Department for Transport

Monitoring drink driving:

A statistical consultation on the Department for Transport's drink drive statistics

July 2014

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Foreword

British roads are amongst the safest in the world, and like other countries, we too have seen a downward trend in road casualties over a number of years. Whilst the downward trend is a positive sign, there are still too many deaths on our roads. These deaths can be attributed to a number of factors, one of which is drink driving.

Drink driving jeopardises the safety of every road user an offender comes into contact with and is something we want to eradicate. Over the past 50 years a lot of work has been done to reduce the number of drink drivers on our roads. The public is less tolerant of those who drink and drive. This can be seen in the drop in accidents attributed to drink driving since the drink driving offence was introduced in 1967. The police, the courts, local authorities and road safety organisations have all played a significant part. The THINK! Campaigns raise awareness amongst drivers around the dangers of drink driving. The High Risk Offenders scheme has helped those with repeated drink driving offences. But we are not resting on our laurels: before the end of this Parliament, we will the remove the "statutory option" which has enabled some people with positive breath tests to avoid the drink drive offence and the Home Office is type approving mobile evidential breath testing equipment, which will further aid enforcement.

We can only continue to make improvements on drink driving if we have reliable data on the number of deaths and injuries attributed to drink driving, when and where it occurs, the age of offenders and so forth.

Obtaining quick and accurate figures for drink driving casualties is not straightforward and there are trade-offs to be made between the speed at which data can be produced and its robustness. That is what this consultation is about. We want to know what you think, so that we can produce the most informative and useful statistics, to help all of us lower the scourge of drink driving further.

Jessica Matthew
Deputy Director, Road User Licensing, Insurance and Safety
Department for Transport

Executive summary

Introduction

1. The Department has published annual estimates of drink drive casualties for many years. They are an important tool for year-on-year monitoring of progress on drink driving and assessing the efficacy of drink drive education, legislation and enforcement.
2. Around 6 months after the end of each year, we publish a set of provisional estimates. These are finalized a year later when more data are available. The provisional estimates are intended as a timely snapshot of drink drive deaths.
3. We have identified some problems with recent provisional estimates - related to a diminishing sample size and systematic bias. This has prompted us to review the methodology and timing for the provisional statistics.
4. This consultation presents 3 options for changing the methodology and timing for provisional statistics, as well as discussing the implications of retaining them as they are or stopping them entirely.
5. We are seeking user feedback on these options, particularly on the careful balance between the accuracy and timeliness of the statistics.

What's in this document?

6. Section 1 describes the current methodology and the limitations of the current provisional estimates.
7. Section 2 presents the options in brief, including a summary table with pros and cons for each.
8. Section 3 presents a more detailed discussion and exemplifications for the options.

How to respond

We welcome feedback on the options proposed in the consultation or other suggestions to improve the provisional estimates. A series of questions is given in Annex A if you wish to structure your response but we are happy to receive more "open" responses as well.

The consultation period began on 24 July 2014 and will run until 4 Sept 2014. Please ensure that your response reaches us before the closing date.

If you would like further copies of this consultation document or if you need alternative formats (Braille, audio CD, etc.) you can contact us using the contact details below.

Please send consultation responses to:

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If you would like to have an informal discussion about the consultation and your views, please email or call the number above to arrange.

When responding, please state whether you are responding as an individual or representing the views of an organisation. If responding on behalf of a larger organisation, please make it clear who the organisation represents and, where applicable, how the views of members were assembled.

Freedom of Information

Information provided in response to this consultation, including personal information, may be subject to publication or disclosure in accordance with the Freedom of Information Act 2000 (FOIA) or the Environmental Information Regulations 2004.

If you want information that you provide to be treated as confidential, please be aware that, under the FOIA, there is a statutory Code of Practice with which public authorities must comply and which deals, amongst other things, with obligations of confidence.

In view of this it would be helpful if you could explain to us why you regard the information you have provided as confidential. If we receive a request for disclosure of the information, we will take full account of your explanation, but we cannot give an assurance that confidentiality can be maintained in all circumstances. An automatic confidentiality disclaimer generated by your IT system will not, of itself, be regarded as binding on the Department.

The Department will process your personal data in accordance with the Data Protection Act (DPA) and in the majority of circumstances this will mean that your personal data will not be disclosed to third parties.

1. Background to the existing methodology

1.1 The Department has published annual estimates of drink drive accidents and casualties for over 20 years. They are an important part of DfT's road safety evidence base, for year-on-year monitoring of progress and assessing the efficacy of drink drive education, legislation and enforcement.

1.2 The latest published drink drive statistics are available from this page - www.gov.uk/government/collections/road-accidents-and-safety-statistics

1.3 Collecting the data underlying the estimates takes up to 18 months from year-end. For many years DfT have published provisional estimates using a partial sample of data 8 months after year-end. These are then finalized a year later using more complete data.

1.4 Recent years have seen some significant revisions between the provisional and final statistics. For example, provisional estimates suggested there had been 280 drink drive fatalities in 2011 – an increase from 2010. The revised final figure was 240, 15 per cent lower, unchanged from 2010 and jointly with 2010, the lowest figure since detailed recording began.

1.5 Similar revisions have been seen in previous years as well, which has prompted us to review the quality and value of the provisional estimates.

The existing methodology

1.6 The Department's figures for Reported Road Casualties are collected from Police, via a collection known as "Stats19", in which details of the casualties, vehicles and accident circumstances are recorded. We use the methodology set out below to determine how many of these casualties and accidents involved drink driving.

