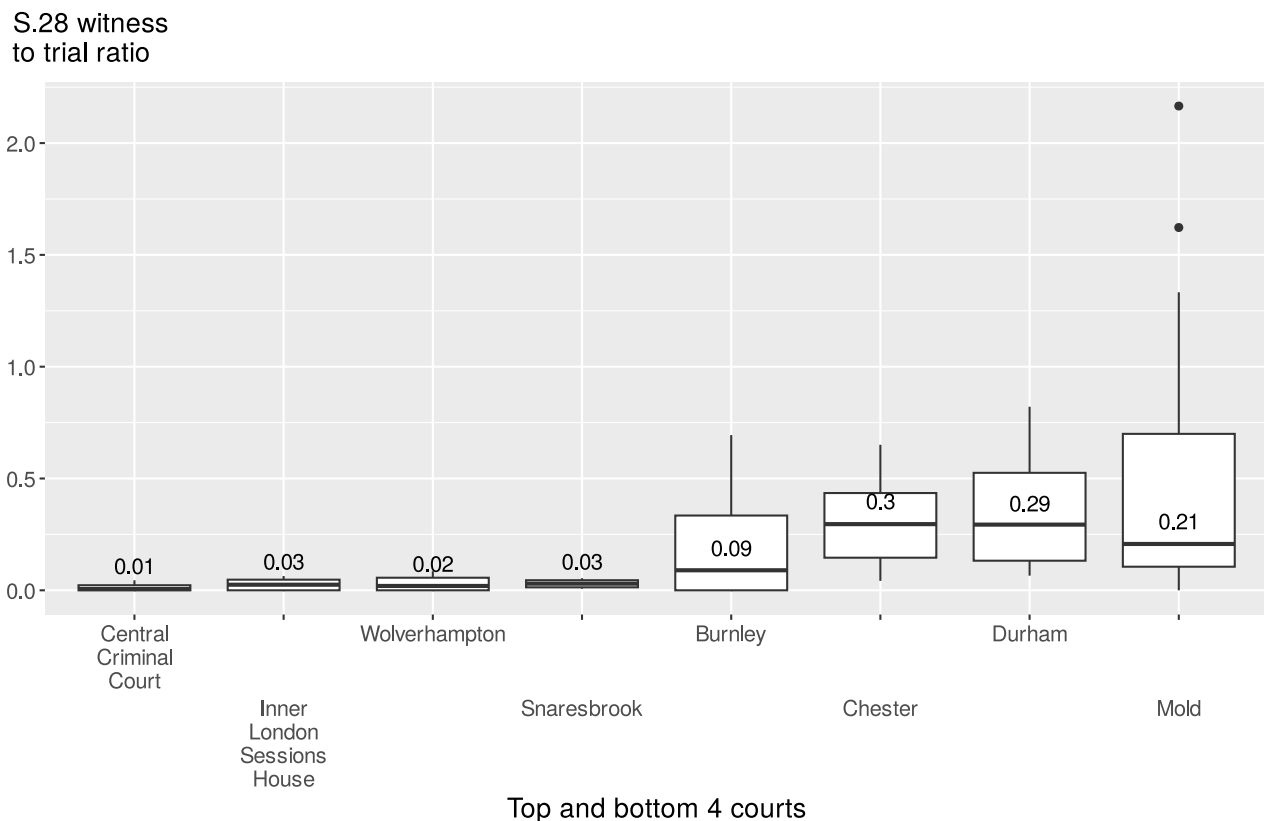


Figure 1 shows the distribution of s.28 ratios across the courts with the highest and lowest ratios, showing the range of values in the sample data. Durham and Chester Crown Courts have around one witness making an s.28 recording per three trials each month, while Wolverhampton and Snaresbrook Crown Courts have around one s.28 witness recording per 35 trials. The lower rates of s.28 in courts with lower ratios also result in relatively lower variation between months.

As trial ineffectiveness was studied from the same roll-out and under largely the same constraints as conviction rates, the same stepped wedge design is being used (see Table 1). Appendix 2, section 2B contains more details on data distribution.

### **Control variables**

To estimate the impact of s.28 on rates of ineffective trials, it was necessary to control for other factors that impact trial ineffectiveness (see Appendix 2, section 2B for model specifications). The following were identified as primary elements, and controlled for:

1. The individual Crown Court. Each Crown Court was assessed as having a different background rate of ineffective trials, reflecting circumstances specific to that court such as the flexibility of the building and available ICT. Courts were further grouped according to the date of rollout, to control for differences between courts that were rolled out early and those that rolled out later.
2. The rate of sex offence (SO) cases within the cohort. As with the conviction rate analysis, this had to be controlled for, to avoid conflating impacts of high SO volumes with high s.28 ratios.
3. The month that the cohort related to (month of trial). Trial ineffectiveness rates are highly responsive to external disruption of the court, and both the COVID-19 pandemic and subsequent court shutdowns and the industrial action by the bar council produced substantial changes in background rates of trial ineffectiveness.
4. The number of months from the roll-out of s.28 provision to intimidated witnesses in each court cohort. Changes to court procedures, especially ones that produce additional demands for listing, are likely to produce short-term impacts on trial effectiveness while the changes are adapted to. We wished to separate as far as possible the impact of introducing new s.28 processes from the impact of the

processes themselves, and controlling for how long a given court had been using s.28 at each measurement point allowed us to do that.

### **Limitations**

As previously noted, s.28 has the potential to add or remove cases, and therefore trials, from the caseload. If these trials had characteristics that would make them more or less likely to be or cause an ineffective trial than other cases on the caseload (e.g. if added or removed cases are more complex; require higher prioritisation; or require more or more specialised representation), this could have introduced bias into the findings.

There is also a risk that courts earlier in the roll-out, especially the pilot courts, represent unusually well-prepared courts, may have received extra support, and may have encountered a disproportionate number of early operational difficulties that will have been resolved before later cohorts. We try to control for this by considering per-court effects, but it may nevertheless have introduced some bias into the data.

The study period includes two substantial disruptions in normal court processes. The COVID-19 pandemic initially closed all courts, with a large number of trials being classed as ineffective during this period. When limited court activity resumed, the lower volumes of trials listed in courts operating substantially below normal capacity resulted in comparatively low trial ineffectiveness. The industrial action by the Bar Council also resulted in rapid changes in background trial ineffectiveness rates. These effects have been controlled for but are still likely to have substantially increased the level of uncertainty in the findings.

### **Summary statistics**

102,119 trials were held in the Crown Courts from January 2019 to August 2023; excluding small and closed courts removed 672 trials, 0.7% of trials.

5,673 witnesses recorded cross-examinations in the data between January 2019 to August 2023, of which 29, 0.5% are excluded due to either matching a small or closed Crown Court, or not being matched to any Crown Court.

**Table 4: Sample trials January 2019 to August 2023**

	<b>Number</b>	<b>Percentage of all trials</b>
Total Trials	102,119	
Trials at small or closed courts	672	0.7%
Trials in Final Sample	101,447	99.3%

**Table 5: Sample s.28 witnesses January 2019 to August 2023**

	<b>Number</b>	<b>Percentage of all s.28 cases</b>
Total s.28 Witnesses	5,673	
Witnesses at small Courts, closed Courts, or unknown Courts	29	0.5%
s.28 Witnesses in Sample	5,644	99.5%

### 3.3 Time to Cross-examination

During the process evaluation of s.28 (Ward et al., 2023), practitioners and witnesses were agreed on the positive effects of earlier cross-examination on witness engagement and well-being (*ibid.*, pp.15–17, 30–51). However, the practitioners were also concerned that requiring the same judge and counsel for s.28 hearings would lead to scheduling difficulties and increased administrative work (*ibid.*, pp.20–23), which could have a knock-on effect on all types of trial hearing dates.

The analysis tested the occurrence and effects of early, pre-recorded cross-examinations on hearing dates by looking at court data from January 2019, when the rollout of s.28 began, to August 2023. We used a custom dataset for this analysis, separate from the data used for the conviction rates and effectiveness analyses, as it focused on cross-examination and trial dates within the study period rather than receipts or trial scheduling.

The work done in the timeliness analysis differs from the regression analysis in the previous sections, being indicative rather than confirmatory. These analyses aim to identify patterns and trends in the data (Cooksey, 2020) by describing typical timelines, ranges and variability, and levels of difference between groups of cases. (See 4.3 Time to Cross-

examine (findings) for full details.) Unlike the regression analysis, no controls were put in place for factors specific to individual Crown Courts, and only limited controls were used to account for trends over time not related to s.28 (See 4.3 and Figure 3)

Most cases with a s.28 recording are violent or sexual offences (they have a most serious offence (MSO) classified as 'Violence against the person' or 'Sexual offences'). This distribution is expected, as specific conditions must be met to qualify witnesses for special measures, which include s.28 pre-recordings (for a summary of the conditions see the 'Eligibility for Special Measures' section, CPS, 2023).

This can be seen in Table 6, in which a sexual offences 'most serious offence' makes up the bulk of cases with a s.28 hearing, followed by violence against the person with just over a tenth as many. Of total sexual offences, 24.9% have an associated s.28 hearing, and cases of violence against the person have 2.6%. The volume of s.28 cases with an MSO that was not a violent or sexual offence was too small to be useful in the analysis.

To assess these elements, data from standard Crown Court systems were accessed via SQL query to standardised management information system databases. S.28 recording metadata were locally cleaned and pseudonymised, then the pseudonymous dataset was addressed via SQL. These combined datasets were used to find the earliest hearing record for cases of violence against the person or sexual offences with a 'not guilty' plea. We excluded 'guilty' and 'no plea entered' cases as these do not have an associated trial hearing. These datasets were separated into cases with a s.28 hearing, and cases with a first hearing at trial (i.e. non-s.28 cases). A s.28 hearing should occur before a trial sitting (see the background section earlier in this report). Judges considering requests for s.28 special measures need to be assured that scheduling one will "materially advance the date for cross-examination and re-examination" (Crown Prosecution Service, 2023).

Although this analysis uses a different dataset than the work on outcomes, it has many of the same limitations (see section 1 Executive Summary above). Primarily, that the datasets were defined as the period of the rollout, which began on a restricted basis in January 2019. The records end in August 2023 due to a technical change in the pre-recording system, but as there is usually a delay of a year or more between receipt and main trial this will not exclude many s.28 cases. This does, however, reduce the

number of courts earlier in the sample, and this increases uncertainty seen in the analyses in terms of the margins of error. As the focus of these analyses is relatively narrow, it is also likely that there are systematic differences, e.g. at court, practitioner, or witness level, not captured.

**Table 6: Full breakdown of numbers of cases by most serious offence, with and without s.28, January 2019 to August 2023**

<b>Most serious offence</b>	<b>S.28 recorded</b>	<b>No s.28 recording</b>
Sexual offences	3,179 (24.9%)	9,595 (75.1%)
Violence against the person	341 (2.6%)	12,874 (97.4%)
Miscellaneous crimes against society	35 (1.1%)	3,124 (98.9%)
Robbery	25 (1.3%)	1,942 (98.7%)
Public order offences	20 (1.9%)	1,044 (98.1%)
Theft offences	12 (0.6%)	2,113 (99.4%)
Possession of weapons	11 (0.7%)	1,654 (99.3%)
Drug offences	7 (0.2%)	4,469 (99.8%)
Fraud offences	7 (1%)	695 (99%)
Criminal damage and arson	5 (0.9%)	554 (99.1%)
Summary non-motoring	1 (1.6%)	60 (98.4%)
Not known	1 (0.7%)	143 (99.3%)
Summary motoring	0 (0%)	20 (100%)
<b>Total</b>	<b>3,644</b>	<b>38,267</b>

This functionally limits the analyses to the first two categories, as there are not enough trial records to draw conclusions about hearings in the other classifications.

