Discriminability in deception detection is not *d*:

Reporting the Overlap Coefficient for practitioner-accessible results

Short title: *Overlap in deception distributions*

Liam Paul Satchell*[1],

[1] Department of Psychology, University of Winchester

*Corresponding author information: Dr Liam Satchell, Department of Psychology, University of Winchester, Winchester, Hampshire, SO22 4NR, United Kingdom, (e-mail: liam.satchell@winchester.ac.uk).

Biography

Liam Satchell is a senior lecturer in psychology at the University of Winchester's Centre for Forensic and Investigative Psychology. He specialises in methodological and individual difference perspectives in applied research settings including investigative interviewing, policing, antisocial behaviour, education, sport and other contexts.

Abstract

Applied psychology aims to develop evidence-based conversation between researchers and practitioners. We should aim for these conversations to be more transparent and accessible, including in terms of how we summarise and discuss statistical analysis. However, classically deployed mean-difference statistics can hide shared variance between conditions and do not truly reflect researchers' aims of 'differentiating' or 'discriminating' conditions. Importantly, mean differences do not provide practitioners with meaningful guidance on how to interpret one case at one point in time. Here, through focusing on deception detection research I provide an introduction to using the overlap coefficient (OVL) to enhance research-practice conversations. I highlight that even large mean differences ($d = 3.00$) can have one in ten cases presenting ambiguously ($OVL = 0.13$). I argue that reporting the overlap (and non-overlap) values and framing our results in terms of 'percentage of cases differentiated', allows us to better communicate our findings to practitioners. The use of the OVL statistic allows us to temper and expand the reporting of findings in applied psychology and will enhance practitioner-research communication.

Keywords:

Deception detection; Applied research; overlap statistics; methodology; communication of results

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The aim of applied psychological research is to develop an evidence base for informing practice. However, it is often the case that traditional academic conventions are not the most informative way to interpret and communicate findings for practice. For example, it is a common approach in applied research to develop experiments that attempt to elicit differences between two (or more) groups, and then inform practitioners about these differences to encourage practice change. But it remains a challenge to define at what point do we consider these researched differences *meaningful* and important for practice. This article highlights the limitations of using classical statistical inference criteria in a case of applied research. The aim is to inform researchers how we might be over- and underestimating the utility of our results for practice when not looking at the distributions of our conditions. Further, I am to inform practitioners of questions they may want to ask of research data presented to them. I use the example of deception detection research as an active research area in which these concerns might be important, but the content discussed here applies widely to applied experimental research.

The typical approach used in developing deception detection techniques in academic research is to start by defining a potential cue to deception (such as non-verbal utterances or number of spatial details mentioned). Then the researchers randomly allocate a sample of participants to deliver an honest or a deceptive statement in an experimental manipulation. Thus, creating two (or more) conditions. Then statements provided by participants are assessed for differences in the interview aspect of interest (e.g. non-verbal utterances, etc) between the two conditions. Then statistical tests used demonstrate any differences in presence of this aspect between lie- and truth-tellers. These differences are deemed 'noteworthy' using the preferred heuristic of the researchers – with tools which can observe differences in a variety of ways such as p, BF_{10} or non-0 overlap of 95% CI of effect sizes. If the difference is considered noteworthy, the researchers then suggest that this is an interview aspect that is indicative of truth- or lie-telling.

Researchers are known for using differences in such interview aspects to suggest "discrimination" (Leins, Fisher, Vrij, Leal, & Mann, 2011, p264) between truths and lies. Some authors suggest that an interview aspect could be used as a "diagnostic cue to deception" (Liu et al., 2010, p35). The language of differentiating, discriminating, or diagnosing deception suggests that practitioners could use the interview aspect to detect a deceptive statement apart from a truthful statement in practice. However, this is not necessarily possible based on the inference from these classical mean-difference statistics alone. Whilst the average respondent for each group may feasibly differ in these features, there can be considerable overlapping variability between the *distributions* being compared. That is, whilst the average person may differ, in many cases the typical truth teller looks the same as the typical liar and vice versa. More attention is needed on observing the typical variability in each condition. The average comes with the spread caveat – the average point does not exist, but merely indicates the middle of comparable distributions. As researchers, in standard reporting, we include standard deviations as well as means to summarise distributions in our conditions, because of this recognition of population variability. However, we do not routinely use tests or descriptive language to show readers how much conditions vary and these variances overlap.