1.7 For the purpose of the statistics, a drink drive casualty is defined as someone killed or injured in an accident where at least one of the following criteria is true:

- At least one driver fails or refuses a roadside breath test[1]
- At least one driver dies and is subsequently found, via Coroners' toxicology tests, to have an illegal blood-alcohol content (BAC)[2]

1.8 Breath test requests and results are recorded as standard in stats19 and can be straightforwardly used to identify those accidents (and their resulting casualties) for the first criteria above. A small adjustment is made to account for "missing" breath tests due to "hit and run" accidents.

1.9 The toxicology data are not part of stats19. They are collected separately from Coroners (and Procurators Fiscal in Scotland) and linked to stats19 records to identify accidents and casualties meeting the second criteria in paragraph 1.7.

1.10 This consultation focusses on the use of toxicology data and whether the way it is currently used for constructing drink drive casualty estimates is robust.

1.11 Drivers account for the majority (70 per cent) of those killed in drink drive accidents[3] and thus, relative to other casualty severities, the estimates of drink drive deaths are particularly sensitive to the toxicology data. For this reason, the analysis and exemplifications presented here focus on the drink drive death estimates, although similar, albeit smaller, considerations would apply to the other casualty severity estimates.

Use of toxicology data

1.12 Toxicology data are not available for all killed drivers recorded in stats19 – either because a toxicology test was not carried out, it was not possible to collect the data from the coroner or because the driver died more than 12 hours after the accident and thus the toxicology tests are unlikely to be reliable indicator of BAC at the time of accident.

1.13 Typically, for the final estimates, the BAC is known for around 60 to 70 per cent of killed drivers. These cases are matched to stats19 to identify the other casualties (e.g. other drivers, passengers, pedestrians etc). To account for the drivers with unknown BAC, this casualty count is scaled up. The scaling

[1] The legal breath limit is 35 micrograms of alcohol per 100ml of breath
[2] The legal BAC limit is 80milligrams of alcohol per 100ml of blood
[3] See Reported Road Casualties in Great Britain 2012 report - www.gov.uk/government/publications/reported-road-casualties-great-britain-annual-report-2012

factor is the ratio of all killed drivers to those with a known BAC.

1.14 Thus we implicitly assume that the cases with known BAC are representative of all cases, including those without known BAC.

1.15 It takes around 18 months to collect the final sample of toxicology data. The provisional estimates are based on whatever data are available 6 months after year end – typically between 30 and 40 per cent of killed drivers. The same imputation method as above is used, thus assuming that the provisional sample is representative of the wider population of killed drivers.

Limitations of current provisional estimates

1.16 We have identified two major limitations of the current provisional estimates – an increasingly small sample size and evidence of systematic bias.

Small sample size

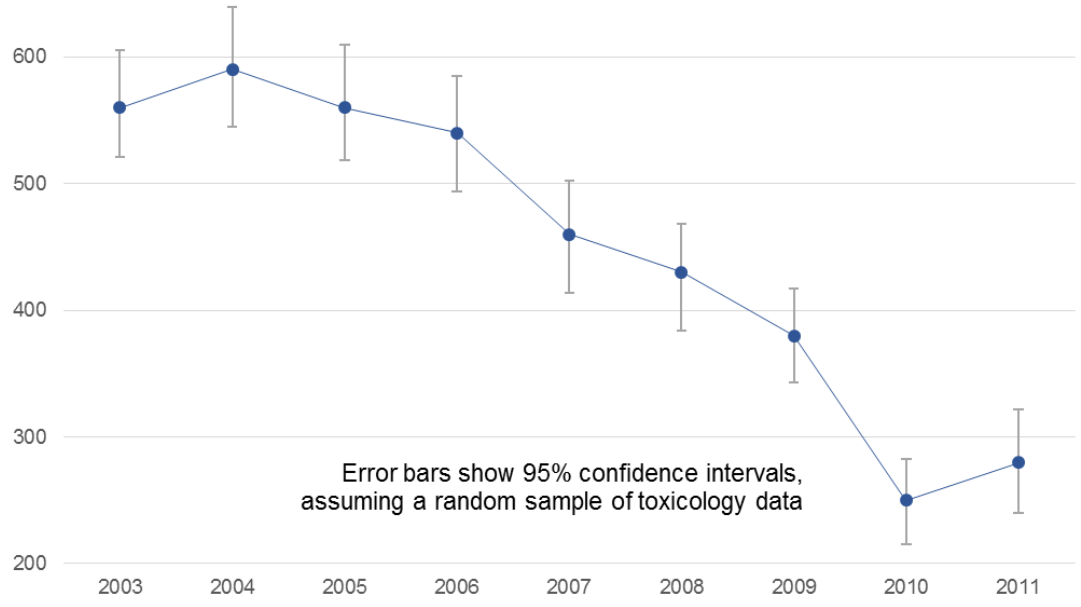
1.17 The number of killed drivers has fallen to record lows in recent years. Over the last decade, the number of motor vehicle drivers and riders killed more than halved, from around 1,900 in 2003 to just 920 in 2012.

1.18 The sample of toxicology data has also fallen, reflecting this reduction and the provisional estimates are now based on a relatively small sample of returns. The decreasing sample size increases the uncertainty around the estimate.

1.19 For example, the 2012 provisional statistics were based on just 264 toxicology returns with known BAC. The provisional estimate for drink drive deaths was 280. Assuming a random sample, the 95 per cent confidence interval around this estimate is 240 to 330, around 15 per cent either side of the central estimate.

1.20 Given that the year-on-year change in the final estimates is typically of the order 5 per cent, the confidence interval on the provisional estimates is too wide to be useful in monitoring year-on-year trends, as shown by Figure 1.1 below.