Similarly, a very small number of cases do not yet have trial hearing dates (238, or 0.5% of records) and so cannot be included in the analyses (see Appendix 2, section 2C for a full description of cases which do not have a trial date). All the dates used in this analysis have already happened and so reflect actual, rather than scheduled, dates.

Finally, as detailed in earlier sections, court functioning (and therefore timeliness) was affected during the period under analysis by court closures and industrial action. While it was feasible to include potential effects of the pandemic closures using an agreed national

timeline (see the analysis section for details), the industrial action was intermittent (Burns, 2022) and the effects hard to measure, making it impossible to isolate using the data we have.

This analysis should also be seen in the context of the other results in this report.

Timeliness is not the only measure; for example, witnesses rated themselves as more likely to proceed with testimony using a pre-recorded cross-examination (Ward et al., 2023, p.54).

Personally identifiable data were used only to generate unique pseudonymous identifiers for onward processing. All other data were processed as high-level aggregate data to avoid any risk of identification. There should be no risk to any court user from having their data included in this analysis.

## 4. Findings

### 4.1 Conviction Rates

We analysed how different levels of s.28 usage in courts impacted the likelihood of conviction for the period January 2019 to September 2022.

For the sample as a whole, conviction rates for s.28 cases were slightly lower than for non-s.28 cases. Conviction rates for all sexual offences cases appear higher for s.28 cases.

**Table 7: Unmodified conviction rates by s.28 status and offence type for study sample, January 2019 to September 2022**

	<b>S.28 Cases</b>	<b>Non-s.28 Cases</b>
All Cases	63.9%	64.7%
All Sexual Offences	64.0%	60.4%
All Adult Sexual Offences	54.8%	53.0%
All Child Sexual Offences	67.9%	70.0%

Not all factors potentially impacting convictions could be controlled for. However, once we controlled for the impact of the conviction rate over time, court-level impacts, and changes in the rate and type of sexual offence cases (see 3.1), there was no evidence that s.28 has an impact on conviction rates. For full statistical results see Table 12 in Appendix 3, section 3A.

Looking at all offence types (but correcting for the impact of Adult SO and Child SO), the impact of s.28 usage was calculated very close to zero. A more detailed breakdown of analyses is in Appendix 3, section 3A.

This result is equivalent to the result we would have expected if s.28 had no impact on conviction rates in either direction, or a mixed impact where cases convicted because of s.28 are balanced by other cases acquitted because of s.28. This means that the effect of s.28 was too small for us to reliably say that an effect exists.

When considering only Sexual Offence cases, the result was similar that the effect was too small to distinguish from random noise.

Analysis was also made of the cohort of only Child Sexual Offences and only Adult Sexual Offences. The total number of s.28 cases in each of these analyses was too few for the analysis to be considered reliable, but the results were comparable to the larger sample sizes.

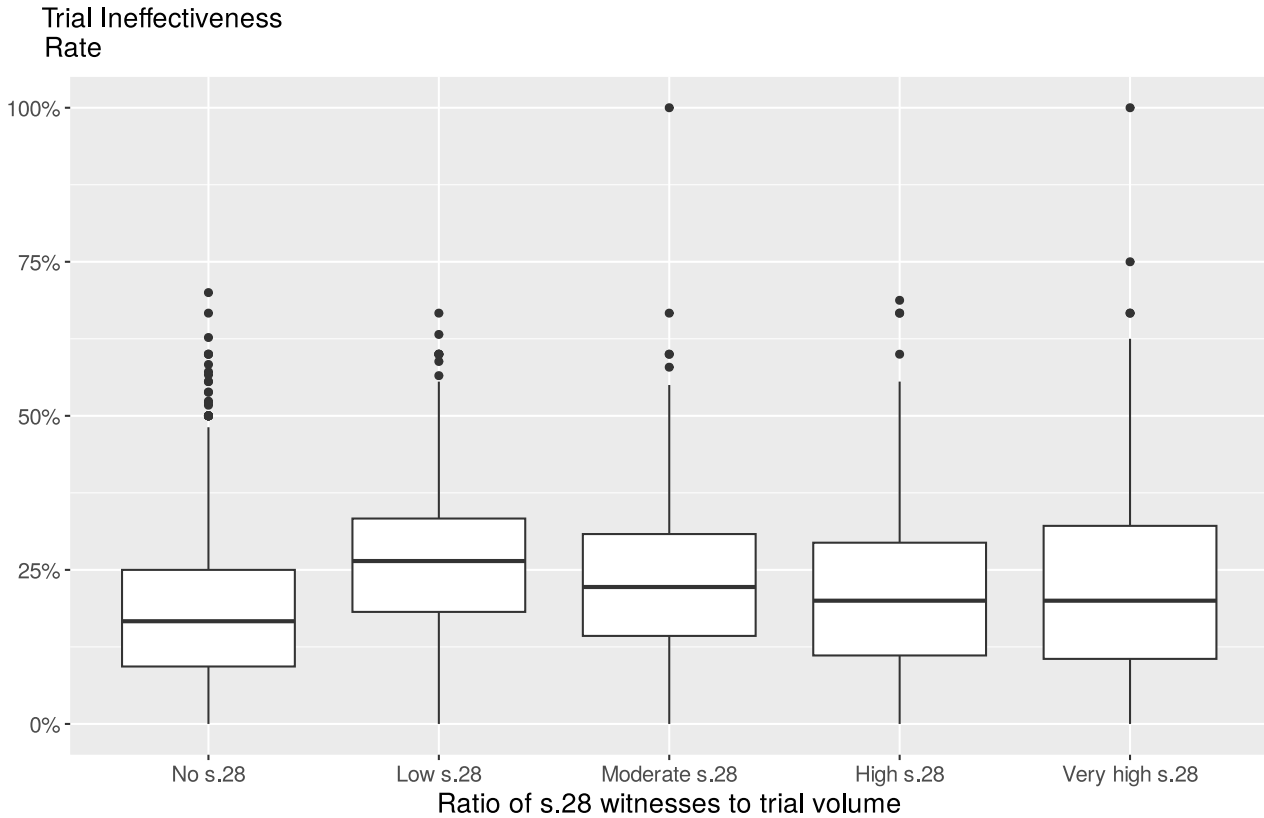
## 4.2 Trial Ineffectiveness

Trial ineffectiveness was analysed by considering how different ratios of s.28 recording work in courts each month impacted the rate at which trials were ineffective (i.e. unable to go ahead). For the sample as a whole, the median trial ineffectiveness was lowest where no s.28 cases occur and highest where a low ratio of s.28 witnesses occur in a month.

**Table 8: Trial average monthly trial ineffectiveness by s.28 ratio, January 2019 to August 2023**

<b>Ratio of s.28 trials</b>	<b>Median Trial Ineffectiveness</b>	<b>Mean Trial Ineffectiveness</b>
No s.28	16.7%	17.7%
Low s.28 (less than 1:10)	26.4%	26.3%
Moderate s.28 (1:10 to less than 1:5)	22.2%	23.0%
High s.28 (1:5 to less than 1:2)	20.0%	20.7%
Very high s.28 (1:2 or greater)	20.0%	25.3%

**Figure 2: Boxplot showing Trial Ineffectiveness Distribution by s.28 ratio January 2019 to August 2023**



In Figure 2, the box represents the trial ineffectiveness rate for the middle 50% of cases. The line across the box centre represents the median trial ineffectiveness rate. The line extending below the box represents the bottom 25% of the data to the minimum point. The line extending above the box represented the top 25% (or so) of the data up to the maximum. The dots above the top of the upper line are the outlier cases.

While courts with no s.28 recordings in that month reported the lowest Trial Ineffectiveness, the unadjusted data for January 2019 to August 2023 showed no clear trend of Trial Ineffectiveness rising with increased s.28 ratios.

We controlled for the impact of the factors discussed in the methodology section: individual Crown Courts; rate of sex offence (SO) cases within the cohort; month of trial; number of months from the s.28 roll-out; see Control variables under methodology for full details.

When this was done, we found no evidence that increasing rates of s.28 usage has a negative impact on trial ineffectiveness across the Crown Court system. Courts where

more s.28 cross-examinations were recorded saw a similar rate of ineffective trials to courts with little or no s.28 recording (see Table 14 in Appendix 3, section 3B for detailed results).

This was consistent with s.28 recordings not causing trials to become ineffective, or with s.28 having an impact on trial effectiveness that is too small to quantify from available data.

However, in the first few months post rollout there appeared to be a minor increase in the percentage of ineffective trials. This finding may partly explain the observations made shortly after the roll-out during previous studies (Ward et al, 2023), when practitioners may have experienced a noticeable amount of disruption. Appendix 3, section 3B contains a more detailed breakdown of these analyses.

### **4.3 Time to Cross-examination**

This section examines the influence of having a s.28 hearing on trial and first hearing dates.

All cases are received in the Crown Court, and at an appropriate time a main trial date is set. An s.28 case would typically have an earlier s.28 hearing date for the pre-recorded cross-examination, followed by a main trial. A non-s.28 case would just have a main trial.

The first hearing date is the date of the s.28 hearing, which marks the start of the trial for s.28 cases (Criminal Practice Directions, 2023: 6.3.36). The start date is the date of the main trial for other cases. So the time to cross-examination in a s.28 case is the time from receipt of the case in the crown court to the s.28 hearing. The time to cross-examination in a non-s.28 case is the time from receipt of case in the crown court to the main trial.

We were testing:

- If s.28 pre-recorded cross-examinations were occurring earlier than first hearings at trial for cases not using s.28. That is, are s.28 special measures “materially advancing” (CPS, 2023) cross-examination dates for these cases.

- If there was a difference in time taken to main trial hearing dates for cases with s.28 cross-examinations versus those without. Are they being pushed back or held around the same time?
- If there were effects from the COVID-19 court closures on time to cross-examination and trial (for both groups). We followed the timeline of the Select Committee on the Constitution (2021), with full closure set as being from 17th March 2020 until 18th May 2020, rounded to the start and end of the month respectively to be more inclusive of pandemic effects. COVID-19 impacted cases are cases where the interval between receiving the case and the first cross-examination falls partly, or wholly, within this period.

Comparing the date of the earliest hearing with the case receipt date allowed us to estimate average time to first hearing. The results were split into two groups, cases with a scheduled s.28 pre-recorded cross-examination, and cases without, where the cross-examination would take place at trial. The first analysis in Table 9 shows the results without excluding cases affected by the pandemic closures. The numbers are taken from the timeliness dataset described in the methodology section.

**Table 9: Dates of s.28 cross-examinations versus trial cross-examinations, INCLUDING those affected by the COVID-19 closures, January 2019 to August 2023**

<b>ALL CASES WITHOUT COVID-19 EXCLUSION</b>	<b>S.28 cross-examination</b>	<b>No s.28 cross-examination</b>
Total number of cases	3,310	38,287
<b>1. Mean days to s.28 hearing</b>	<b>175</b>	--
Median days to s.28 hearing	145	--
<b>2. Mean days to trial hearing</b>	<b>368.3</b>	<b>333.8</b>
Median days to trial hearing	329	263
<b>3. Mean days cross-examination brought forward by s.28</b>	<b>193.3</b>	--
Median days brought forward by s.28	158	--

Comparing the mean days elapsed between case receipt and a s.28 pre-recording (175 days), to the mean days for cases with a s.28 pre-recording to come to trial (368.3 days), gives the mean days that a cross-examination was brought forward in s.28 cases. So for

cases with a s.28, the date of the cross-examination is an average of 193.3 days earlier, compared to the date of the first trial hearing for the s.28 cases.

