Unconscious lie detection as an example of a widespread fallacy in the Neurosciences

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Neuroscientists frequently use a certain statistical reasoning to establish the existence of distinct neuronal processes in the brain. We show that this reasoning is flawed and that the large corresponding literature needs reconsideration. We illustrate the fallacy with a recent study that received an enormous press coverage because it concluded that humans detect deceit better if they use unconscious processes instead of conscious deliberations. The study was published under a new open–data policy that enabled us to reanalyze the data with more appropriate methods. We found that unconscious performance was close to chance – just as the conscious performance. This illustrates the flaws of this widely used statistical reasoning, the benefits of open–data practices, and the need for careful reconsideration of studies using the same rationale.
Introduction

Lie detection is of considerable importance to modern society, in particular in connection with police investigations, court proceedings, and security questions. For example, the U.S. government invests large amounts of money for training “behavior detection officers” to detect terrorists from their behavior at airports. These programs have been criticized for being irrational (Tierney, 2014) because scientific evidence suggests that humans are only correct in approximately 54% of lie–truth judgments (Bond & DePaulo, 2006). This is essentially as good as flipping a coin. In this context, the recent lie detection study (ten Brinke, Stimson, & Carney, 2014) presents the surprising finding that unconscious processes are much better in detecting liars than conscious processes. Consequently, the study received enormous attention with potentially far–reaching practical consequences. For example, consider jurors at court were advised “Truth or lie — trust your instinct, says research” (Briggs, 2014; Anonymous, 2014). This could make it very difficult to allow for a rational debate in cases where the truth does not seem as obvious as our instinct might suggest (Loftus, 2003). We show that the lie detection study does not provide “strong evidence” that “consciousness interfere[s] with the natural ability to detect deception” (ten Brinke et al., 2014, p. 6).

The reasoning used by the lie detection study, as well as by many other studies in the Neurosciences, is illustrated in Fig. 1. Participants watched two videos of interrogations. In one video the suspect was lying, in the other the suspect was telling the truth. Participants did not know who was the liar. The goal of the study was to find out whether participants could tell apart liars from truth–tellers (e.g., from signs of stress). After watching the videos, participants performed two tasks. The “direct” task (Fig. 1A) is assumed to tap conscious processes because the participants simply see pictures of the suspects and classify these pictures as truth–tellers or liars. As expected (Bond & DePaulo, 2006), participants were very bad in this direct task (49.6% correct, with chance level being 50%).

Things seemed to change drastically when the “indirect” task (Fig. 1B) was performed, which is assumed to tap unconscious processes. Now the pictures of the suspects were presented only briefly (“prime”) and hidden from consciousness by special masking techniques. The participants sorted well visible words (the “targets”) like “honest” or “deceitful” into the categories “truth” or “lie”. Interestingly, participants were significantly faster if such a word was preceded by a congruent picture of a suspect (e.g., the word “deceitful” was preceded by a picture of a liar) than if the word was preceded by an incongruent picture (e.g., the word “deceitful” was preceded by a picture of truth–teller). This can only be explained if some information

1 Selected press coverage (retrieved Mar–May 2014): New York Times, Apr. 26 [http://www.nytimes.com/2014/04/27/business/the-search-for-our-inner-lie-detectors.html] Science Magazine, Apr 1 [http://news.sciencemag.org/signal-noise/2014/03/spot-liar-trust-your-instinct] BBC, Mar 29 [http://www.bbc.com/news/health-26764868] British Psychological Society, Mar 28 [http://www.bps.org.uk/news/our-subconscious-mind-may-detect-liars] Süddeutsche Zeitung, Mar 27 [http://www.sueddeutsche.de/wissen/psychologie-unterbewusstsein-durchschaut-unehrlichkeit-1.1923587] The Times, Mar 26 [http://www.thetimes.co.uk/tto/science/article4045032.ece] Pacific Standard, Mar 25 [http://www.psmag.com/navigation/health-and-behavior/unconscious-mind-better-detecting-lies-77368] Science Daily, Mar 24 [http://www.sciencedaily.com/releases/2014/03/140324104520.htm] New Scientist, Mar 23 [http://www.newscientist.com/article/mg22129610.700-invisible-how-to-see-through-lies.html]
### A. Direct task

| hidden attribute | required response | Result |
|------------------|-------------------|--------|
| A                | A                 | Classification accuracy bad |
| A                | B                 | (not significantly above chance) |
| B                | B                 | 