Figure 1.1 Previous provisional drink drive death estimates with 95% confidence intervals (assuming a random sample)

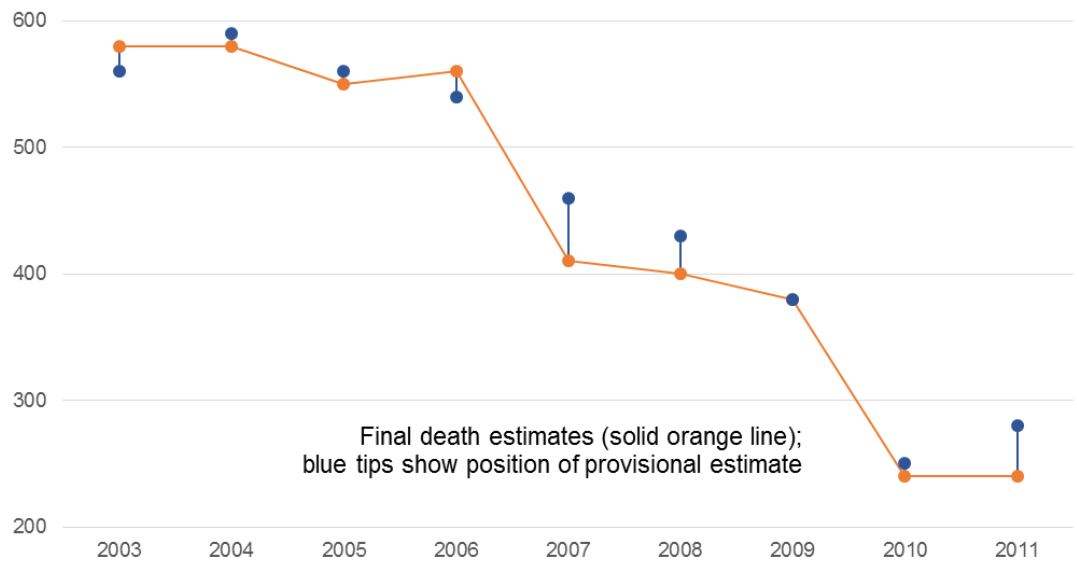


Systematic bias

1.21 The provisional sample consists of the toxicology data that have been collected by six months after year end. Thus, this is effectively a convenience sample and may not be representative of the wider population of killed drivers.

1.22 Figure 1.2 below compares the final (orange line) drink drive death estimates with the provisional (blue tip) over the past decade.

Figure 1.2 Previous final estimates (orange) with blue tips to show position of provisional estimates



1.23 If the provisional sample were a truly random sub-sample of the final set of data, we would expect roughly equal probability of an upwards or downwards revision between the provisional and final. In practice, there appears to be a strong tendency for downward revision – e.g. in 4 out of the last 5 years. This is indicative of some systematic bias although "clusters" of unusual results can occur by chance within a random distribution.

1.24 We can identify the bias more robustly using hypothesis testing, specifically the Sign Test.[4] To carry out the test, we consider 2 samples of coroners' data:

- wave 1 - records collected by 6 months after year end
- wave 2 - records added to the sample between 6 and 18 months after year end

Wave 1 forms the provisional sample; the final sample is made up of waves 1 and 2 combined.

1.25 The statistic of interest from both samples is the proportion of killed drivers with an illegal BAC.

[4] See e.g. www.statstutor.ac.uk/resources/uploaded/signtest.pdf for a brief explanation of the Sign Test

1.26 In 8 of the last 9 years (2003 - 2011), the BAC statistic for wave 1 is higher than for wave 2.

1.27 If the two waves were drawn from the same population (i.e. if there were no bias), the difference between the statistic for wave 1 and wave 2 would be equally likely to be positive or negative. The scenario is similar to that of tossing a coin – for an unbiased coin, we expect equal probability of heads or tails.

1.28 We can treat both the coin problem and the series of wave differences as a binomial distribution, where p is the probability of a positive difference (i.e. wave 1 is higher than wave 2) and n is the number of trials (i.e. the number of years of data we are considering).

1.29 In this case, $n=9$ and the null hypothesis is that there is no bias: $p = 0.5$ (equal probability of wave 1 being higher or lower than wave 2). Under the null hypothesis, the probability of seeing at least 8 instances of wave 1 being higher than wave 2 is just 1.95 per cent, allowing us to reject the null hypothesis at both the 95 and 98 per cent confidence levels.

1.30 In other words, it is very unlikely that the pattern of downward revisions seen over the past decade could arise purely from random variation – thus we conclude that the provisional estimates are systematically biased compared to the final.

2. The proposals - in brief

2.1 Having identified 2 major limitations of the existing provisional statistics methodology, this section presents options for improvements.

2.2 We have identified 3 options to improve the provisional statistics, plus options to retain or entirely stop them. The chosen option would be implemented for the provisional 2013 statistics and thereafter.

Options for the provisional statistics

Option 0

Retain provisional statistics with their current form and timing

Option 1

Stop publishing provisional statistics entirely

Option 2

Delay provisional statistics to later in the year to allow a larger, more representative sample to be gathered

Option 3

Adjust the estimates to account for the bias

Option 4

Produce the estimate as a range, rather than a single “best” estimate

2.3 Note that the options are not mutually exclusive – for example, option 2 (delaying to later in the year) could be combined with 3 or 4. We welcome user views on these possible “hybrid” options.

2.4 The options are explored in more detail and exemplified in Section 3 but a summary is given here.

Pros and cons

2.5 In considering any statistic, it is important to strike the right balance between timeliness, accuracy and precision.

- **Timeliness** in this context refers to how quickly statistics are available after the relevant period. Timely statistics are valuable for ongoing monitoring of trends, identifying sudden changes that might require action or intervention and assessing the immediate impact of changes such as new policy, practices, legislation or technology.
- **Accuracy** refers to how close to the "true" value the estimate is.
- **Precision** relates to the degree of uncertainty in the value – i.e. the range of values in which the true value is likely to lie, usually arising from random sampling variation.