In practice, an interviewer making use of these researched cues to deception is (effectively) randomly sampling one person from somewhere in the true[1] distribution. If there is a notable overlap between lie- and truth-teller distributions it is not possible to easily attribute one interview to one veracity state. For example, Figure 1A compares a hypothetical truth and deceptive condition. Whilst the average person in these two groups does 'meaningfully' differ (in a similar size to many published studies), there are many people who present the same behaviour in the truth and deceptive condition (shaded region). If presented with a person doing '1' or '2' hypothetical behaviours in figure 1A, it would not be easy to tell if this was a sign of lying or truth-telling. Whilst this is somewhat known to researchers, this can have important implications for how we communicate these results to practitioners.

[1] It should be noted that it is a point of debate whether the studied distribution adequately represents the population that would be obtained in practice. I thank Dr Timothy Luke for his comment here.

Beyond reporting differences between the average interviewees in the study conditions, it could be useful to understand the ‘uniqueness’ of each lie- or truth-telling distribution. For example, in Liu et al’s (2010) study, the difference between lie- and truth-telling children’s refusal to answer questions was able to ‘differentiate’ 78% of participants into the lying or truth-telling condition. On the other hand, the ‘discriminability’ between lies and truths suggested by Leins et al’s (2011) use of coded consistency was not as effective. Coded consistency presented identically in 38% of participants, indicating that four in ten cases were not clearly truths or lies. Despite these both being statistically significant differences between groups, it can be seen that Liu et al.’s technique is a more useful tool for separating truths from lies. It is not the intention here to focus on these two papers or any particular papers as the use of ‘discriminability’ terminology when discussing mean differences in cues to deception data is widespread. However, these studies are good illustrations of what is missed when not considering condition distribution overlaps.

Due to the challenges of using common statistics to demonstrate mean differences as evidence for discriminability between groups, I propose that applied researchers should use the established ‘overlap coefficient’, as a method to show shared variance between groups. By using this underreported statistic, deception detection studies (and applied experimental research at large) would gain *i*) better discussions of findings with practitioners, *ii*) a more thorough conversation about the size of effects found in deception detection research and, *iii*) better understanding around the informativeness of heterogeneity in data for applied uses.

Since this paper was first drafted, other authors have presented discussions about using statistics on the overlap between lie and truth telling distributions as an insight into deception cue usefulness (see the third commentary in Nahari et al., 2019). This current paper adopts a different approach to the previously presented U3 or ‘DISCO’ suggestions in Nahari et al. Here, the overlap coefficient is preferred as it is more readily accessible to practitioners as an intuitive statistic (percentage of overlapping cases) and the ability for the overlap coefficient to highlight heterogeneity of effects (see below). The aim of this paper is also to serve as a practical introductory text for making results more intuitive and accessible to end users (building on work presented elsewhere: Satchell et al., 2017).

The Overlap Coefficient (OVL)

One way to evaluate shared variance between distributions is the "Overlap coefficient" (OVL, see (Hanel et al., 2019; Inman & Bradley, 1989). This coefficient has an opposite function to Cohen's *d* (see Diner, 2010), in that it expresses the shared variance between the two sampled populations as opposed to differences between means. An OVL value of 1.00 denotes 100% overlap between two distributions (equal distribution properties). An overlap coefficient of 0.00 reflects 0% overlap between two distributions (all cases are separated into two distributions). Here, the OVL of 0.00 reflects total discriminability; we can classify a statement as deceptive or truthful based on a particular cue alone. Alternatively, OVL of 1.00 suggests that there is no distinction between the distributions at all. Very recently Hanel et al., (2019) have written an introduction to OVL in the context of defining using overlap to highlight similarities between groups in general theoretical research. This paper would make good reading for readers interested in the academic research uses of the OVL coefficient, whereas the current paper focuses more on implications for practice.