### B. Indirect task

| hidden attribute | visible attribute | required response | Result |
|------------------|-------------------|-------------------|--------|
| A                | A                 | A                 | fast |
| A                | B                 | B                 | slow |
| B                | A                 | A                 | slow |
| B                | B                 | B                 | fast |

Fig. 1: Experimental rationale and fallacy: Typically there exists some hidden stimulus attribute. In the lie detection study this was whether the picture of a suspect showed a truth–teller or a liar. In other studies this could be the numerical size of a number or the emotional expression of a face that is hidden from consciousness by masking techniques. **A. Direct task:*** When participants directly classify the hidden attribute, they typically perform badly. **B. Indirect task:*** Nevertheless, the hidden attribute (“prime”) can affect RTs if participants perform a task on another well visible stimulus (“target”). In the lie detection study, participants decided whether well visible target–words were related to lying or truth–telling. They were faster if the targets were preceded by a congruent but hidden picture (e.g., the word “deceitful” preceded by the picture of a liar). While this is only possible if the hidden attribute was somehow processed by the nervous system, the fallacy is to conclude that there was relatively good unconscious classification accuracy of the hidden attribute, better than in the direct task.

However, the authors of the lie detection study (ten Brinke et al., 2014) derived further reaching conclusions from the significant congruency effect — as is common practice in the Neurosciences. They concluded, that (i) the significant congruency effect indicates “accurate unconscious assessments” (p. 7) of truth–tellers vs. liars; (ii) in parallel to this accurate unconscious processing, there exists another, inaccurate conscious process; (iii) the accurate unconscious assessments can even be “made inaccurate [...] by conscious” processes (p. 7), such that it might be wise to prevent “conscious deliberation about credibility” (p. 7).

We show below that all these conclusions are not warranted by the data. More generally, we describe that a significant congruency effect alone does not provide sufficient evidence for such
conclusions.

The fallacy

The main reason is that while the significant congruency effect indeed suggests that the primes have been classified to a certain extent, it does not indicate how good this classification was. The test for a significant difference between reaction times (RTs) in congruent and incongruent trials is only concerned with the question whether a ‘true’ difference exists in the population at all. The test does not tell us how big this difference is and for how good a classification performance it could be harnessed.

In a nutshell: The fallacy is to conclude from a significant effect in the indirect task that there has been good indirect classification performance of the prime (at least better than the classification performance in the direct task). However, the significant effect only indicates that some information about the stimuli has been processed, not how much information. Given enough statistical power, the indirect classification performance could be arbitrarily small while nevertheless there could be a significant congruency effect. This is not only a remote theoretical danger, as we show with our reanalysis of the lie detection study.

Reanalysis of lie detection data For the reanalysis, we put the data of the lie detection study to the test: If the significant congruency effect on RTs is supposed to serve as evidence for good unconscious processing, then we should be able to use the RTs to decide for each trial whether the prime and target stimuli were congruent or incongruent. Small RTs would indicate a congruent trial, large RTs would indicate an incongruent trial.

We applied two classifiers to the data: (i) the statistically optimal classifier under the assumption that RTs follow normal or lognormal distributions (Ulrich & Miller, 1993) and (ii) a model–free classifier trained on the data according to the standard protocol from statistical learning (for details please consult the methods section). The two classifiers achieve classification accuracies of (i) 50.6% and (ii) 49.3%. We also found that (iii) on the given data there cannot exist a classifier with accuracy larger than 54% — the same value that was interpreted as “detection incompetence” in the lie detection study (ten Brinke et al., 2014, p. 1). In short: the classification accuracy in the unconscious task is just as dismal as in the conscious task and can for all practical purposes be considered as being at chance level. There is no evidence for “accurate unconscious assessments” (ten Brinke et al., 2014, p. 7).