2.6 At present the provisional statistics are timely, in that they are available ~8 months after year end, but as demonstrated above, their precision and accuracy are limited.

2.7 The pros and cons of the options are summarized in Table 1 overleaf. A more detailed discussion of each option is in Section 3.

2.8 The schematic in Figure 2.1 illustrates the balance between accuracy, precision and timeliness for the different options. Timeliness and accuracy are shown on the axes and precision is indicated by the shading. Note the colours and positions of the options are intended as a relative illustration rather than a precise quantification.

Figure 2.1 Schematic showing accuracy, timeliness and precision for the different options

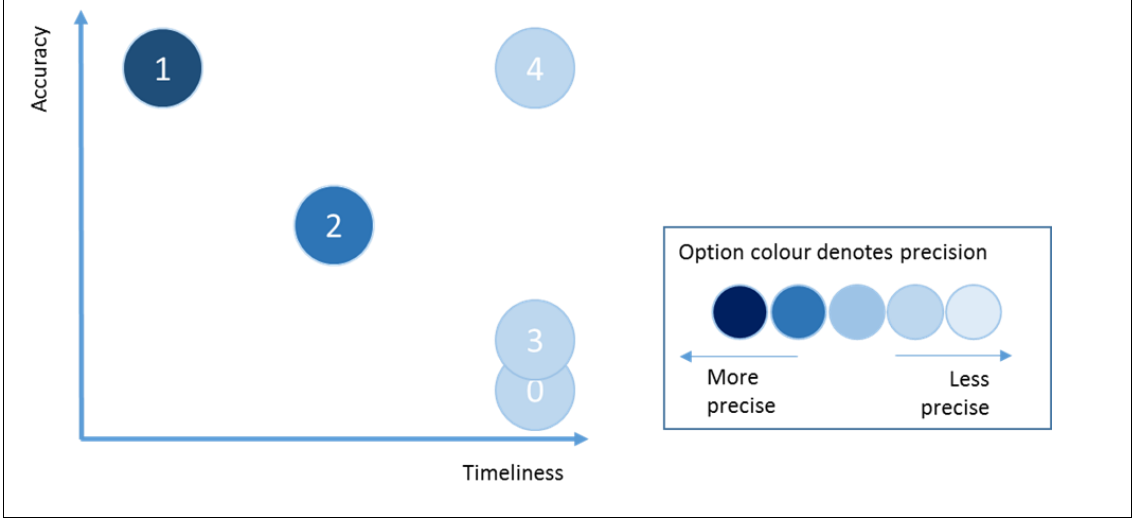


Table 2.1			
	Description	**Pros**	**Cons**
0	Retain provisional estimates in current form	Timeliness would be retained	Limited accuracy and precision
1	Stop publishing provisional statistics entirely (Only the final statistics would be published ~18 months after year end)	Only the most accurate and precise statistics would be published for each year Avoids potentially confusing situation of having 2 sets of estimates for each year	Severe loss of timeliness: final estimates are not available until 18 months after the year ends, which limits the scope for "ongoing" monitoring of trends
2	Delay production of provisional estimates to later in the year	Small improvements in accuracy and precision	Some loss of timeliness Some bias would remain, albeit reduced, because the % BAC continues to decrease throughout the collection Would not entirely eliminate revisions when finalizing estimates
3	Apply an adjustment to try and account for the bias	Possible improvement in accuracy Timeliness would be retained	Limited accuracy - difficulties in deriving a robust predictive methodology for the adjustment. No improvement in precision Would not entirely eliminate revisions when finalizing estimates
4	Produce the estimates as a range, rather than a single, central estimate	Timeliness would be retained Better reflection of the uncertainty in the provisional estimate Allow us to be reasonably confident of a range where the final value lies	No improvement in precision - range would be large and small year-on-year changes would not be detectable Reduces the scope for revisions outside the range, but doesn't entirely eliminate it.

3. The proposals - in detail

3.1 This section explores the options in more detail, including exemplifications where relevant.

Option 0 - retain provisional statistics with their current format and timing

3.2 The current provisional statistics are timely, in that they are available 6 months after the end of the year in question. However, we have demonstrated above that they suffer from the problems of small sample size and systematic bias, which limit their precision and accuracy respectively.

3.3 Although having timely statistics is useful for year on year monitoring of trends, this confers little benefit if the provisional statistics give an inaccurate and misleading picture of what that trend actually is, as we saw for the 2011 estimates.

3.4 If we were to continue to publish the statistics in their current form, any accompanying commentary would need to be more upfront about the likelihood of downward revision, in particular highlighting that the estimates are likely to be an upper limit on the true value. However, experience suggests this approach would be likely to add to users' confusion and that even strong caveats around published figures do not always get picked up in practice.

Option 1 - stop producing provisional estimates entirely

3.5 Stopping the provisional statistics entirely would mean that the final, annual estimates would be the only published figures on drink drive accidents and casualties, available 18 months after the end of the year in question. The final estimates are clearly more precise and accurate than the provisional

3.6 Stopping the provisional estimates would eliminate the confusion of having 2 sets of published estimates for each year. The interest generated by the drink drive statistics publication often overlooks the final estimates, in the face of

the new provisional estimates, although the final are a more accurate guide to trends. Stopping the provisional estimates would eliminate both of these problems and would mean that only one estimate would be published for each year.

3.7 However the very clear disadvantage of stopping the provisional estimates is a loss of timeliness. Final estimates are published 18 months after the end of each year which would seriously curtail their value for quickly identifying and responding to sudden changes in drink driving. For example, if a new drink driving policy were introduced at the start of a year, it would be over 2 years before the published Official Statistics could detect its impact.