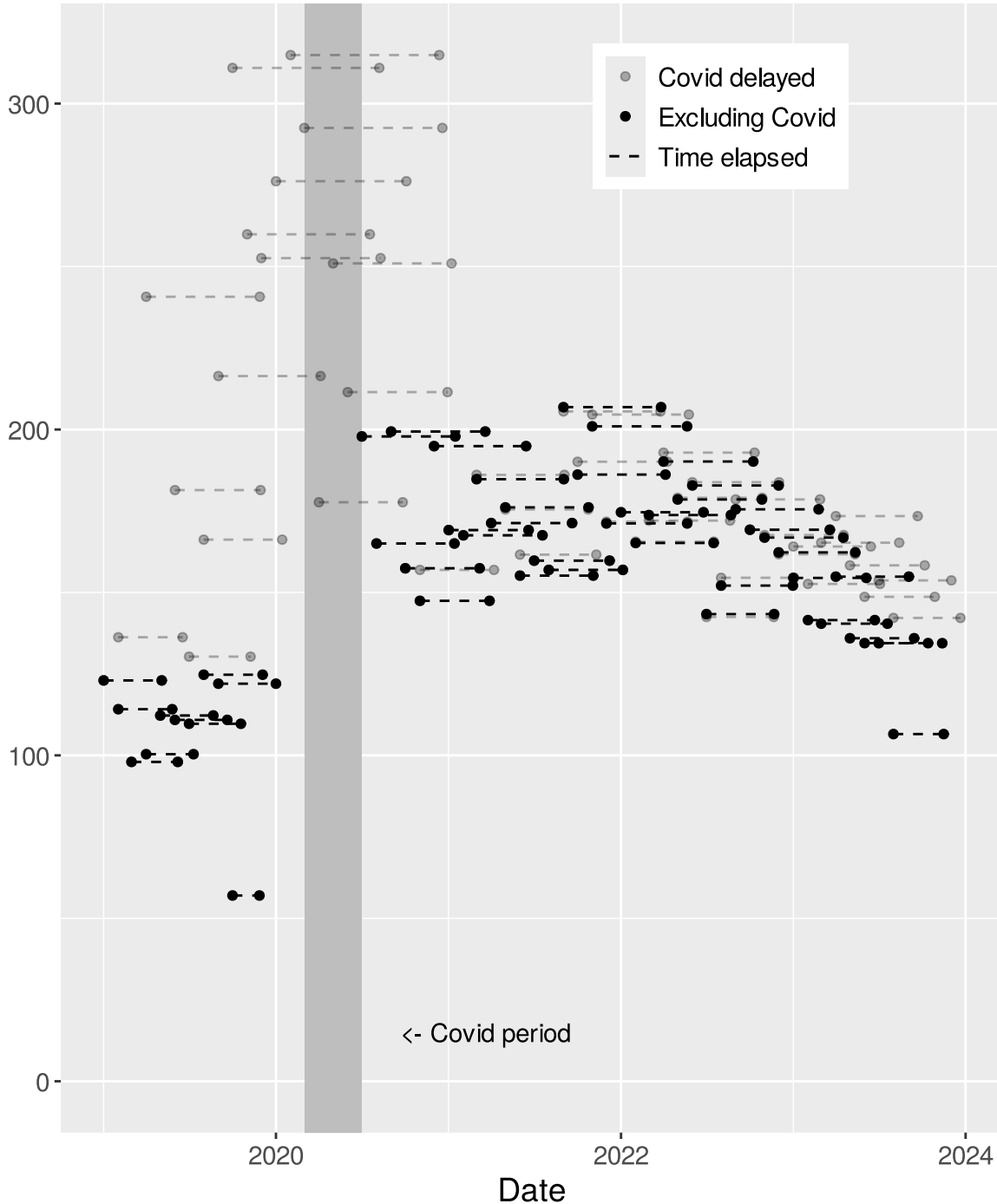
However, the average days to the first trial hearing differed between s.28 and non-s.28 cases. For s.28 cases, the average time to the main trial was 368 days. For cases without s.28 measures, the main trial date was around 34 days earlier, at 334 days. So compared to a trial without s.28, the s.28 cross-examination was brought forward by 158.8 days.

This difference could be explained by the variation between cases, as the corresponding median values (a measure of the most commonly occurring number of days) and standard deviations (a summary of the range of observations, in this case the length of time between case receipt and first hearing - see Appendix 3, section 3C) differ, sometimes notably. This means that the average estimates are quite uncertain, but most social scientists recognise that a large amount of uncertainty is common in these cases (Cahill-O'Callaghan & Mulcahy, 2022). Therefore, reducing "a complex socio-legal context to measurable, quantifiable indicators" often requires a recognition that results are exploratory or indicative, requiring us to draw inferences about things that are not directly observable.

Nonetheless, in this instance there was at least one observable influence, that of the court closures due to COVID-19. Figure 3 shows the potential influence of excluding s.28 cases that 'passed through' the COVID-19 closure period, that is, cases where the interval between receiving the case and the first cross-examination falls partly or wholly within the period represented by the grey vertical bar.

**Figure 3: Crossover of the COVID-19 period and its effects on time to trial in s.28 cases, January 2019 to August 2023**

Mean days to cross-exam by month



In Figure 3, all cases have a s.28 cross-examination. The first dot represents the case receipt and the second the cross-examination. The paler dots represent the full data, and the darker dots the data with COVID-19-crossover removed. Because these are averages,

some of the paler dots that do not appear to cross into the COVID-19 period have some overlap in one or more individual cases, and so appear lower following the exclusion.

COVID-19-impacted cases appear to have longer times (the length of the lines) between the case receipt and the date of the cross-examination. We estimated the influence of this by excluding cases where the interval between receiving the case and the first cross-examination falls at least partly within the period 1<sup>st</sup> March, 2020, when the courts were about to close, and 30<sup>th</sup> May, 2020, when the courts were gradually reopening. These results can be seen in Table 10.

**Table 10: Dates of s.28 cross-examinations versus trial cross-examinations, EXCLUDING those affected by the COVID-19 closures, January 2019 to August 2023**

<b>WITH COVID-19 EXCLUSION</b>	<b>S.28 cross-examination</b>	<b>No s.28 cross-examination</b>
Total number of cases	2992	30731
<b>1. Mean days to s.28 hearing</b>	<b>166.1</b>	--
Median days to s.28 hearing	143	--
<b>2. Mean days to trial hearing</b>	<b>356.1</b>	<b>294.2</b>
Median days to trial hearing	322	232
<b>3. Mean days brought forward by s.28</b>	<b>190</b>	--
Median days brought forward by s.28	157	--

In these results, s.28 cross-examination for the COVID-19 excluded sample tends to occur nine days earlier (166 days) compared to the COVID-19 inclusive sample (175 days). This suggests COVID-19 was responsible for at least some of the delay. Average days to main trial for s.28 cases has dropped by about 12 days.

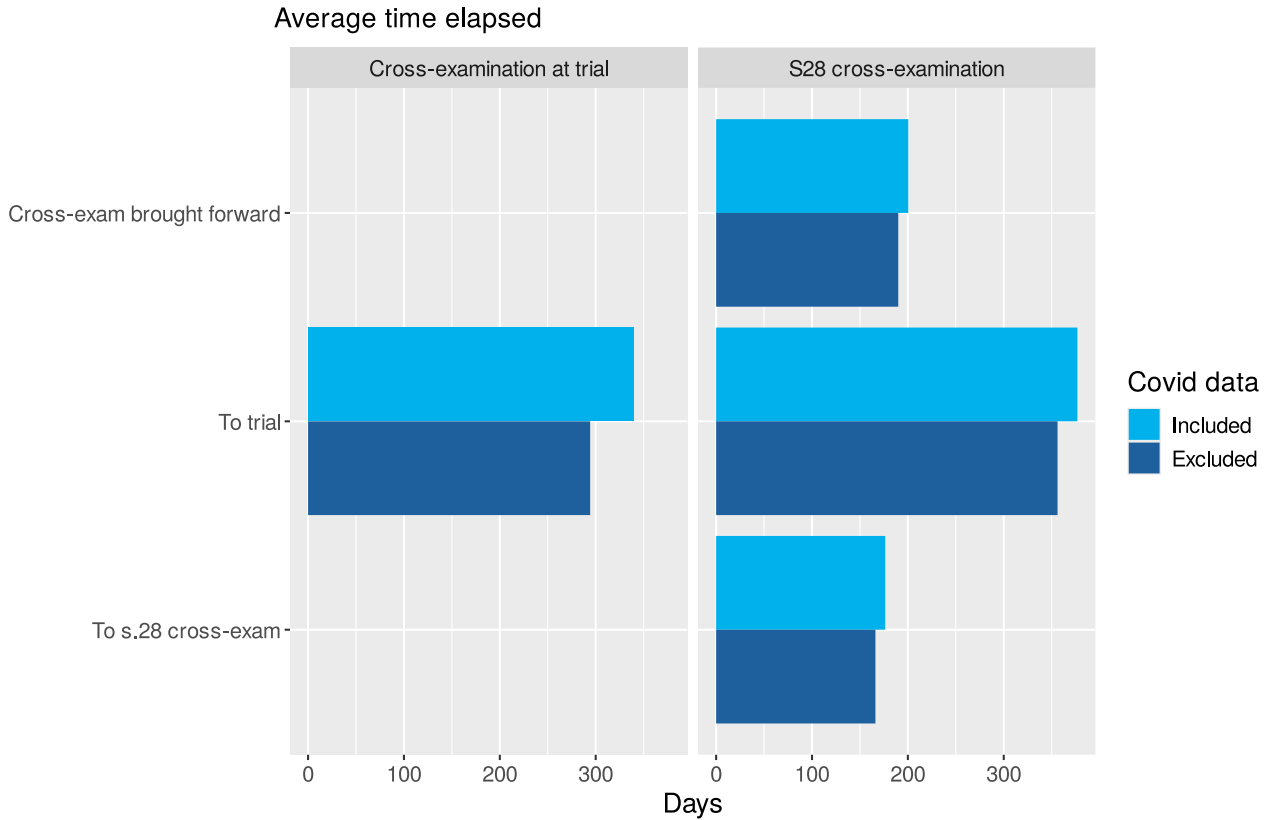
The average days the cross-examination has been brought forward under s.28 cases, has not changed much in the COVID-19 excluded sample, reducing from 193 days to 190 days. The standard deviation has dropped by around five days (see Table 15 and Table 16 in Appendix3, section 3C for a detailed breakdown). This suggests that the averages are reasonably informative, given the variation we expect to see in analyses of these types of data (Cahill-O'Callaghan & Mulcahy, 2022).

Interestingly, times to trial for cases without s.28 special measures have dropped by around 40 days in the COVID-19 excluded sample. This makes the main trial dates for s.28 cases an average of 62 days later, compared to trials for cases without s.28 compared. This is an increased delay of 28 days compared to the COVID-19 inclusive sample.

One possible interpretation of this change is that s.28 cross-examinations, whose times did not change as much, were prioritised during the period affected by closures. At that time, the same number of s.28 cases would have used a comparatively larger share of the reduced available court capacity, relative to full court capacity. This would have caused cases without s.28 measures to take longer to come to trial. Figure 4 shows the comparison a bit more clearly. Here the left chart, cases without s.28 special measures, shows a fairly large difference between the two sets of data, and the s.28 chart on the right indicates smaller differences, including cross-examination dates and days brought forward.