Making analyses more accessible. Researchers working on the topic of police interviewing have the aim of suggesting improvements to the procedures used in the criminal justice system based in good evidence-based practice. For this to happen, researchers must convince policy makers and stakeholders of the utility of our research. It would be of benefit for researchers to use language that is readily understandable by those not well versed in academic statistics. The overlap coefficient can be easily discussed in these contexts by referring to the percentage of the tested cases that were indistinguishable. When OVL= .20, this denotes that 20% of participants were not clearly defined as being truth- or lie-tellers using that selected interview aspect. In a hypothetical example, we could count the number of smiles displayed by truthful (M= 20.00, SD= 16.00) or deceptive (M= 25.00, SD= 11.00) interviewees. It is possible that this hypothetical effect is significant (with even a modest sample size N_1= 60, N_2= 60; $t(118)$= 2.00, p= .048, d= .37) and researchers might advise practitioners that more smiles are a sign of deception. However, the calculated overlap for these two groups is OVL= .78. This would suggest that 78% of cases were not readily distinguishable as truthful or deceptive when using number of smiles as something to focus on as a cue to deceit. Alternatively, this finding could be reported as '*eight out of ten cases in this interview aspect cannot differentiate honest* *from deceptive behaviour*'. Despite the populations differing on average, the overlap shows that this cue is probably not usefully 'diagnostic'.

Researchers could also do more to consider the task of the practitioner who is only exposed to one event at one point in time. When investigating a single event, an interviewer does not have the context 60 honest and 60 deceptive interviewees to understand general differences in the performance between the groups on the specific instance being investigated. Instead, interviewers are exposed to a one random example of the distribution of interview aspects. Unless conducting an interview the 'average' person (who does not exist in real terms), mean differences between lie- and truth-tellers alone are not informative for guessing which category the current interviewee belongs to. When there is greater overlap in distributions of lie- and truth-telling it is difficult to use a particular interview aspect to make a veracity judgment. For example, the distributions in Figure 1A are not convincingly different despite easily being a statistically significant difference at $N_{\text{Truth}}= 35$, $N_{\text{Lie}}= 35$, $t(68)= 2.09$, $p= .040$, $d= .50$. However, Figure 1B shows a highly significant difference, with a notably large Cohen's *d* of 3.00. This type of finding is rare and the size of effect in 1B is bigger than typically found in the psychological literature (see Richard, Bond, & Stokes-Zoota, 2003); however, even in this case of this large *d*, OVL= .26. This means that in this sampled population, 26% of the performance in that interview aspect is indistinguishable between groups. In the generated data in figure 1B, an interviewee performance of '3' does not clearly indicate truthful or deceptive behaviour. One in ten events are not clearly attributable to deceptive or honest behaviour.

It could be of use to consider the opposite of the overlap coefficient, a non-Overlap coefficient (nOVL = 1-OVL). This nOVL statistic illustrates the percentage of non-shared cases. For example, for Figure 1B nOVL= 0.74. That is to say, 74% of cases are distinct when comparing truth tellers and liars on that hypothetical interview aspect. Further examination of the data is needed to define the critical levels where distinction occurs, but this nOVL value highlights the 'discriminability' that deception detection researchers wish to discuss.

Focusing our analysis on how different distributions are, highlights the limitations of our academic *d* heuristics for applied practice. It is the case that *d* (and *p* or BF_{10}) can tell us something about population differences at large and these statistics are of theoretical interest. However, a practitioner may be best informed by saying how much overlap there is between truthful and deceptive behaviour when given a specific interview aspect.

[Figure 1 here]

The Maximum Overlap of Interest (MOvI). What is the acceptable overlap between lie- and truth-telling distributions? Is an interview aspect with an OVL= .30 effective enough to be useful in applied practice? The question of 'smallest effects of interest' is complicated when applying psychology to criminal justice settings (for more on smallest effects of interest, see Lakens & Evers, 2014). The stakes are much higher in applied settings than in research of academic interest alone as lives can be radically changed based on criminal justice system decisions. If researchers assume our findings will meaningfully inform decision making, we could be concerned that our advice is based on an interview aspect that is statistically different between groups yet does not distinguish liars and truth tellers in 42% of cases (figure 1C). One could even consider 19% overlap between distributions cause for significant concern when working in high stakes settings (e.g., figure 1B).

Thus researchers should establish the acceptable maximum overlap for using their interview aspect in practice (and preferably in a preregistration). There is no reason for this current paper to set a standard recommendation for Maximum Overlap of Interest (MOvI), but individual researchers to provide justifications for their own MOvI. An author should establish that they consider, for example a MOvI of .20, to be the greatest amount of overlap they consider acceptable for an interview aspect to be used for guidance. In this case the MOvI established by the researcher still allows one in five ambiguous cases. It should be noted that, that many of our current approaches do not produce small OVL values. For example, in hypothetical data M_{Truth}= 25.00, SD_{Truth}= 8.00, M_{Lie}= 10.00, SD_{Lie}= 4.00, there is an OVL= .20 and d= 2.50. Even when MOvI are set at a modest level, distribution differences need to be large.