Option 2 - delay the provisional estimates to later in the year

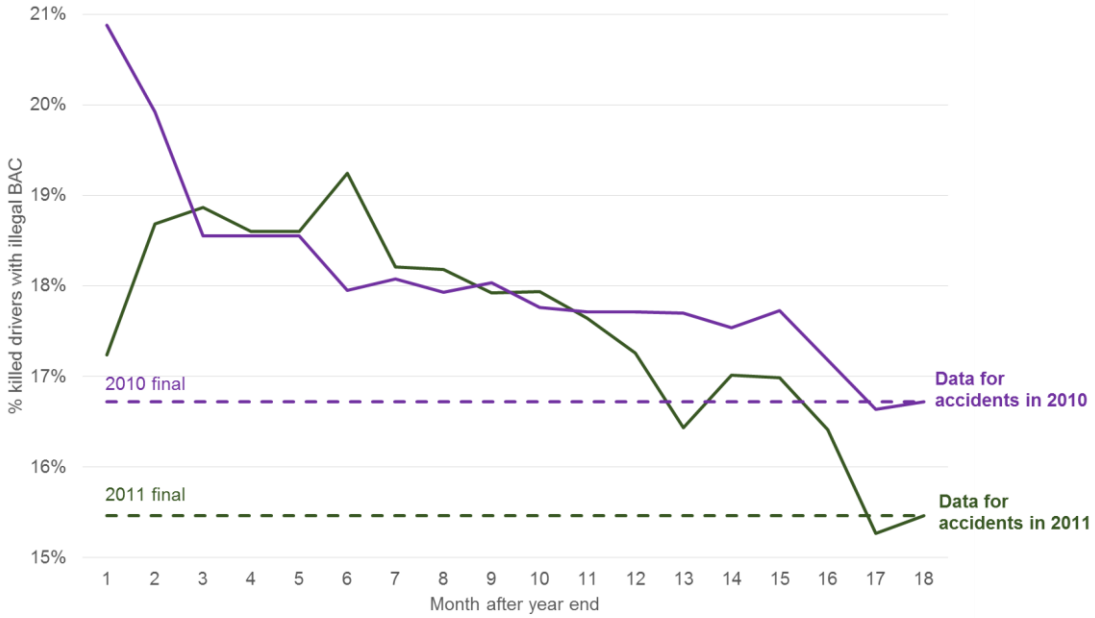
3.8 The current provisional sample is both systematically biased and increasingly too small a sample to provide a reliable estimate.

3.9 As the toxicology data are gathered continuously for up to 18 months after year end, the estimates could be delayed to later in the year to enable a larger, and more representative, sample of data to be gathered.

3.10 We have explored the feasibility of this option by looking at the evolution of the sample collected over the 18 months to year end. Data for 2010 and 2011 are used as a detailed test case and as before, the statistic of interest is the proportion of killed drivers with an illegal BAC.

3.11 Figure 3.1 shows the evolution of this statistic over time, based on the sample of data that would have been available at monthly intervals between year-end and the final sample being available, for 2010 (purple) and 2011 (green). The horizontal dashed lines show the estimates for the final samples.

Figure 3.1 Evolution of BAC statistic from toxicology sample during 2011 (green) and 2010 (purple) data collections



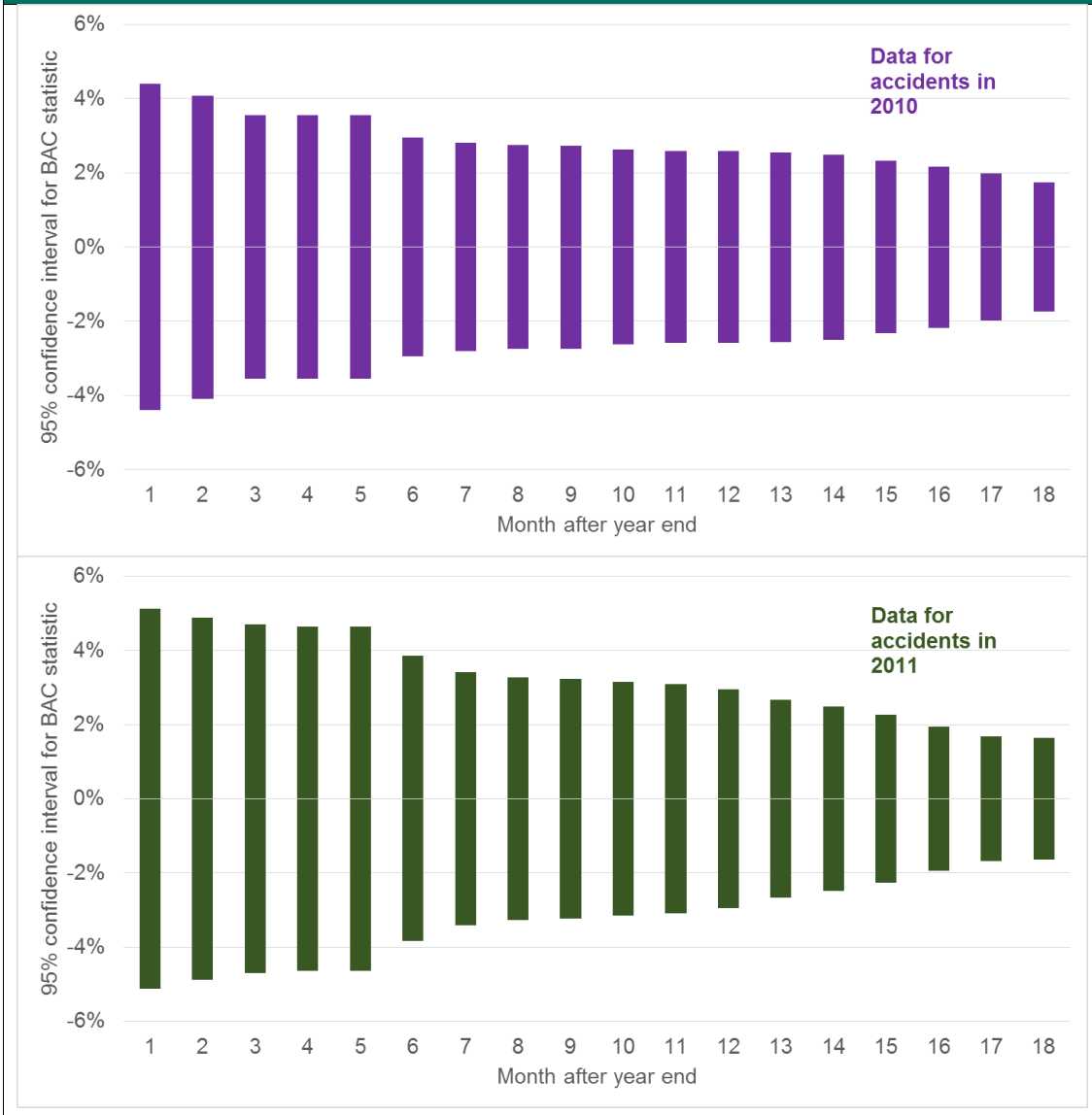
3.12 In both cases, with the exception of some large fluctuations (likely due to the processing of a batch of forms from a single area in one month), the statistic decreases roughly continuously throughout the collection period, rather than converging towards the final sample.