However, this still means that without the effect of the COVID-19-related court closures, s.28 pre-recorded cross examinations take place 128 days earlier than cross-examinations in non-s.28 cases. On average, the s.28 hearing took place 190 days before the main trial in s.28 cases.

**Figure 4: Changes between times to first hearing with and without the likely COVID-19 data excluded, January 2019 to August 2023**



Looking at the trends in the data, it appeared very likely that s.28 cross-examinations tend to occur four to five months earlier than non-s.28 cross-examinations, comparing s.28 cases to cases without. This is a substantive number despite the large amount of uncertainty and seemed to indicate that one of the important aims for this special measure was being met.

It also appeared that trials for cases with s.28 cross-examinations tended to have a trial date one to two months later than non-s.28 cases. But it was difficult to say whether this finding reflects a tendency of cases with vulnerable or intimidated witnesses to be more complex and therefore more time-consuming to prepare for trial. Nonetheless, it was an indicator that may be worth exploring: a more complex model might be able to take more factors into account, but the data was quite limited and so a more extensive investigation could be required. However, it was reassuring to note (as per the section on ineffective trials) that trial effectiveness did not seem to have been impacted despite the possible time differences.

## 5. Implications / Conclusions

S.28 can be seen as part of a greater focus in the justice system on the roles of witnesses and victims in trials (Metson & Willmott, 2024). Special measures, and s.28, focus particularly on reducing the burdens on witnesses with a vulnerable background. But changing procedures brings costs in court time, for practitioners, and administrators, especially when introducing new technical systems which require training and scheduling. Implementation requires compromise and a careful examination of the efficiency and effectiveness (Munro et al., 2024). This report tried to answer some of these questions, which incorporate the views of professionals and academics involved in the process.

A specific concern that practitioners have raised is that pre-recorded evidence will not have the same impact as live testimony. Anecdotally, they feel that juries are not as swayed by witnesses that do not appear in person (Ward et al., 2023, p.54). Witnesses, on the other hand, said that they were not only more likely to give evidence, but that they felt that pre-recording had improved the quality of the evidence they gave (*ibid.*).

This was examined by comparing case outcomes, during the period from January 2019 to September 2022, for trials with pre-recorded (s.28) cross-examination at trial (non-s.28 cases). When we controlled for the impact of the conviction rate over time, court-level impacts, and changes in the rate and type of sexual offence cases, there was no evidence that s.28 impacts on conviction rates. This suggests that the quality of the evidence had, at least, not deteriorated.

We believe that the most straightforward explanation is that the perceived change in jury impact, reported in the earlier process evaluation, had not been demonstrated in actual outcomes. If there had been a negative change, it has not been large enough to have an effect on case outcomes. Previous work has suggested that individual jurors may be influenced in different directions in a way that effectively 'cancels out' at a whole-jury level (Ellison & Munro, 2014). Our evidence is consistent with this.

Whilst we are unable to break down the impact between different eligibility groups, no sub-group that we could test had an effect that was significant on its own, or that was meaningfully different from any other group. The risk to conviction rates should therefore not be an argument against the introduction of s.28 to additional cohorts likely to benefit.

There remain arguments against pre-recording cross-examinations. Court and police resources are a major factor. We cannot say that there will be no negative effect on any given additional cohort, for example, complainants of domestic abuse; only that there is no current evidence that there would be. However, our analysis suggests that judges, prosecutors, or witnesses with concerns that pre-recording a cross-examination could undermine a trial can be reassured.

When we controlled for the impact of time, court-level impacts including s.28 roll-out, and changes in the rate and type of sexual offence cases, we were unable to detect a significant impact of s.28 on court functioning. The percentage of ineffective trials may have increased slightly as the s.28 was implemented in each court, but over time, and presumably as courts adjusted to the process, there does not appear to be a discernible effect on the percentage of ineffective trials.

The other key aim of the s.28 investigation was to ensure that using s.28 was bringing forward cross-examinations for eligible witnesses who chose to use it. Both law practitioners and administrative staff were unclear on the feasibility of using pre-recordings to schedule earlier cross-examinations. Although there is now “primary concern” (Metson & Willmott, 2024) in the criminal justice system for helping victims and witnesses to achieve both fairness and best evidence, courts remain under pressure (Select Committee on the Constitution, 2021). Here, we looked at court receipts to compare the timings of pre-recorded and live cross-examinations.

The data appeared to show that s.28 was working to bring forward cross-examinations in s.28 cases compared to cases without s.28. Our analysis showed reasonable evidence that pre-recorded cross-examinations occur, on average, a little over six months earlier than cross-examinations at trial (around 190 days excluding COVID-19 delays; 193.3 otherwise). On the other hand, the main trial for cases with s.28 measures seemed to

occur later than for cases without s.28; around 62 days (with COVID-19 delays excluded), or 35 days (with COVID-19 delays affecting all cases).

Both of these numbers come with caveats. Firstly, overall, there is a large amount of variation in the times for individual cases. This is detailed in the results section. The factors influencing case timings are extremely complex and outside the scope of this analysis. Secondly, it is possible that the later main trial dates for s.28 cases reflect cases that are more complicated to bring to trial as they involve serious offences, and vulnerable and intimidated witnesses. So, it may be worth quantifying these differences and comparing like to like with types of cases involving special measures, but this is not possible without more investigation and data collection.

Lastly, a high-level analysis of case numbers shows a relative increase in cases with s.28 hearings. This may be partly explained by a broader acceptance of special measures which would imply promotion of, and confidence in, the procedures, found by the Nuffield Foundation in a broader review from April 2020 to February 2024 (Jackson et al., 2024). However, it is not possible to say, using this data, whether more subjective aims (such as better recall, improved witness well-being, and better communication with witnesses) have also been met. Nonetheless, the balance of evidence examined in this report seems to suggest that s.28 pre-recorded cross-examinations are helping move towards the "victim-centred and trauma-informed" (Ministry of Justice, 2023a) process that is the overall goal of these changes.

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# Appendices

## Appendix 1: Data Processing

Data was from the Crown court systems, Xhibit and Common Platform, taken mainly from the datasets used in the National Statistics Criminal Court Statistics Quarterly publication.

At the time of writing there was an unresolved issue with missing data in this dataset. However, the estimated impact was <2,000 cases with no identified bias to case types or characteristics, so it was considered unlikely to impact analyses.

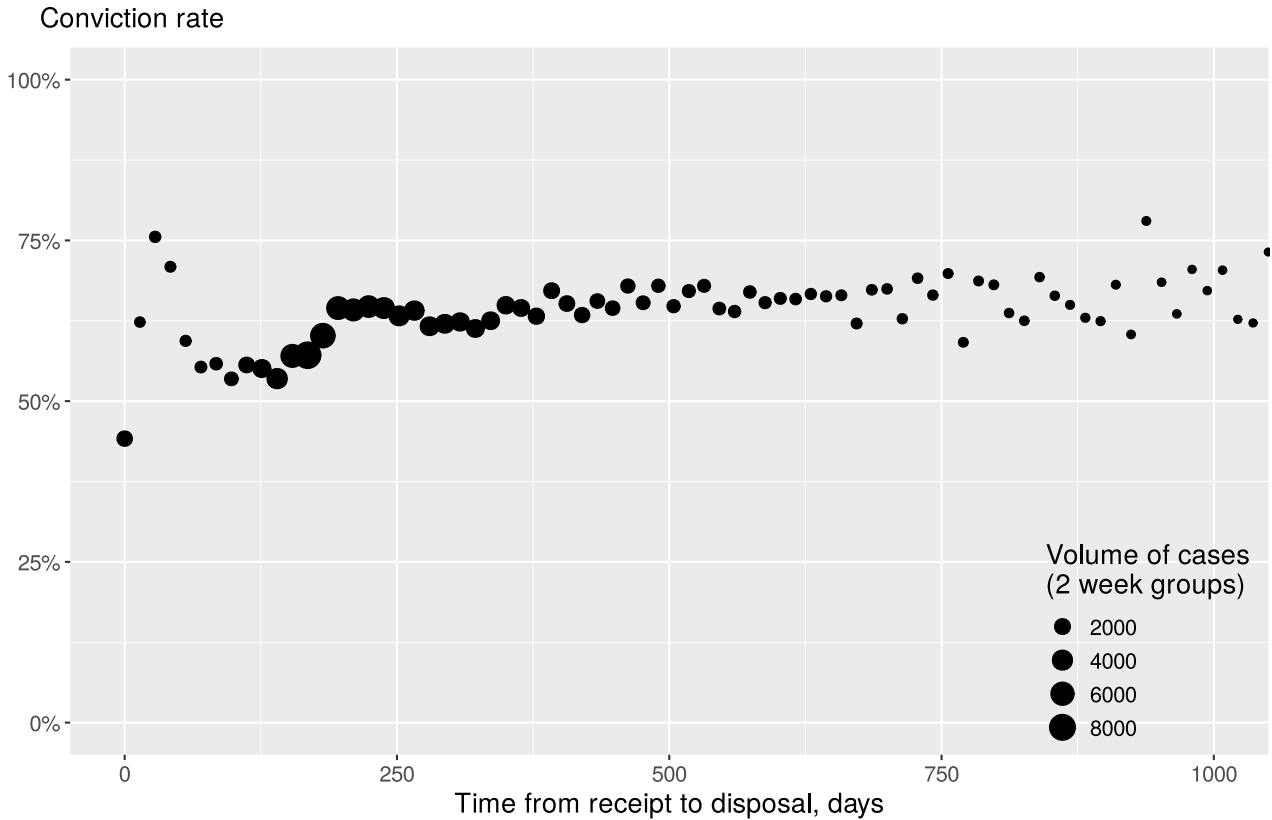
Records were selected for the analysis using the following criteria.

Records selected for Conviction Rate:

- Case received into the Crown Court 1<sup>st</sup> January 2019 to 8<sup>th</sup> August 2022,
- Plea of 'Not Guilty',
- Disposed,
- Not from a Crown Court identified as being too small, Blackfriars Crown Court (which closed in 2019), or no identified crown court.

The sample was truncated to cases received by 8<sup>th</sup> August 2022 (before roll-out of s.28 to the final group of court). A substantial proportion of cases in the final period did not have a disposal; cases in our sample with a disposal before around 250 days had a notably lower conviction rate (Figure 5), so including this data would have introduced a bias.

**Figure 5: Conviction rate of sample cases by number of days from receipt to disposal, cases received January 2019 to August 2023**



Records selected for Trial Ineffectiveness:

- Trial Date in the Crown Court 1<sup>st</sup> January 2019 to 31<sup>st</sup> August 2023
- Trial effectiveness not 'vacated'
- Not from a Crown Court identified as being too small, Blackfriars Crown Court (which closed in 2019), or no identified crown court

S.28 data was from metadata for all section 28 recordings made between 1<sup>st</sup> January 2019 and 31<sup>st</sup> August 2023. These contain the date, time, and location (Crown Court) of each recording; the length of the recording; and case, witness, and defendant IDs. Short recordings under 1 minute and records with identifiers including the word 'training' or clearly malformed IDs were removed. This data was then linked to the main Crown Court datasets.