Discovering more than mean-differences in data. Presented in figure 1D is data with d= 0.00. By most standard measures of reporting, this would indicate 'no difference between groups.' This is not the case on observing the data. There is, in fact, a distinct difference in variance between groups and a difference that is meaningful for a practitioner. Let us assume that the fictional wide distribution illustrated figure 1D represents truth tellers (M_{Truth}= 1.00, SD_{Truth}= 5.00) and the hypothetical peaky distribution represents lie tellers (M_{Lie}= 1.00, SD_{Lie}= 1.00). There is more variability in the truth-tellers than the lie-tellers which may, perhaps, reflect the effect of strategic, controlled behaviour by lie-tellers as opposed to naturally varying behaviour of the truth tellers (in line with theory, see (Vrij, 2008). This would be highly relevant for researchers to observe and report on for practitioners, and unless observing the distributions, would be missed.

The distribution in figure 1D has a smaller mean difference than figure 1A (*d*= 0.50), however, the overlap coefficient draws attention to the fact that there is stronger discriminability of individual cases in figure 1D (OVL= .19) than 1A (OVL= .66). Whilst the overlap coefficient does not diminish the utility of reporting *d*, differences between *distributions* (i.e. potential occurrences of cases) are can be efficiently reported with nOVL.

A further advantage of comparing distributions of data rather than mean differences is the opportunity to draw on comparisons which do not make assumptions about the underlying distribution of data. Whilst many statistical tests assume that both distributions are normally distributed (and suitable for parametric analysis), developed distribution-free overlap coefficients (such as that provided by Pastore, 2018; Pastore & Calcagnì, 2019) allow comparison of the overlap between two non-normal distributions. This is an advance on what is offered by usual Cohen's *d* comparisons.

Calculating the overlap coefficient in R

Here, I briefly summary an experimental applied psychology relevant example of calculating the overlap coefficient in R. More detail on this code can be found in Pastore and Calcagnì, (2019). Table 1 describes an example of a hypothetical dataset; the number of self-corrections in statements by truth tellers (*M*= 2.32, *SD*= 1.80) and lie-tellers (*M*= 2.54, *SD*= 2.01) are recorded.

[Table 1 here]

The R output will return the OVL value of approximately .82. In this hypothetical dataset, 93% of cases are indistinguishable. Further, nOVL can be calculated using the newly found OVL by computing: 1-.82. This returns a nOVL value of .18, with only 18% of cases being distinct.

[Table 1 here]

Reporting to overlap as part of standard results. The hypothetical result from figure 1B could be written up as follows;

In the current study, there was a statistically significant difference between truth- ($M_{Corrections}$*= 4.00, SD= 1.00) and lie-tellers (*$M_{Corrections}$*= 1.00, SD= 1.00) in the number of self-corrections (*$t(98)= 15.00$*,* $p< .001$*,* $d= 3.00$*, OVL= .13). Using the number of interviewee self-corrections successfully discriminated between 87% of cases, which is superior to our defined preregistered MOvI of .20. On further studying the data, we found that four or more self-corrections were clearly indicative of truth-telling and no self-corrections was clearly indicative of lie-telling (see figure 1B). One to three events of self-correction were more ambiguous and not diagnostic in our data.*

Limitations of OVL. The OVL coefficient is not without limitations. It is the case that the OVL value is only useful for interpreting single cases when the data collection is large enough to represent the true population. Much like with tests for mean differences, the accuracy of estimating distributions improves with increased sample sizes. As with any approach in applied psychological research, we should be mindful of the many factors that contribute to the observed variance. Variability in study situation, (mock) investigators or interviewers, and backgrounds of participants all contribute to things that may lead our OVL estimate to not match the true distribution. Researchers should be cautious about interpreting interview aspects as differentiating truths and lies when only testing participants on one event. The OVL statistic, like all tests for group differences and similarity could only be used to predict performance on the selected standardised event. The larger the sampling of participants and events, the smaller the error in distribution estimates (i.e. smaller standard deviations) and the more effective the discriminability function will be.

The reporting and discussing of OVL statistics face the same concerns as general effect size reporting in deception detection work. Work by Luke (2019) has thoroughly presented concerns with the size of effects reported in deception detection research. That paper serves as a strong introduction to the issues of selective reporting, publication bias and inflated effect sizes found in the current deception detection literature. Similar issues could occur with future reporting of OVL. Authors are encouraged to pre-register their MOvI before running their studies for the upmost transparency. Further, OVL has value for peer reviewers and editors of research in this area could use their role to encourage the reporting of OVL for practitioner's access.