Fig. 2A illustrates this with the distributions relevant for classification performance. The average RT–difference between congruent and incongruent conditions was only 4.4 ms, whereas

2Of the two experiments in the lie detection study (ten Brinke et al., 2014), we concentrate on the second one, as this is the one that presents ‘unconscious’ stimuli. For the first experiment we obtained similar results (classification accuracy: 51.1%). All analyses were implemented twice independently, once in Matlab and once in R. The R–code is open available, see Part 1 of Materials and Methods.
Fig. 2: RT-Distributions relevant for classification and significance tests. A. Accurate classification of congruent vs. incongruent trials requires distinct RT–distributions. The top panels show RT-histograms for exemplary participants (left/right: participant with median/maximal accuracy of 50.8%/56.1%). The large panel shows RT–distributions for an idealized participant, based on average values and lognormal distributions (Ulrich & Miller, 1993). All distributions overlap so heavily that classification accuracy is essentially at chance, showing that the RTs convey hardly any information about congruent vs. incongruent trials. B. A significant difference requires distinct distributions for the mean RTs of congruent vs. incongruent trials; with the standard deviation given by the standard error of the mean (SEM; e.g., Franz & Loftus, 2012). These distributions are clearly distinct, reflecting the significant difference \( t(65) = 2.22, p = 0.03 \); mean difference: 4.4 ms, SEM: 2.0 ms, Cohen’s d: 0.27; cf. Cohen, 1988). Comparing A. and B. shows that classification accuracy can be at chance level even though the means are significantly different. This is caused by the massive reduction of the relevant standard deviation when calculating the SEM (cf. Part 3 in Materials & Methods). Note, that we even had to change the scale of the abscissa in B to show the distributions appropriately. C. Histograms of RTs in behavioral task of Dehaene et al. (1998). The distributions overlap heavily, suggesting that classification accuracy will be low. The histogram corresponds to Figure 2b of Dehaene et al. (1998) and was electronically digitized from the printed version. In all plots, dashed/solid lines indicate congruent/incongruent conditions.
the average within–subjects standard deviation was 146.5 ms. This gives a signal–to–noise ratio of 0.03, which is much too small for a meaningful classification performance.

To understand why this can happen even though the RT means are significantly different, note that the classification whether a trial is congruent or incongruous has to be performed on a single-trial basis. In particular, the accuracy of the classifier does not improve with more data. The statistical test for the difference in population means, on the other hand, is based on the estimated variability of the sample means, which gets smaller with more data. As shown in Fig. 2B, it can easily happen that two distributions are nearly indistinguishable by a classification task, yet a tiny difference in their means becomes significant if the sample size or the number of repetitions are large enough. See Part 2 in Materials & Methods for more details.

**Better approaches.** What would a more appropriate approach look like? For a meaningful comparison, we have to look not only for a significant effect, but also at how much information is transmitted by this effect to the task of classification. A straightforward way to do this is to consider the classification accuracy directly, as we did above. Other approaches are possible as well. For example, one could use signal detection theory (Swets, 1961) on both tasks to determine and compare appropriate d–prime values — as has been done in some studies (Schmidt, 2002; Gegenfurtner & Franz, 2007; Schmidt & Vorberg, 2006). Alternatively, one could apply classic information theory on both measures (Shannon, 1948), an approach we are currently working on. For the lie detection study, all these methods would lead to the same conclusion: unconscious lie detection does not work any better than its conscious counterpart. Both are essentially at chance–level.

**The problematic reasoning is widely used**

One might argue that this is a limited problem of one single study. However, the problematic reasoning is widely and routinely used. For illustration, we sketch three highly influential studies (Dehaene et al., 1998; Morris, Öhman, & Dolan, 1998; Pessiglione et al., 2007). Many more studies exist in the literature.