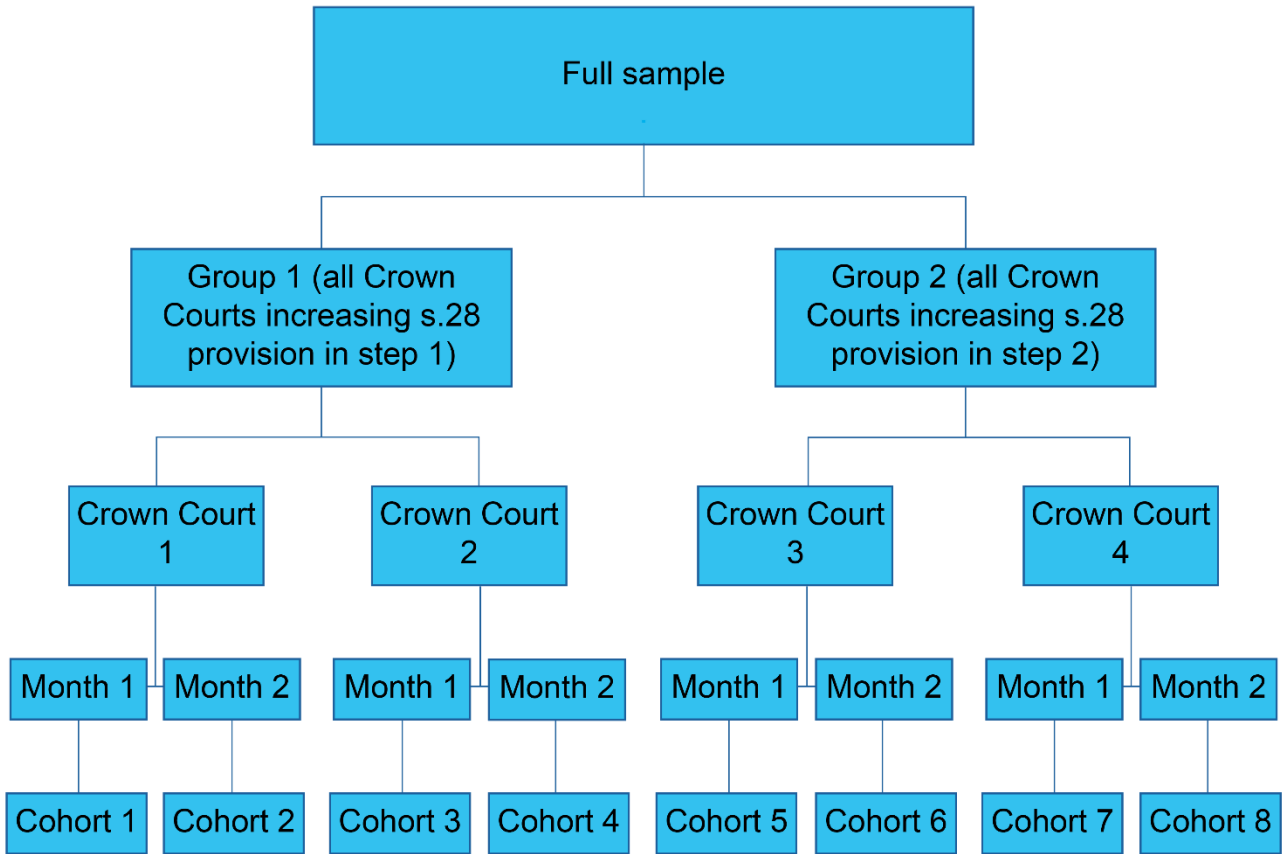
Where a case is marked as s.28, we do not have further details on the importance of the s.28 evidence to individual charges or individual defendants. Therefore, there is

considerable uncertainty in linking s.28 usage to specific parts of cases (i.e. individual defendants and offences). Analysis at a sub-case level risks introducing biases where a single use of s.28 could be counted against charges when no s.28 evidence was presented on those charges, or against defendants when no s.28 evidence was presented on those defendants. Therefore, we have not carried out any analysis at charge or defendant level.

To minimise the impact of unobserved characteristics we analysed all cases received in each month by each Crown Court. Some types of cases, such as sexual offence cases, have very high s.28 eligibility. The proportion of all cases that were Sex Offence cases in a court was therefore included in the wider analysis, and the analysis was repeated with only Sex Offence cases to further validate the findings. While Modern Slavery Offences also have very high eligibility, the small number of cases in the sample meant no special steps were taken.

The unit of analysis for conviction rates was the court-month (referred to hereafter as a cohort). Cohorts were grouped by court, and courts were grouped by the date they introduced s.28 for intimidated witnesses (see Figure 6).

**Figure 6: Diagram of grouping within analysis data for Conviction Rate and Ineffectiveness analysis**



For each cohort, we calculated:

- Size and conviction rate of cohort
- S.28 volume and rate for the cohort
- Adult and Child Sexual Offence rate of the cohort

For record selection methodology for Time to Cross-examine see 3.3.

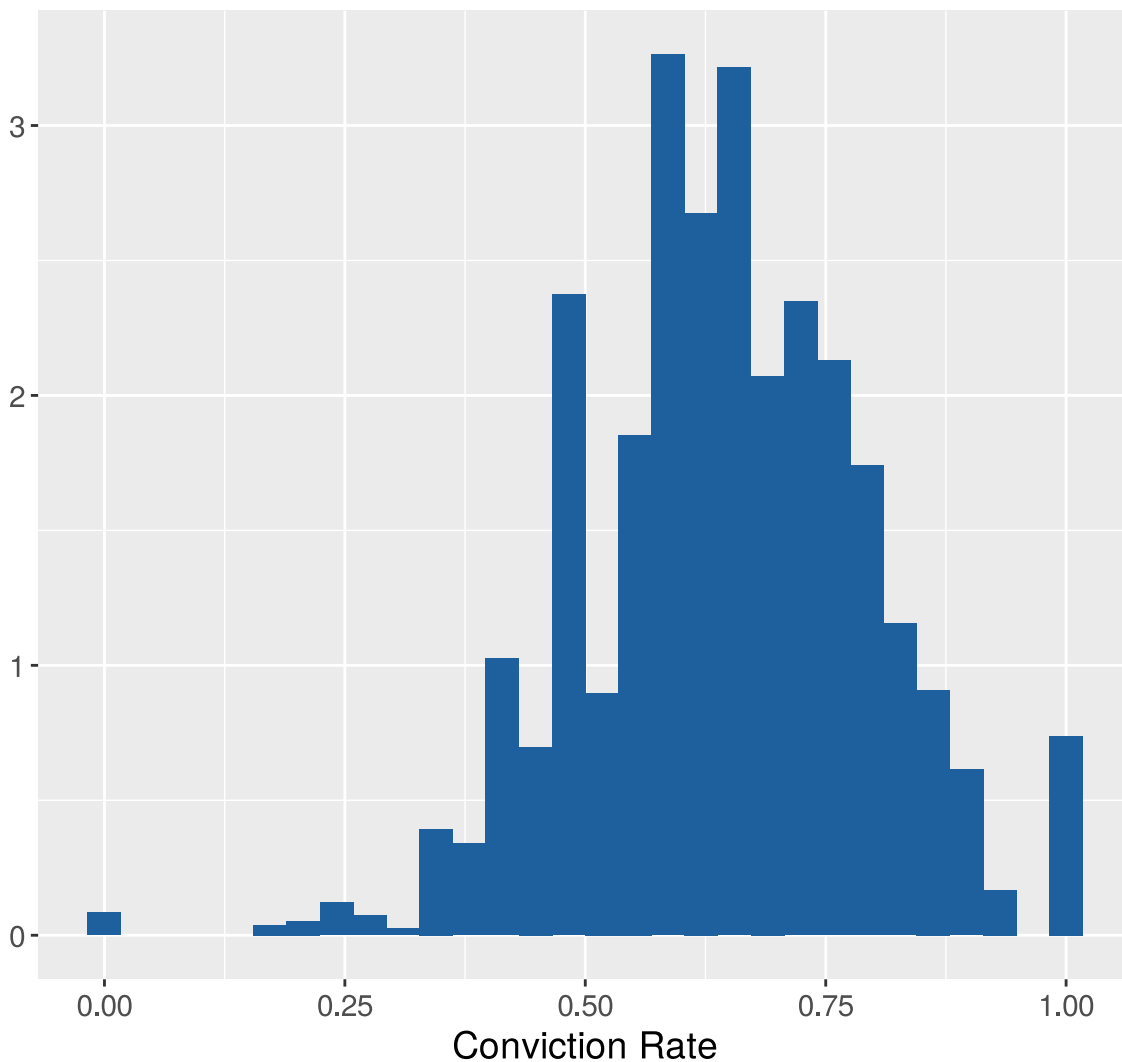
## Appendix 2: Methodology

### 2A: Conviction Rates

The minimum required sample size was calculated based on the Inter-cluster Correlation Coefficient and Design Effect of the main model (detailed below) and the stepped wedge design (Woertman et al., 2016). This gave us a total required sample (in this case, court-month cohorts) of 233.7 (assuming the minimum measurements per-step and cohorts-per-step). As the total sample included 3020 cohorts, the sample size was comfortably met.

**Figure 7: Histogram of conviction rates, weighted by cohort size**

Number of Cohorts (weighted)



Conviction Rates are somewhat right-skewed because the median is greater than 0.5, but the upper boundary is 1. So there are more cohorts at the maximum value than we would expect to see in a completely balanced normal distribution. Our analysis suggests that this distribution is close enough to a normal distribution that no modelling assumptions are violated.

Sexual Offences have lower conviction rates than other offences (Ministry of Justice, 2023b). As s.28 eligibility is higher for Sexual Offence cases, and therefore s.28 cases are disproportionately Sexual Offence cases, we included a correlation between Adult and Child Sexual Offences in the model (separately, as during testing these groups performed differently).

Analysis took the form of a linear mixed model regression performed using the lme4 package (Bates 2015) to generate full maximum likelihood estimates.

Conviction rates, s.28 usage rates, and adult and child sexual offence rates were represented as simple proportions. The month of the cohort was expressed as a proportion of the experimental timeline (so the first month was 0, the middle month was 0.5, and the last month was 1). The Crown Court was coded as a factor for grouping.

## **2B: Trial Ineffectiveness**

Analysis of Trial Ineffectiveness is based on 3,242 cohorts over 69 Crown Courts, substantially higher than the calculated required minimum sample in a stepped wedge designed trial (39) (Woertman et al., 2016).

Offences are prioritised according to several factors, some of which change through the listing process or are not available in data. In general, Sexual Offences receive a higher priority than other offence types, and that was included as an explanatory variable in the analysis.

Analysis took the form of a linear mixed model regression performed using the lme4 package (Bates, 2015) to generate full maximum likelihood estimates.