3.13 This does demonstrate that there may be a small degree of bias in the final sample: if the bias were eliminated by the end of the final data collection cycle, then successive additions of data would be equally likely to shift the statistic upwards or downwards, rather than the continuous downward fall we see here. The bias in the final estimates is likely to be small, given that the final sample covers the majority of killed drivers (60 to 70 per cent). Eliminating this entirely from the final estimates would require a long and burdensome extension of the data collection to gather a few extra cases, which would be likely to be disproportionate to the additional accuracy offered.

3.14 Figure 3.1 also demonstrates that there is no single obvious cut-off time to delay the provisional estimates to reduce their bias. The size of the systematic bias and would be reduced by publishing later but not eliminated entirely and the strong tendency for downward revision (albeit smaller) would remain.

3.15 Irrespective of the bias, the available sample size increases over time, so there is scope to improve the precision of the estimates by delaying them. Figure 3.2 below shows the evolution of the 95% CI for the % illegal BAC. As a rule of thumb, when the sample doubles, the CI width reduces by around 30 per cent and between the provisional and final sample, the CI halves.

Figure 3.2 Evolution of 95% confidence interval around BAC statistic from toxicology sample during 2011 (green) and 2010 (purple) data collections



3.16 Again, there is no obvious single cut-off point at which the precision markedly improves but any delay would offer some improvement. However, it is worth remembering that random variation in the sample is only one factor affecting the

provisional statistics – a bigger problem is the systematic bias, which a delay would not eliminate.

3.17 In summary – delaying the estimates to later in the year would confer some improvement in accuracy and precision, but would not eliminate the systematic bias entirely. A delay would also reduce the timeliness of the statistics.

Option 3 - adjust the estimate to account for the bias

3.18 This option involves making an estimate of the magnitude of the bias in the provisional estimates and adjusting the estimate accordingly.

3.19 Although we have demonstrated that direction of revision between provisional and final estimates is usually downward, the magnitude of the revision varies year-on-year, so this option would inevitably involve some degree of estimation of the adjustment needed.

3.20 One sensible approach would be to use an average of the revisions seen in previous years and apply it to the current year. For illustrative purposes, we have used a 3-year moving average here to show what the provisional estimates would have looked like under this option.

3.21 To recap, the figure for drink drive deaths is composed of 2 parts:

- deaths from accidents where at least one driver failed a roadside breath test – this estimate is "fixed" for both the provisional and final estimates as it is derived entirely from STATS19
- deaths from accidents where at least one driver died and was subsequently found to have an illegal BAC – this estimate is based on toxicology data and changes from provisional to final

3.22 To calculate an adjustment factor to apply to (b), we start by considering the BAC statistic from the coroners' sample: the proportion of killed drivers with an illegal BAC. We calculate the ratio between the provisional and final BAC statistic for each year.

3.23 To adjust the BAC statistic for a given year, we take the average ratio seen in the previous 3 years and scale that year's provisional BAC statistic. The adjusted BAC statistic is