Dehaene et al. (1998) investigated whether humans can unconsciously process information about the magnitude of numbers. Stimuli were numbers between 1 and 9 that were hidden from consciousness by masking. Participants categorized whether the numbers were larger or smaller than 5. In direct tasks (Fig. 1A) participants were not significantly different from chance level (52.6% and 54% correct). Nevertheless, the masked numbers had significant effects in indirect tasks (Fig. 1B): If participants responded to a target number that could be congruent with the prime (e.g., both smaller than 5) or incongruent (e.g., one smaller and the other larger than 5), then participants showed significant effects on RTs and significant lateralizations in electroencephalography (EEG) and functional magnetic resonance imaging (fMRI). Based on the same reasoning as outlined above, Dehaene et al. (1998) concluded that in the indirect task participants "unconsciously appl[ied] the task instructions to the prime, would therefore categorize it as smaller or larger than 5, and would even prepare a motor response appropriate
to the prime” (p. 598). The authors summarize “that a large amount of cerebral processing [...] can be performed in the absence of consciousness” (p. 599).

However, these significant differences in RTs, EEG and fMRI measurements do not tell whether classification accuracy in the indirect task was better than in the direct task. If not, then there would be no evidence for unconscious processing of the primes. We cannot directly evaluate the relevant classification performance because we do not have access to the data. Instead, we analyzed the published histogram of all RTs performed in the behavioral task (Fig. 2C). If we determine the classification accuracy based on these distributions, we obtain 55% correct, which is discomfortingly close to the accuracy in the direct tasks. While this is only a very rough estimate, it cannot rule out the possibility that there might indeed be a similar problem in the study by Dehaene et al. (1998) as we found for the lie detection study. The only way to find out would be replication studies or a reanalysis of the existing data.

Morris et al. (1998) investigated emotional learning in the amygdala. Two angry faces were used as stimuli, one of which had been conditioned to an aversive event. The faces were hidden from consciousness by masking, such that participants were at chance when classifying whether such a face was shown to them. Nevertheless, activity in the right amygdala was significantly modulated by the fact that one of the two faces had been associated with the aversive event, as measured with positron emission tomography (PET). Using again the same reasoning, the authors conclude that “we provide the first evidence that the human amygdala can discriminate the acquired behavioral significance of stimuli without the need for conscious perception” (p. 469). Our critique is again: The significant modulation of amygdala activity does not show whether there is also good classification accuracy that is clearly different from chance level and that would justify the conclusion of a superior process operating in parallel to the conscious process.

Pessiglione et al. (2007) investigated subliminal motivation. Images of coins were presented, either one pound or one penny, and hidden from consciousness by masking, such that participants were at chance level when classifying the coins. Nevertheless, activity in the ventral pallidum (VP) was significantly modulated by the value of the coins, as measured by fMRI (similar results were found for skin conductance and grip force). The authors concluded that there are two motivational processes, one conscious and the other unconscious: “Thus, only the VP appeared in position to modulate behavioral activation according to subliminal incentives and hence to underpin a low–level motivational process, as opposed to a conscious cost–benefit calculation” (p. 906). Our concerns are again the same: The significant modulation of activation does not tell whether the information available to the VP suffices for a classification performance that is clearly better than the conscious classification performance. Therefore, it is not clear whether the authors’ assumption of two processes (an unconscious and a conscious one) for cost–benefit calculation is warranted.
Is there unconscious processing?

Because all our example studies happen to be related to the question of whether there exists unconscious processing independent of and parallel to conscious processing, we want to preclude a potential misunderstanding. We are mainly interested in describing the methodological fallacy, not in discussing unconscious processing. Such a discussion would go beyond the scope of this article and would have to take into account a long history of research (Eriksen, 1960; Holender, 1986; Reingold & Merikle, 1988; Greenwald, Draine, & Abrams, 1996; Hannula, Simons, & Cohen, 2005; Kouider & Dehaene, 2007). Therefore, we do not claim that unconscious processing independent of conscious processing does not exist or cannot be shown. We do, however, claim that the lie detection study does not provide evidence for a superior unconscious lie detection ability and that this study shows in an exemplary way how the claims of the other studies using the same flawed rationale can go astray and need careful reconsideration using more appropriate methods.