A measure of each court's size and capacity was needed in order to model the relative amount of work expended on s.28 in a given court and month. Raw numbers of trials or

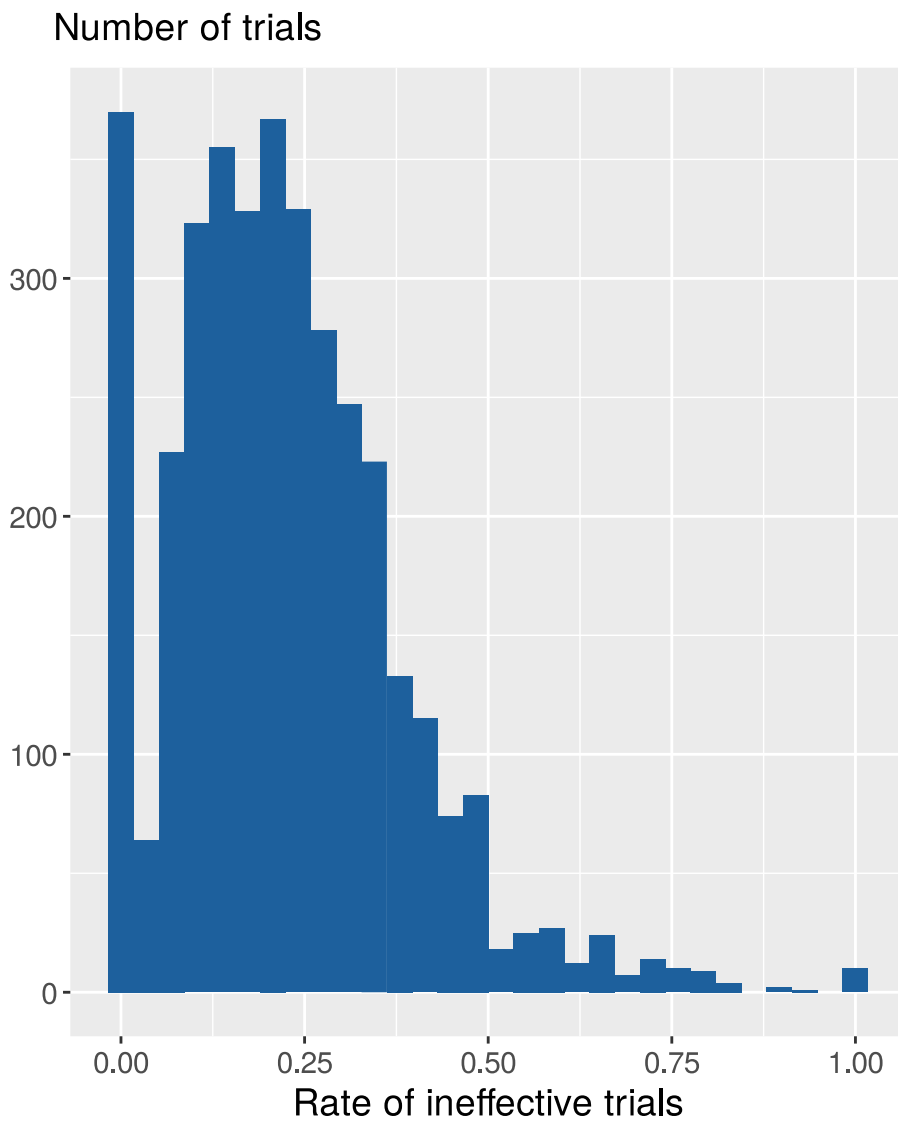
cases per month were highly variable in smaller courts, which was not reflective of the court's actual (relatively static) capacity. However, over the full period of the study some changes in court capacity could be expected, so a completely static volume was not suitable.

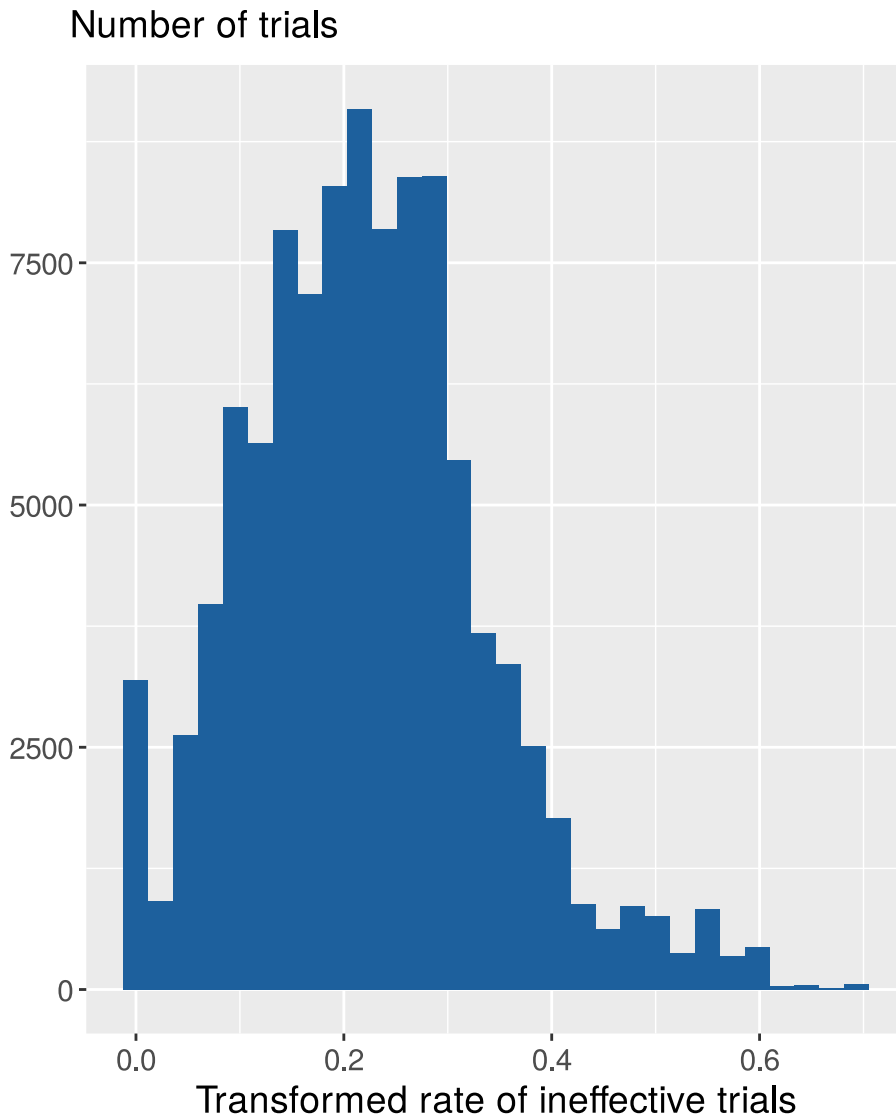
For this reason, we used a smoothed average trials-per-month calculated using locally estimated scatterplot smoothing (LoESS). Compared to moving-mean and moving-median estimates, this produces a viable estimate of underlying court capacity without abrupt changes. S.28 activity volumes were obtained from the number of unique witnesses making recordings in each month. The final measure of s.28 volume in the analysis was then the sum of witnesses over the calculated expected trial volume per court per month:

$$\frac{\sum w_{ij}}{eT_{ij}}$$

Trial ineffectiveness rates across crown courts are not normally distributed – trial ineffectiveness is truncated at zero (with zero-inflation) and right-skewed. Consequently, we used  $\text{Log}(i + 1)$ , where  $i$  is the Trial Ineffectiveness rate of that cohort, as it produces a distribution close to normal without removing zero-ineffectiveness data points.

**Figure 8: Trial ineffectiveness rates across crown courts**

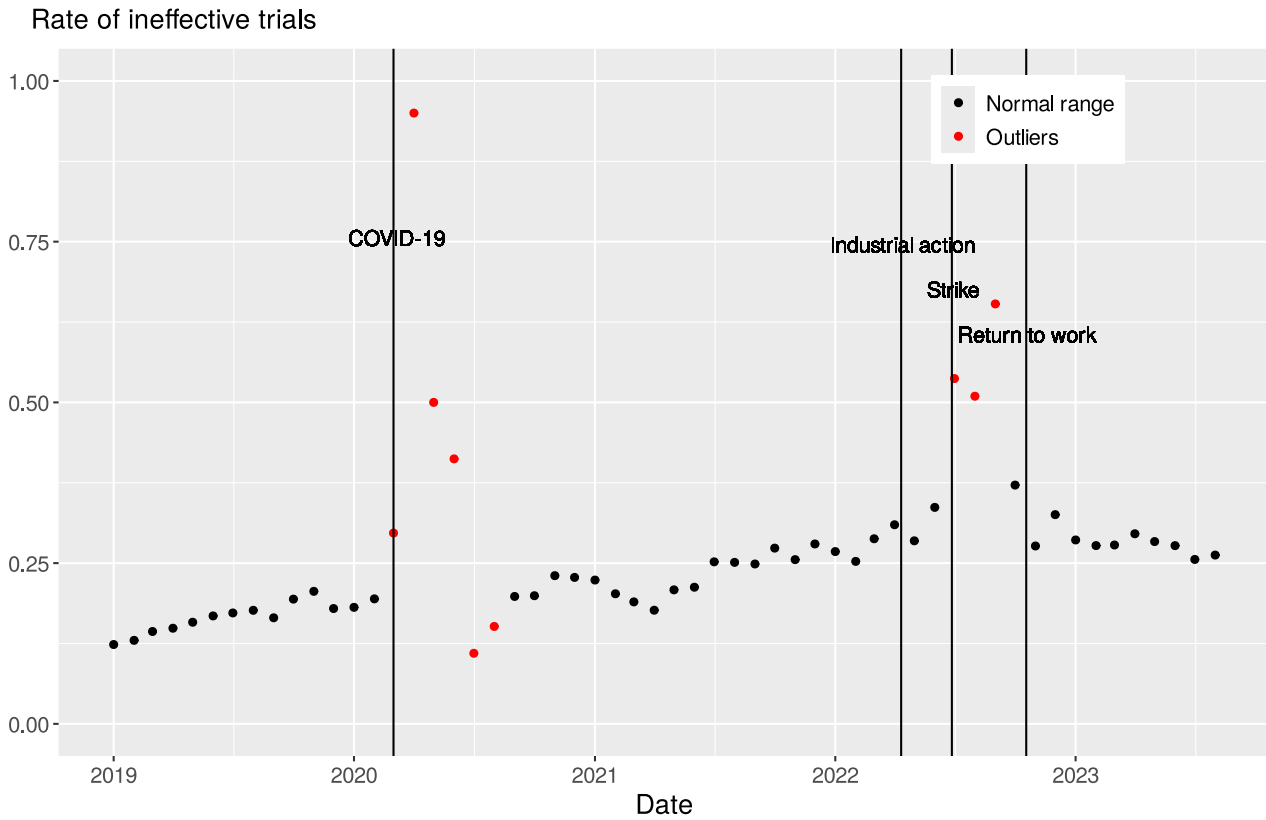




The trend in trial ineffectiveness over time was not linear, or reducible to a simple function, because it responds to a range of external factors. In particular, the COVID-19 pandemic and Criminal Bar industrial action causes substantial shifts in trial ineffectiveness at various times over the study.

We modelled the month that a particular cohort related to as a random effect (grouping factor) with months treated as independent of each other. Additionally, the months that represented the largest outliers from the surrounding trend were identified and removed (see Figure 9).

**Figure 9: Trend in Trial Ineffectiveness over time, January 2019 to September 2023**



Changes to procedures can produce disruption separate from any disruption caused by the procedures themselves (i.e. training, staff unfamiliarity, etc). A term was included for the rollout of the s.28 process, grouping all courts by the number of months after the roll-out of s.28 to Intimidated witnesses (and all courts pending rollout of s.28 for intimidated witnesses in a single group).

Data points were weighted by the cohort size. This model estimates separate linear effects on the dependent variable (log of Trial Ineffectiveness Rate + 1) for two independent variables: s.28 ratio, and sexual offence rate. It also estimates a global intercept, a per-court intercept, and a per month intercept.