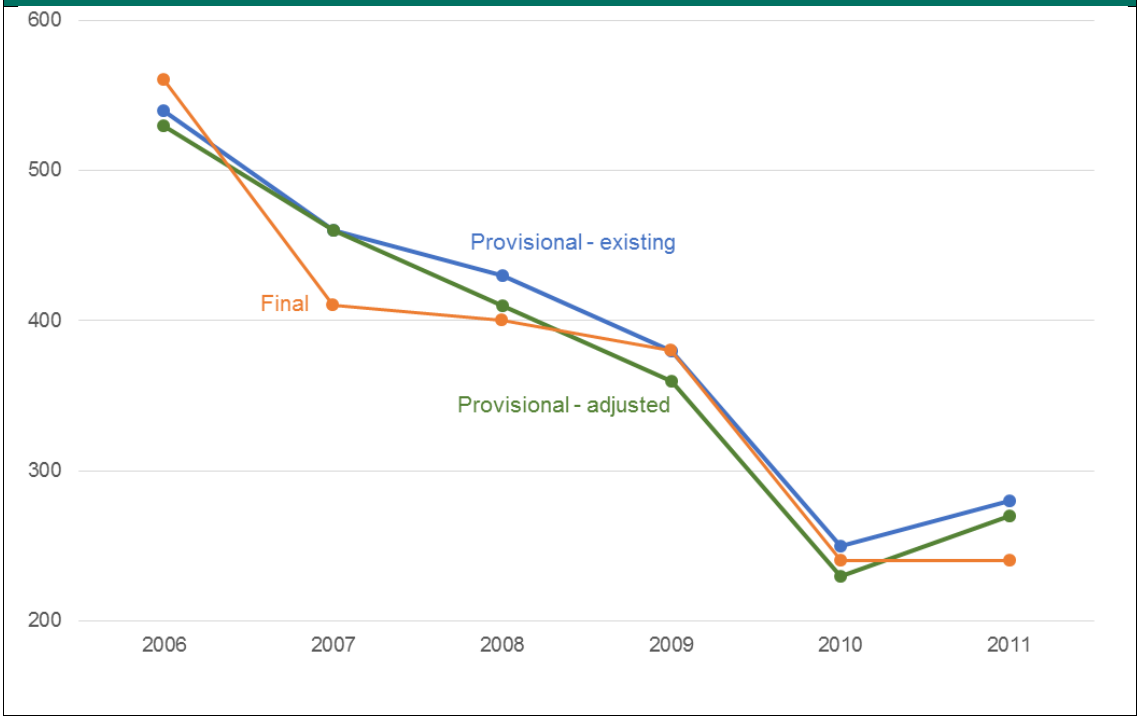
then used to generate the death estimates in (b), which are aggregated with the “fixed” estimates for (a).

3.24 Table 2 and figure 3.3 show the provisional estimates that would have resulted from this process for 2006 onwards.

Table 2: Comparison of provisional death estimates using the existing methodology and an adjusted methodology

	Provisional		Final	Revision between provisional and final		
Year	Existing method	Adjusted method	Final estimate	Using existing method	Using adjusted method	Does the adjustment reduce the revision?
2006	540	530	560	*4%*	*6%*	no
2007	460	460	410	*-11%*	*-11%*	no
2008	430	410	400	*-7%*	*-2%*	yes
2009	380	360	380	*0%*	*6%*	no
2010	250	230	240	*-4%*	*4%*	no
2011	280	270	240	*-14%*	*-11%*	yes

Figure 3.3 Comparison of provisional death estimates using the existing methodology (blue), adjusted methodology (green) and final estimates (orange)



3.25 The obvious measure of whether the adjusted methodology is an improvement is whether it would have given a smaller revision between the provisional and final estimates. This is explored in the final 3 columns of Table 2.

3.26 In 2008 and 2011, the adjusted methodology would have reduced the size of revision by a few percentage points, but still "under-adjusts" relative to the actual revision. In other years, the methodology "over-adjusts" and would have made the revision worse, albeit in the opposite direction.

3.27 This illustrates the fundamental problem with the adjustment approach: although we know that the provisional estimates are systematically higher than the final, we have no reliable means of predicting by how much. Given the adjustment over the last 6 years has varied between 4% and -14%, there is a lot of scope to get it wrong. Thus, the adjustment methodology effectively replaces one set of inaccurate figures with another.

3.28 More sophisticated methods than using a 3-year average are possible and we would welcome user suggestions on other ways to do the adjustment. However, the fundamental problem of unpredictability in the magnitude of the bias for a specific year remains.

Option 4 - produce the estimates as a range, rather than a single, central estimate

3.29 This option would present the statistics in a way that explicitly demonstrates the uncertainty and scope for revision. The advantage is that we could be reasonably certain that the "true" value lies within the given range, thus removing the problem of revisions. The disadvantage is that a wide range would make it difficult to detect year-on-year changes based on the provisional statistics.

3.30 As for Option 3, one possible methodology for this is exemplified below, but we would welcome user suggestions for other ways to do it.

3.31 As with Option 3, the starting point for calculating the range is the key statistic from the toxicology sample: % of killed drivers with illegal BAC. The error / scope for revision in the provisional toxicology sample arises from 2 factors:

- Sampling uncertainty

- Bias

3.32 The effect of sampling uncertainty can be captured by a confidence interval. For a random sample, the confidence interval can be calculated by a “z-test” approach (approximating a binomial distribution as a normal distribution). A common approach is to use a 95% confidence interval (CI).

3.33 However, as we demonstrated above, the systematic bias in the provisional sample means it is far more likely to be revised downwards than upwards. A conservative way to capture the full range of likely revisions would be to consider the biggest revision seen in recent years (2011, when the % illegal BAC reduced by a factor of 1.2 between the provisional and final sample) and treat it as a “worst case scenario” for a lower limit.