Conclusions

We described a reasoning that is widely used but flawed. In the case of the lie detection study (ten Brinke et al., 2014), the commendable open–data practice allowed us to show in an exemplary way how this reasoning can lead to wrong conclusions. More generally, conclusions of the many studies using this reasoning should be treated with caution and could be wrong. In the future, we should employ better statistical methods and conclusions based on the flawed reasoning should be reconsidered.

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3Note that our conclusions on the lie detection study (ten Brinke et al., 2014) are corroborated by a recent commentary of lie–detection experts (Levine & Bond, 2014) who question the plausibility of the lie-detection results in the light of other research and meta–analyses in this area. While Levine and Bond (2014) had to speculate that one of the conditions is a statistical outlier we can now show the statistical reasons behind the wrong conclusions. Hence, our findings converge with the intuition and meta–analytic data of these lie–detection experts.
Materials and Methods

1 Classification and statistical optimality in more detail

In this section, we describe how an optimal classification of the single trial data in an indirect task (Fig. 1B of main text) can be performed. We prove below that under the assumption that the RTs follow a normal distribution or a lognormal distribution (Ulrich & Miller, 1993), the statistically optimal classifier is given by a median split of the reaction times (“median classifier”). We also describe a typical classifier as used in machine learning that does not require any distributional assumptions (“trained classifier”). Finally, we derive a theoretical upper bound for classification performance on the given data that in principle can never be exceeded (“over–optimistic upper bound”). Before going into details, we first describe the results of applying these classifiers to the data of the lie detection study (ten Brinke et al., 2014).

Classification results for lie detection study. For each participant, the goal is to classify the trials in the indirect task as ‘congruent’ or ‘incongruent’, based on the RTs of this participant. For each participant, we proceed as follows. (i) For the model–based median classifier, we compute the median RT, use this as the threshold of a step function classifier (see below), and compute the accuracy of this classifier over all trials. (ii) For the model–free trained classifier, we randomly split the trials into a training and test set of 50% each (other split sizes lead to very similar results). We determine the best threshold on the training set, and compute the resulting accuracy on the test set. We repeat this procedure 10 times with different random splits of the data and report the average over these test accuracies. (iii) For the over–optimistic upper bound, we evaluate the accuracy of all possible thresholds for the step function classifier over all trials and report the best result. The following table shows means and standard deviations over the accuracies of all participants:

| Method                                      | mean(accuracy) | std(accuracy) |
|---------------------------------------------|----------------|---------------|
| (i) Median classifier (model: lognormal)    | 50.61%         | 2.65%         |
| Median classifier (model: normal)          | 50.61%         | 2.65%         |
| (ii) Trained classifier (model–free)        | 49.34%         | 2.64%         |
| (iii) Over–optimistic upper bound           | 53.73%         | 1.99%         |

We can see that both the model–based (i) and model–free (ii) classifiers perform nearly exactly at chance level. The over–optimistic upper bound shows that on this data set, there does not exist a classifier that can obtain an accuracy higher than 54% — the value that was interpreted as “detection incompetence” in the lie detection study (ten Brinke et al., 2014, p. 1).

General form of the optimal classifier. Consider a classification task where the input is a real-valued number \( x \) (e.g., a reaction time, RT), and the classifier is supposed to predict one
of two labels $y$ (e.g., 'congruent' or 'incongruent'; for simplicity we use labels 1 and 2 in the following). Following the standard setup in statistical decision theory (Bishop, 2006, section 1.5) we assume that the input data $X$ and the output data $Y$ are drawn according to some fixed (but unknown) probability distribution $P$. This distribution can be described uniquely by the class-conditional distributions $P(X \mid Y = 1)$ and $P(X \mid Y = 2)$ and the class priors $\pi_1 = P(Y = 1)$ and $\pi_2 = P(Y = 2)$. A classifier is a function $f : \mathbb{R} \to \{1, 2\}$ that assigns a label $y$ to each input $x$. The classifier that has the smallest probability of error is called the Bayes classifier. In case the classes have equal weight, that is $\pi_1 = \pi_2$, the Bayes classifier has a particularly simple form: it classifies an input point $x$ by the class that has the higher class-conditional density at this point. Formally, this classifier is given by

$$f_{opt}(x) := \begin{cases} 1 & \text{if } P(X = x \mid Y = 1) > P(X = x \mid Y = 2) \\ 2 & \text{otherwise.} \end{cases}$$  \hspace{1cm} (1)$$