**2C: Timeliness****Table 11: Breakdown of numbers of cases by month received, with and without a trial date recorded, January 2019 to August 2023, as of October 2024**

<b>Month of receipt</b>	<b>No trial date</b>	<b>Trial date</b>
January 2019	0	1,036
February 2019	0	883
March 2019	1	968
April 2019	0	985
May 2019	0	1,003
June 2019	0	825
July 2019	0	907
August 2019	0	829
September 2019	0	814
October 2019	0	881
November 2019	1	761
December 2019	0	679
January 2020	0	812
February 2020	0	811
March 2020	0	673
April 2020	0	263
May 2020	1	455
June 2020	4	764
July 2020	1	1,018
August 2020	2	968
September 2020	5	1,039
October 2020	2	1,051
November 2020	0	888
December 2020	0	810
January 2021	1	848
February 2021	3	817
March 2021	1	936
April 2021	0	834

<b>Month of receipt</b>	<b>No trial date</b>	<b>Trial date</b>
May 2021	3	807
June 2021	11	817
July 2021	6	761
August 2021	11	628
September 2021	9	619
October 2021	7	658
November 2021	4	709
December 2021	6	649
January 2022	4	682
February 2022	12	657
March 2022	11	777
April 2022	6	700
May 2022	15	756
June 2022	12	740
July 2022	12	689
August 2022	14	739
September 2022	6	619
October 2022	8	631
November 2022	7	674
December 2022	14	540
January 2023	7	614
February 2023	6	584
March 2023	3	676
April 2023	3	564
May 2023	6	541
June 2023	9	435
July 2023	3	467
August 2023	1	402

## Appendix 3: Results

### 3A: Conviction Rates

**Table 12: Maximum Likelihood Estimates and confidence values for independent variables on Conviction Rates, cases received January 2019 to September 2022**

Term	Estimate	Standard Error	P Value	Lower Confidence interval	Higher Confidence Interval
(Intercept)	0.686	0.0079	2.69E-217***	0.671	0.702
s.28 Rate	0.006	0.0351	0.864	-0.063	0.075
Time (across study period)	-0.042	0.0101	2.53E-05***	-0.062	-0.023
Adult Sexual Offence Rate	-0.143	0.0232	8.26E-10***	-0.188	-0.097
Child Sexual Offence Rate	0.029	0.0247	0.242	-0.020	0.077

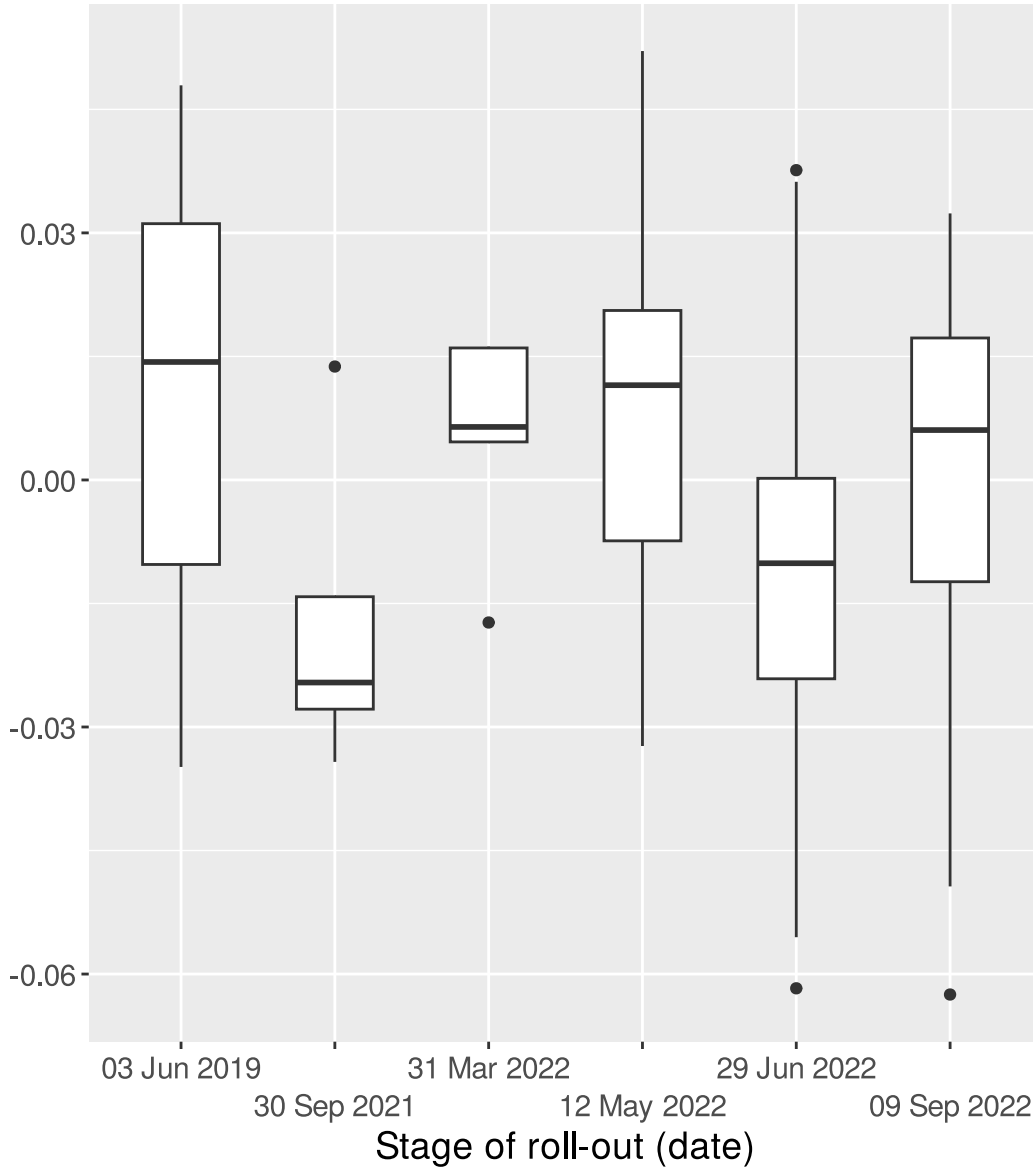
\*\*\*: 99.9% significance, \*\*: 99% significance, \*: 95% significance, .: 90% significance

The estimated effect size for s.28 rate, 0.006, is very small compared to the standard error of 0.0351, giving a large p-value of 0.864. In addition, the confidence interval contains zero. Taken together, this means that we fail to reject the null hypothesis and that the effect of s.28 rate cannot be distinguished from zero.

To exclude the possibility of impacts from pilot courts, we examined the calculated intercepts for courts by the stage of the roll they were placed in (see Figure 10). While there was some variability between courts particularly in the second and third waves (with 7 and 12 courts respectively), the initial pilot courts were in line with the larger, later waves.

**Figure 10: Per-Court intercept values by the s.28 for intimidated witnesses roll-out date for that court group**

Court Intercept



The initial pilot courts had results in-line with the rest of the group.

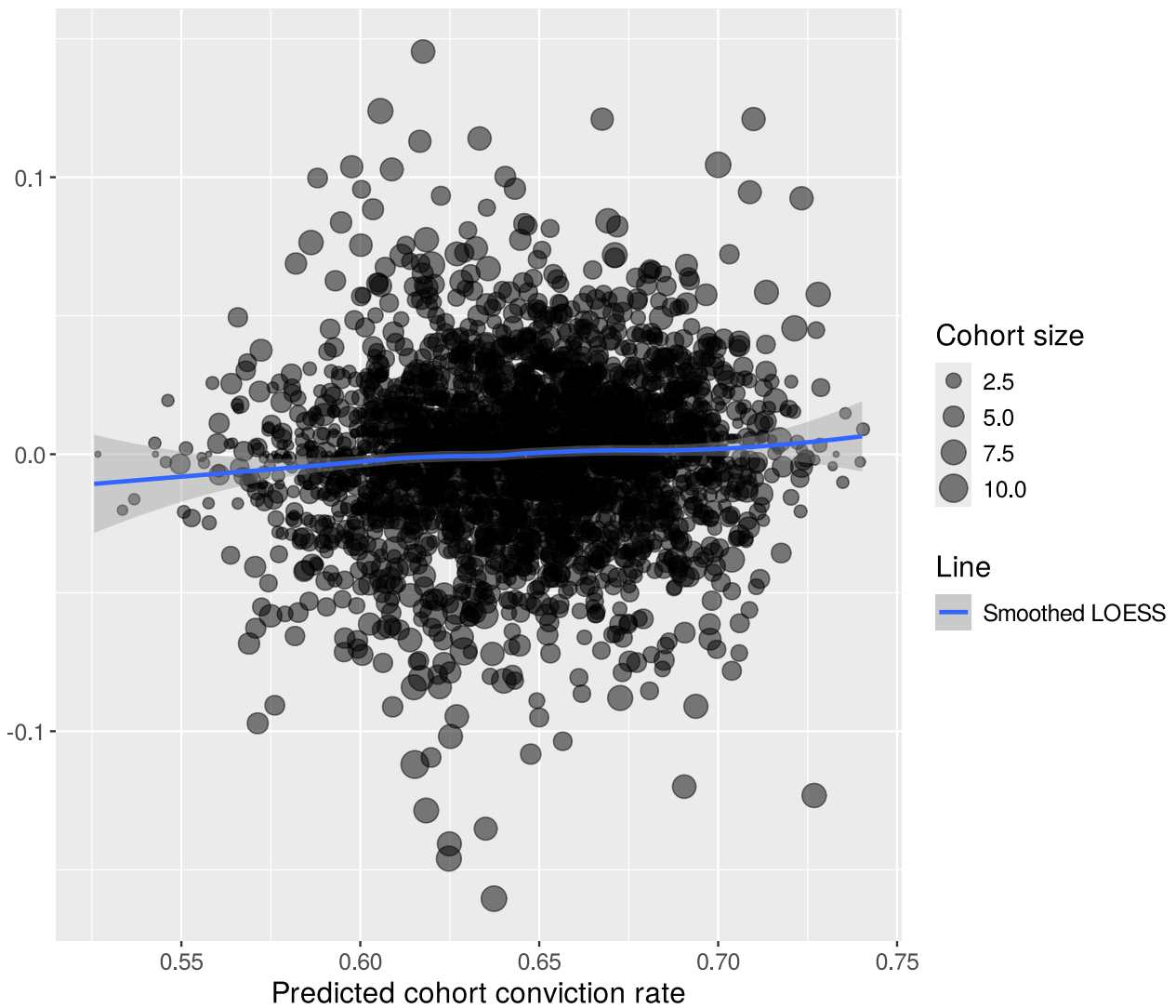
To assure the robustness of the model, several variations were tested with additional factors. Inclusion of an explicit factor for whether a cohort was pre or post-rollout produced a model with singular fit against that variable (i.e. no effect was observed).

Including an explicit variable for the court group produced a model that was not significantly different from the main model.

To screen for additional major factors not included in the analysis, the model residuals were examined.

**Figure 11: Model residuals versus predicted conviction rate**

Model residuals (scaled for cohort size)



This showed the residuals to be normally distributed around 0.

To ensure that impacts of sexual offences are not conflated with the impacts of s.28, the model was repeated on a subset of the data that contained only sexual offences. As all data are either child or adult sexual offences, only one term, Child Sexual Offence Rate, has been included to avoid multicollinearity.

**Table 13: Analysis for data containing sexual offences only**

	<b>Estimate</b>	<b>Standard Error</b>	<b>P Value</b>	<b>Lower Confidence interval</b>	<b>Higher Confidence Interval</b>
(Intercept)	0.548	0.0127	7.17E-155***	0.523	0.573
s.28 Rate	0.005	0.0196	0.798	-0.033	0.043
Time (across study period)	-0.031	0.0181	0.086.	-0.067	0.004
Child Sexual Offence Rate	0.162	0.0101	8.86E-56***	0.142	0.182

\*\*\*: 99.9% significance, \*\*: 99% significance, \*: 95% significance, .: 90% significance

Results for this subset are in line with results for the whole model.