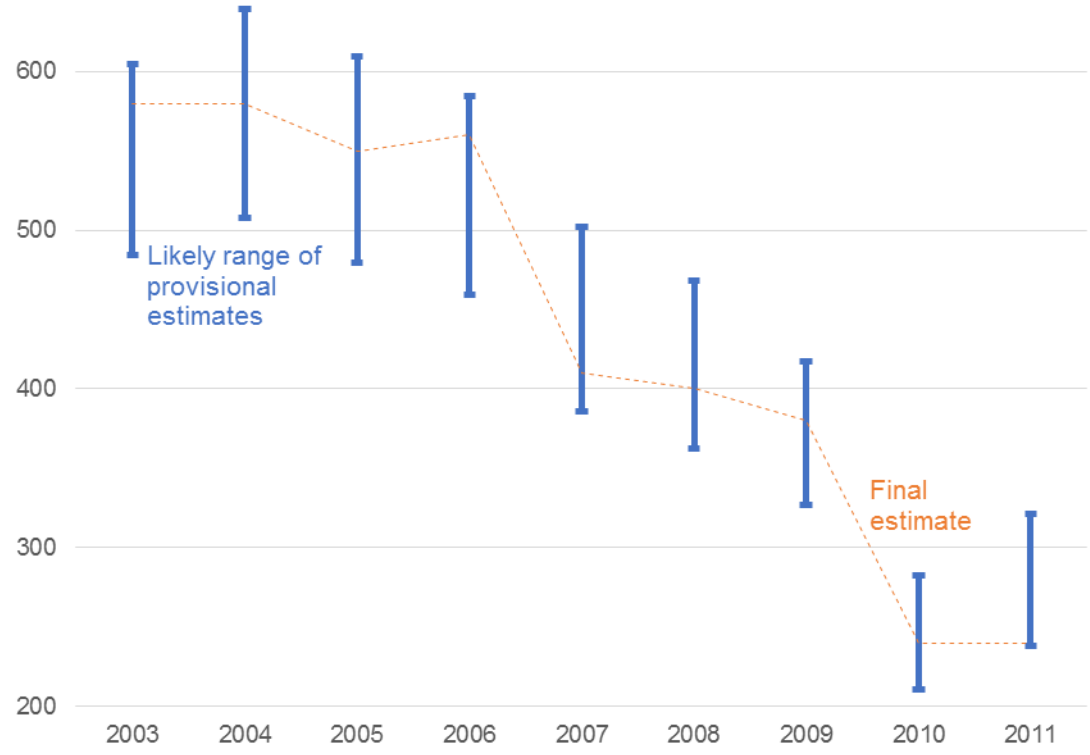
3.34 For illustrative purposes we have applied the range methodology to the provisional estimates for previous years, to show what the statistics might have looked like using this approach. In each case, the range is defined by an upper bound (95% CI) and a lower bound (maximum expected downward revision).

3.35 Table 3 and figure 3.4 show the provisional death estimates that would result from the range methodology. The grey line underneath shows where the final estimates were for each year.

Table 3 Exemplification of range method for estimating provisional drink drive deaths; lower bound (based on "worst case" revision); upper bound (95% confidence interval for a random toxicology sample)

	Provisional estimates			Final
Year	Lower bound	Upper bound	*Range*	Final estimate
2003	480	600	*(480 , 600)*	580
2004	510	640	*(510 , 640)*	580
2005	480	610	*(480 , 610)*	550
2006	460	580	*(460 , 580)*	560
2007	390	500	*(390 , 500)*	410
2008	360	470	*(360 , 470)*	400
2009	330	420	*(330 , 420)*	380
2010	210	280	*(210 , 280)*	240
2011	240	320	*(240 , 320)*	240

Figure 3.4 Comparison of range method for provisional (blue bars) and final (dashed orange line) death estimates



3.36 The range of estimates should not be treated as symmetric: due to the unpredictability in the size of the downward revision, we wouldn't be able to say where in the range the true value is most likely to lie and the mid-point of the range should not be seen as a "best estimate". If we were to adopt this methodology, we would not attempt to produce or publish a "best estimate" - what we could say is that we are reasonably certain that the actual value lies in that range.

3.37 The clear disadvantage of this methodology is that the ranges are wide and would mask any small changes in drink driving. For example, for 2011, we would have been able to say that drink drive deaths in 2011 were somewhere between 320 and 240 but not whether this was different from the previous year's final estimate (240 deaths, in 2010). However the analysis in Section 1 demonstrated that even where the provisional estimates do suggest a small year-on-year change, it is often not robust or reliable.

3.38 The range methodology would be useful for giving a timely "snapshot" to identify whether there has been a big change in

drink drive deaths. This would be particularly useful for getting an early indication of whether a new initiative (e.g. in enforcement, policy, legislation etc) has had a big impact.

3.39 Another disadvantage of this method is that it relies on a known “worst case scenario” method to determine the likely range of downward revisions, because there is always scope for a bigger revision in future. For example: 2011 was the biggest downward revision seen, and has thus been used to create the lower limit for the range method but a bigger revision could happen in future and would cause the final estimate to fall outside the range.

What will happen next

A summary of responses, including the next steps, will be published within three months of the consultation closing on DfT's website. Paper copies will be available on request.

If you have questions about his consultation please contact:

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If you would like to have an informal discussion about the consultation and your views, please email or call the number above to arrange.

Annex A: Full list of consultation questions

We welcome any feedback on the options proposed above or other suggestions to improve the provisional estimates. The questions below are intended as a guide if you wish to structure your response but we are very happy to receive more "open" responses as well.

C.1 What use do you make of the provisional and final drink driving estimates at present?

C.2 Which, if any, of the proposed options below do you think is the best future form for the provisional estimates and why?

Options for the provisional statistics

Option 0

- Retain provisional statistics with their current form and timing

Option 1

- Stop publishing provisional statistics entirely

Option 2

- Delay provisional statistics to later in the year to allow a larger, more representative sample to be gathered

Option 3

- Adjust the estimates to account for the bias

Option 4

- Produce the estimate as a range, rather than a single “best” estimate

C.3 If option 2 is your preferred option, when in the year do you think the provisional estimates should be published?

C.4 If option 3 is your preferred option, do you have any suggestions for an improved adjustment methodology?

C.5 If option 4 is your preferred option, do you have any other suggestions on how to construct a sensible range?

C.6 Do you have any views on hybrid options, combining aspects of options 2, 3 or 4

C.7 Do you have any other suggestions on improved methodologies for the provisional estimates?

C.8 Do you have any other comments or feedback?

Annex B: Consultation principles

The consultation is being conducted in line with the Government's key consultation principles which are listed below. Further information is available at https://www.gov.uk/government/publications/consultation-principles-guidance

If you have any comments about the consultation process please contact:

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