It is also worth noting that OVL is only an attempt to improve the communicability of applied research. It is to enhance the transparency of common methods in the field. However, giving advice based on whether one cue, signal, or outcome differs between experimental groups, is very different to the noisy, multivariate world of practice. There are broader methodological questions about looking for single cue differentiations between groups. Practitioners experience the gestalt whole of a person. One may be looking to observe 'self-corrections' as a cue to dishonesty, but it is an important question as to how relevant that cue is when considering the tone of speech, emotional context, interviewer-interviewee dynamics, other relevant evidential and linguistic cues and so on. Practice is more complex and contextual than univariate approaches and guidance to practitioners. Focusing on OVL helps give a good critical balance to the labelling of *d* as 'discriminability', but broad reform of the types of questions and methodologies used in interviewing research enhances our ability to answer applied issues. This is beyond the scope of this current paper to list potential methodological reforms to the investigative interviewing research, but important to recognise that using OVL is a way to address the analyses frequently deployed in this area of research.

Future directions

Fundamentally, applied psychology endeavours to provide assistance for practitioners who are experiencing one interview on one case at one time. To reach this aim, our research must be much more prescriptive to the individual case and communicate this to practitioners. As well as OVL and nOVL statistics, researchers could consider the benefits of 'normative' approaches to behaviour. Like IQ and applied psychometric use, academics could consider norm-scoring individual cases against the possible distribution of performance. *Z*, or the more accessible *T*, scores would be able to index individual cases. For figure 1B, a case with $T = 65$ is easily defined as an 'above average' number of self-corrections and is more likely to belong to the truth-telling distribution (which we could define, in this case, as $T > 55$). The purpose here is to be able to identify the 'atypicality' of one interview, assuming there is a known population parameter from a variety of events and participants. This could lead to create a stronger evidence trail for those making use of research in practice.

I have focused my commentary on deception detection research as an example of providing practitioners with advice about differentiation between two groups. However, the discussion presented here applies equally to many areas of forensic and legal psychology, such as comparing techniques for interviewing witnesses or risk assessment methods. In fact, this is equally relevant for broader uses of applied psychology. It is important to have academic and theoretical criteria of meaningful differences, and these may well be different to those used in practice. However, applied psychology treads and important line and must recognise that liberal discrimination criteria can have serious consequences for those working in psycholegal practice.

Summary

Overall, the OVL (and nOVL) statistics could improve the way in which we discuss techniques designed to detect truth- and lie-tellers. More than telling end-users that there is a 'statistically significant difference' in this interview aspect or d= .80 for the difference between lie- and truth-tellers, practitioners may wish to know 'using this interview aspect, we can clearly separate lie- and truth-tellers in 31% of cases'. In this case, we see the academically interesting d= .80 has an OVL= .69 and may not be of much use in applied practice. We can also have a clearer conversation about the MOvI for applied researchers about their acceptable error rate for not being able to clearly distinguish between truthful and deceptive statements. The OVL statistic also highlights cases where heterogeneity in variances is informative. When two groups might perform differently in terms of their variance but not their means (Figure 1D) the percentage overlap highlights a difference where d does not. Focusing the distinctiveness of *distributions* as opposed to differences in *means* would benefit many streams of applied research.