**Optimal classifier for normal and lognormal distributions.** We now consider the special case where the class-conditionals follow a particular distribution. Let us start with the normally distributed case. We assume that both class-conditionals are normal distributions with means $\mu_1, \mu_2$ and equal variance $\sigma^2$, and we denote their corresponding probability density functions (pdfs) by $\varphi_{\mu_1,\sigma}$ and $\varphi_{\mu_2,\sigma}$. Under the additional assumption that both classes have equal weights $\pi_1 = \pi_2 = 0.5$, the cumulative distribution function (cdf) of the input (marginal distribution of $X$) is given as

$$\Gamma(x) := 0.5 \cdot \left( \Phi \left( \frac{x - \mu_1}{\sigma} \right) + \Phi \left( \frac{x - \mu_2}{\sigma} \right) \right),$$

where $\Phi$ denotes the cdf of the standard normal distribution. For $t \in \mathbb{R}$, we introduce the step function classifier with threshold $t$ by

$$f_t(x) := \begin{cases} 1 & \text{if } x \leq t \\ 2 & \text{otherwise.} \end{cases}$$  \hspace{1cm} (3)$$

In the special case where the threshold $t$ coincides with the median of the marginal distribution of $X$, we call the resulting step function classifier the median classifier.

**Proposition (Median classifier is optimal for normal model)** If the input distribution is given by Eq. (2), then the optimal classifier $f_{opt}$ coincides with the median classifier.

**Proof.** Because both classes have the same weight of 0.5, the Bayes classifier is given by $f_{opt}$ as in Eq. (1). For any choice of $\mu_1, \mu_2$ and $\sigma$, the class-conditional pdfs $\varphi_{\mu_1,\sigma}$ and $\varphi_{\mu_2,\sigma}$ intersect exactly once, namely at $t^* = (\mu_1 + \mu_2)/2$. By definition of $f_{opt}$, the optimal classifier $f_{opt}$ is then the step function classifier with threshold $t^*$. We now compute the value of the cdf at $t^*$:
\[ \Gamma(t^*) = 0.5 \cdot \left( \Phi\left(\frac{t^* - \mu_1}{\sigma}\right) + \Phi\left(\frac{t^* - \mu_2}{\sigma}\right) \right) = 0.5 \cdot \left( \Phi\left(\frac{\mu_2 - \mu_1}{2\sigma}\right) + \Phi\left(\frac{\mu_1 - \mu_2}{2\sigma}\right) \right) = 0.5 \cdot \left( \Phi\left(\frac{\mu_2 - \mu_1}{2}\right) + (1 - \Phi\left(\frac{\mu_2 - \mu_1}{2}\right)) \right) = 0.5. \]

Here, the second last equality comes from the fact that the normal distribution is symmetric about 0. This calculation shows that the optimal threshold \( t^* \) indeed coincides with the median of the input distribution, which is what we wanted to prove. \( \square \)

It is easy to see that this proof can be generalized to more general types of symmetric probability distributions. It is, however, even possible to prove an analogous statement for lognormal distributions, which are not symmetric themselves. We introduce the notation \( \lambda_{\mu,\sigma} \) for the probability density function (pdf) of a lognormal distribution, and \( \Lambda_{\mu,\sigma} \) for the corresponding cdf. These functions are defined as

\[ \lambda_{\mu,\sigma}(x) := \frac{1}{x \sigma \sqrt{2\pi}} \exp\left( -\frac{(\log x - \mu)^2}{2\sigma^2} \right) \quad \text{and} \quad \Lambda_{\mu,\sigma}(x) := \Phi\left(\frac{\log x - \mu}{\sigma}\right). \]

Consider the case where the class-conditional distributions are lognormal distributions with same scale parameter \( \sigma \) but different location parameters \( \mu_1 \) and \( \mu_2 \), and assume that both classes have the same weights \( \pi_1 = \pi_2 = 0.5 \). Then the pdf and cdf of the input distribution (marginal distribution of \( X \)) are given as

\[ g(x) = 0.5 \cdot ( \lambda_{\mu_1,\sigma}(x) + \lambda_{\mu_2,\sigma}(x) ) \]
\[ G(x) = 0.5 \cdot ( \Lambda_{\mu_1,\sigma}(x) + \Lambda_{\mu_2,\sigma}(x) ). \]

(4)

**Proposition (Median classifier is optimal for lognormal model)** If the input distribution is given by Eq. (4), then the optimal classifier \( f_{\text{opt}} \) coincides with the median classifier.