Further iterations of the model included only Adult Sexual Offences and only Child Sexual Offences; while the total sample sizes in these models was too low to be robust, they also broadly agree with the findings of the main model.

For other specific offences the sample sizes in these cases are necessarily very low and our analysis would not reliably distinguish effects at any more granular level.

These results are similar to the outcome of a non-adjusted analysis of the same data; without any correction, while there is variation between offence categories, cases with s.28 provisions see similar rates of conviction to cases that were not eligible or chose not to use s.28.

### 3B: Trial Ineffectiveness

**Table 14: Estimates for trial ineffectiveness**

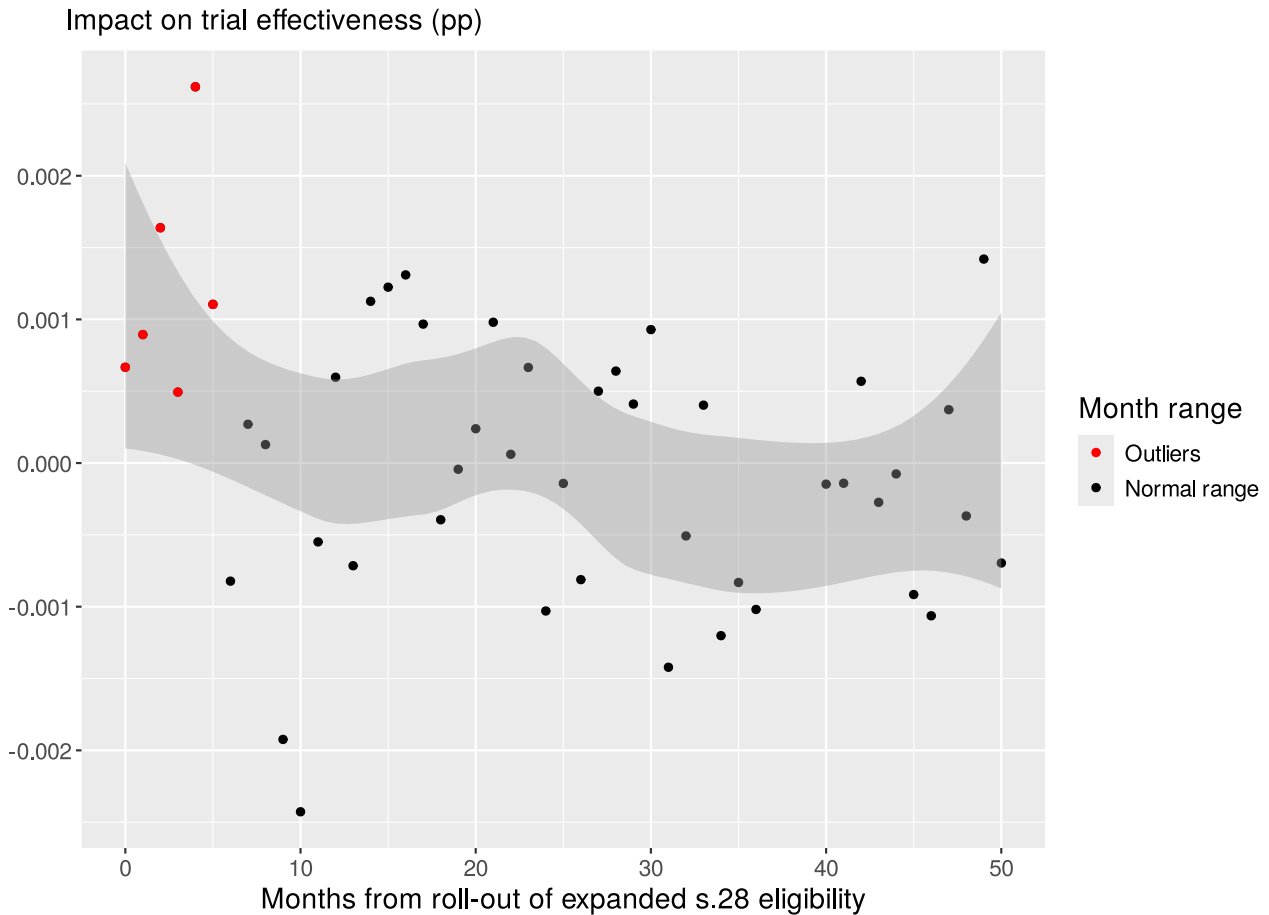
	<b>Estimate</b>	<b>Standard Error</b>	<b>P Value</b>	<b>Lower Confidence interval</b>	<b>Higher Confidence Interval</b>
(Intercept)	0.208	0.010	8.00E-18***	0.187	0.230
S.28 Ratio	0.002	0.017	0.911	-0.032	0.035
Sexual Offence Rate	-0.079	0.016	4.95E-07***	-0.110	-0.048

\*\*\*: 99.9% significance, \*\*: 99% significance, \*: 95% significance, .: 90% significance

The estimated effect size for s.28 ratio, 0.002, is very small compared to the standard error of 0.017, giving a large p-value of 0.911. In addition, the confidence interval contains zero. Taken together, this means that we fail to reject the null hypothesis and that the effect of s.28 ratio cannot be distinguished from zero.

Analysis of the intercepts for the time-since-rollout term indicated that the initial months following roll-out (Figure 12, points shown in red) may have produced slightly higher levels of Trial Ineffectiveness; the observed effect was small and cannot be confirmed based on this study.

**Figure 12: Estimated impact of s.28 roll-out on trial effectiveness by month, January 2019 to August 2023**

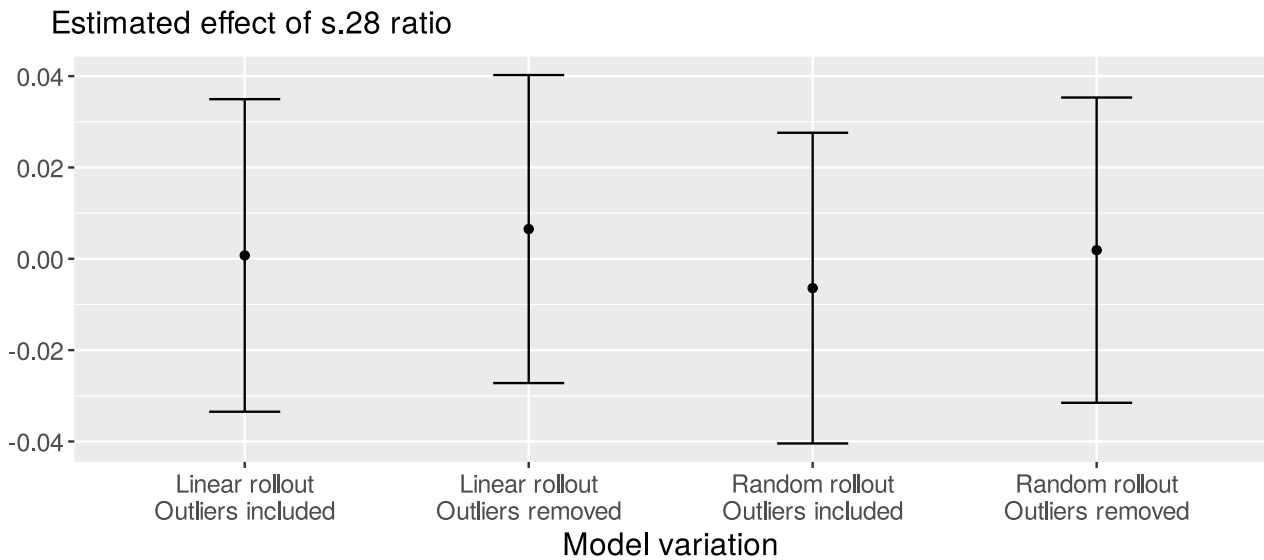


We tested the robustness of the findings by re-fitting the model with:

- outlier months included and excluded,
- the rollout term included as a fixed or random effect.

These models were then tested alongside the main model to analyse the impact of these decisions.

**Figure 13: Estimates of the effect of s.28 ratio on court ineffectiveness from variations of the analysis model**



As shown by Figure 13, neither including months where the overall court ineffectiveness was substantially impacted by external factors, nor calculating the impact of the rollout as a fixed effect, significantly changed the main finding.

### 3C: Timeliness

**Table 15: Dates of s.28 cross-examinations versus trial cross-examinations, INCLUDING those affected by the COVID-19 closures**

WITHOUT COVID EXCLUSION	S.28 cross-examination	No s.28 cross-examination
Total number of cases	3310	38287
<b>Mean days to s.28 hearing</b>	<b>175</b>	--
(standard deviation)	109.2	--
Median days to s.28 hearing	145	--
<b>Mean days to trial hearing</b>	<b>368.3</b>	<b>333.8</b>
(standard deviation)	190.9	240.3
Median days to trial hearing	329	263

<b>WITHOUT COVID EXCLUSION</b>	<b>S.28 cross-examination</b>	<b>No s.28 cross-examination</b>
<b>Mean days cross-examination brought forward by s.28</b>	<b>193.3</b>	--
(standard deviation)	161.3	--
Median days brought forward by s.28	158	--

**Table 16: Dates of s.28 cross-examinations versus trial cross-examinations, EXCLUDING those affected by the COVID-19 closures**

<b>WITH COVID EXCLUSION</b>	<b>S.28 cross-examination</b>	<b>No s.28 cross-examination</b>
Total number of cases	2992	30731
<b>Mean days to s.28 hearing</b>	<b>166.1</b>	--
(standard deviation)	91.6	--
Median days to s.28 hearing	143	--
<b>Mean days to trial hearing</b>	<b>356.1</b>	<b>294.2</b>
(standard deviation)	175.7	205.5
Median days to trial hearing	322	232
<b>Mean days brought forward by s.28</b>	<b>190</b>	--
(standard deviation)	156	--
Median days brought forward by s.28	157	--

## Appendix 4: Rollout Schedule for s.28 provision for Intimidated witnesses

Wave	Crown Courts adding provision for s.28 for Intimidated Witnesses
03 June 2019	Kingston Upon Thames, Leeds, Liverpool
30 September 2021	Durham, Harrow, Isleworth, Wood Green
31 March 2022	Bradford, Great Grimsby, Kingston Upon Hull, Teesside, York
12 May 2022	Bournemouth, Bristol, Exeter, Gloucester, Leicester, Lincoln, Newcastle Upon Tyne, Northampton, Nottingham, Plymouth, Portsmouth, Salisbury, Sheffield, Shrewsbury, Southampton, Stafford, Stoke On Trent, Taunton, Truro, Warwick, Winchester, Worcester
29 June 2022	Birmingham, Bolton, Burnley, Cardiff, Carlisle, Chester, Derby, Manchester Crown Sq, Manchester Minshull St, Merthyr Tydfil, Mold, Preston, Swansea, Swindon, Wolverhampton
09 September 2022	Aylesbury, Basildon, Cambridge, Canterbury, Central Criminal Court, Chelmsford, Croydon, Guildford, Inner London Sessions House, Ipswich, Lewes, Luton, Maidstone, Norwich, Oxford, Reading, Snaresbrook, Southwark, St Albans, Woolwich

## Appendix 5: Unmodified volumes and conviction rates by offence type and s.28 status

	<b>Non-s.28 case total</b>	<b>S.28 case total</b>	<b>Non-s.28 Conviction Rate</b>	<b>S.28 Conviction Rate</b>
Non-Sexual Offences	24,764	266	66.1%	63.5%
Adult Sexual Offences	4619	571	53.0%	54.8%
Child Sexual Offences	3563	1332	70.0%	67.9%

Trials received in Crown Courts, England and Wales January 2019 to August 2022