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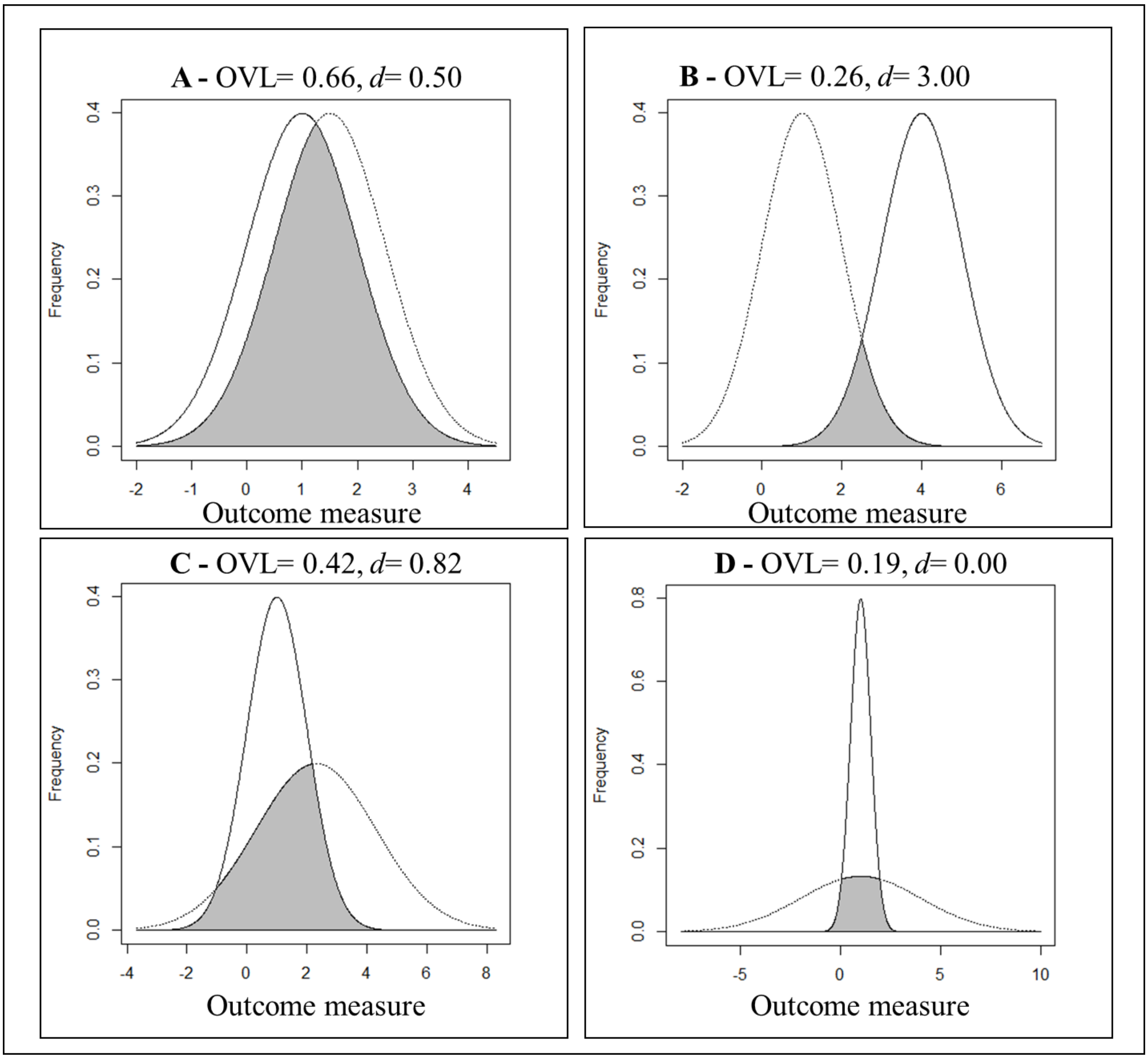

Figure 1. Four examples (A, B, C and D) of different types of distribution that could be encountered in deception detection research. All data is generated to illustrate arguments made in text and is not real data. OVL defines the overlap coefficient and *d* defines Cohen's *d.* Note that figures are based on normal distribution projects and can overlap 0 despite all values being positive integers.

Table 1. *Code for deployment in R to calculate the Overlap coefficient*

```
#Rows starting with # are instructions and R will not run these
lines.
#Install and call the package called ‘overlapping’
install.packages("overlapping")
library(overlapping)

#Build data frame for testing by assigning key variables for
'truth' and 'deception'
data <- list(truth = truthcondition_variable,
             deception = deceptioncondition_variable)
#Run the analysis
summary <- overlap(data)
#Get report on overlap numbers
summary$OV
#Produce a figure of distribution overlap
final.plot(data)

#Worked Example
#Simulate hypothetical data based on two conditions of n= 35 for
example
#Here we simulate mean self-corrections for truth as 2.23, SD=
1.80
truthcondition_selfcorrections <- rnorm(35, 2.32, 1.80)
#Here we simulate mean self-corrections for deception as 2.54, SD=
2.01
deceptioncondition_selfcorrections <- rnorm(35, 2.54, 2.01)
#Using this simulated data, compute overlap by making a data
frame…
data <- list(truth = truthcondition_selfcorrections,
             deception = deceptioncondition_selfcorrections)
#And then running these functions
summary <- overlap(data)
summary$OV
#Returns the OVL value
```