**Proof.** The proof is analogous to the previous one. For any choice of \( \mu_1, \mu_2 \) and \( \sigma \), the densities \( \lambda_{\mu_1,\sigma} \) and \( \lambda_{\mu_2,\sigma} \) intersect exactly once. To see this, we solve the equation \( \lambda_{\mu_1,\sigma}(t^*) = \lambda_{\mu_2,\sigma}(t^*) \), which leads to the unique solution \( t^* = \exp((\mu_1 + \mu_2)/2) \). The input cdf at this value can be computed as

\[ G(t^*) = 0.5 \left( \Lambda_{\mu_1,\sigma}(t^*) + \Lambda_{\mu_2,\sigma}(t^*) \right) = 0.5 \left( \Phi\left(\frac{\mu_2 - \mu_1}{2\sigma}\right) + \Phi\left(\frac{\mu_1 - \mu_2}{2\sigma}\right) \right) = 0.5. \]

The last step follows as above by the symmetry of the normal cdf. \( \square \)
**Training a model–free classifier.** If we do not want to make any assumptions about the underlying probability distribution, we can follow the standard protocol of statistical learning to identify the threshold $t$ of the best step function classifier. For each participant, we are given trials in form of input-output pairs $(X_i, Y_i)_{i=1,...,n}$, $X_i \in \mathbb{R}$, $Y_i \in \{1, 2\}$. We randomly split this data set into a training set consisting of 50% and a test set of the remaining 50% of all trials. On the training set, we determine the threshold $t^*$ that leads to the smallest number of misclassifications (= training error). For the corresponding step function classifier $f_{t^*}$ we now compute the error on the test set (= test error). We repeat this procedure 10 times to remove potential subsampling artifacts and report the mean over these repetitions. For readers familiar with machine learning, note that in this simple scenario, no model selection is involved, so a more complex evaluation procedure such as cross validation is not necessary.

**An over–optimistic upper bound on classification accuracy.** To rule out the case that the result of the model–free classifier is seriously sub-optimal (due to the effect of splitting the data in training and test sets, or due to overfitting or underfitting), we can derive an upper bound on the accuracy of the best step function classifier that possibly exists on the given data. For each participant, we cycle through all possible thresholds $t$ and evaluate the accuracy of the corresponding step function classifier $f_t$ on all trials. We then select the best accuracy obtained in this way as the classification accuracy of this participant. This accuracy is overly optimistic, as this classifier usually overfits and exploits sampling artifacts. On the other hand, it gives an upper bound on the classification accuracy that any other step function classifier could potentially achieve on the data. Finally, note that in the context of the RT experiment, it would not make sense to consider classifiers that do not have the form of a step function classifier — the general classification scenario implied by the experimental setup is to separate slow RTs from fast RTs.

### 2 Why is the relevant standard deviation for the significance test much smaller than that for classification?

Let us illustrate our answer with the data of the lie detection study (ten Brinke et al., 2014). Consider two probability distributions with slightly different expected values, such as the ones in Fig. 2A. The task of the classifier is to predict for each trial whether the measured RT has been generated from a congruent or an incongruent condition. More abstractly, given a real-valued sample, we want to decide which of the two distributions is more likely to have generated that sample point. In general, this will only be possible in a satisfactory manner if the two distributions have only little overlap and their means are considerably different from each other.

The significance test, on the other hand, assesses whether the expected values of the two distributions are different at all. It does not ask for a large difference, it just asks for any difference. The more measurements are taken, the closer each mean estimate will be to the corresponding expected value. We know from the central limit theorem that the SEM is of order
In the limiting case of an infinite number of measurements, the SEM would approach zero.

To get a feeling for this effect, consider the data of the lie detection study. For a rough estimate, let us for a moment ignore the fact that there were different participants (i.e., that between-subjects variability exists; this is not so critical because we are dealing with within-subjects designs such that the difference mainly is affected by within-subjects variability (Franz & Loftus, 2012)). The average standard deviation for a trial was 146.5 ms (Fig. 2A). Each condition was measured about 180 times in each participant and the study had 66 participants, which leads to a factor of $\frac{1}{\sqrt{180 \cdot 66}}$. Taking the difference between the congruent and incongruent conditions increases the SEM by a factor of $\sqrt{2}$ (assuming for simplicity independence and equal variances), such that a rough prediction for the SEM relevant for the significance test is given as $146.5 \cdot \sqrt{2} / \sqrt{180 \cdot 66} \text{ms} = 1.9$ ms. This is close to the empirically obtained 2.0 ms.

3 If the means are significantly different, doesn’t this imply that the classification accuracy is significantly different from chance level?

If we have enough statistical power, significance tests will eventually show that classification is different from chance if the means are significantly different. (In real data, both significances might not occur at the same time because sources of noise are not exactly identical for the means and the classification results.)

However, with regard to the reasoning outlined in Fig. 1 of the main paper, this question is misleading. For the typical neuroscientific interpretation it is not only important that the classification accuracy in the indirect task is significantly different from chance level (this could also happen if the true classification performance were, say, 51%). What counts is whether the classification accuracy is considerably larger than chance level, and in particular, considerably larger than the accuracy obtained in the direct task. This leads back to the old statistical issue of needing to distinguish between the statistical significance of effects vs. the size of the effects.

4 Many studies did not test for the difference of the effects. Isn’t this also a problem?

The correct procedure would indeed be to test for the difference (Franz & Gegenfurtner, 2008; Nieuwenhuis, Forstmann, & Wagenmakers, 2011). This is, however, an issue independent of the general fallacy we are concerned with, so we do not discuss it further.

5 The lie detection study calculated Cohen’s d values. Doesn’t this ameliorate the problem?

While most studies used the RTs in the indirect task for their significance test, the lie detection study (ten Brinke et al., 2014) used a somewhat different approach. For each participant an
individual Cohen’s d value (Cohen, 1988) was computed from the RTs, resulting for the 66 participants in $d_1, \ldots, d_{66}$. Then, a significance test was performed on these values and a second Cohen’s d value $d_{\text{across}}$ was calculated across the individual values $d_1, \ldots, d_{66}$. This value $d_{\text{across}}$ is what is shown in Fig. 2 (Exp. 2) of the lie detection study (ten Brinke et al., 2014), it was found to be $d_{\text{across}} = 0.27$ (ten Brinke et al., 2014, p. 6). This means that the Cohen’s d–values used in this figure refer to the question of whether the means of the RTs are different from each other (which they very well can be, as we explained above). It does not say anything about the effect size of the indirect classification performance, which would be the relevant quantity. The relevant Cohen’s d–values computed on the distribution that is relevant to classification (our Fig. 2A) amount on average to 0.03 (ten Brinke et al., 2014, p. 6), which is very small and therefore fully consistent with the results of our classifiers (for psychophysicists: this is equivalent to a very small d–prime value in signal detection theory).
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Author contributions

The major contribution to this manuscript comes from V.H.F. He discovered the methodological flaws and reanalyzed the data of the lie detection study (ten Brinke et al., 2014). The role of U.v.L. was the one of a critical discussion partner. She verified all arguments and re-implemented the analyses independently in Matlab. Both authors jointly wrote the paper.

Competing Interests

Volker H. Franz received funding from the University of Hamburg and the German Research Foundation. He currently serves in the editorial board of British Journal of Psychology. Ulrike von Luxburg received funding from the University of Hamburg and the German Research Foundation. She currently serves in the editorial board of the Journal of Machine Learning Research and is a board member of the International Machine Learning Society. Previously she served in the editorial board of Statistics and Computing.
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