

Better-than-chance classification for signal detection

Jonathan Rosenblatt Roei Gilron Roy Mukamel

July 28, 2016

Abstract

[TODO]

1 Introduction

A common workflow in genetics or neuroimaging consists of fitting a classifier, and estimating its predictive accuracy using cross validation. Given that the cross validated accuracy is a random quantity, it is then common to test if the cross validated accuracy is significantly better than chance using a permutation test. Examples in the genetics literature include Jiang et al. [2008], Radmacher et al. [2002] [TODO: elaborate]. Example sin the neuroscientific literature include [Golland and Fischl, 2003, Kriegeskorte et al., 2006, Pereira et al., 2009, Varoquaux et al., 2016]. The number of citations¹ of these papers attest to the popularity of the above workflow: 956 for Kriegeskorte et al. [2006], and 274 for Radmacher et al. [2002], as examples.

To fix ideas, we will adhere to a neuroscientific example: In Gilron et al. [2016], the authors seek to detect brain regions which encode differences between vocal and non-vocal stimuli. According to the MVPA analysis workflow, the localization problem is cast as a supervised learning problem: if the type of the stimulus can be predicted from the spatial activation pattern, significantly better than chance, then a region is declared to encode vocal/non-vocal information. We call this an *accuracy test*, a.k.a. *class prediction* in Simon et al. [2003].

This same signal detection task can be also approached as a two-group multivariate test: Inferring that a region encodes vocal/non-vocal information, is essentially inferring that the spatial distribution of brain activations

¹Based on GoogleScholar. Accesses on 26.7.2016.

25 is different given a vocal/non-vocal stimulus. A practitioner may then call
 26 upon a two-group location test such as Hotelling’s T^2 [Fujikoshi et al., 2011].
 27 Alternatively, if the size of the brain region is too large compared to the num-
 28 ber of observations, so that the spatial covariance cannot be fully estimated,
 29 then a high dimensional version of Hotelling’s test can be called upon, such
 30 as in Srivastava [2013] or Schäfer et al. [2005]. In contrast to *accuracy tests*,
 31 we call these *location tests*, a.k.a. *class comparison* in Simon et al. [2003].

32 At this point, it becomes unclear which is the preferred test. The compar-
 33 ison between location and accuracy tests was precisely the topic of Ramdas
 34 et al. [2016], who compared the Hotelling location test to the accuracy of
 35 *Fisher’s linear discriminant analysis* classifier (LDA) [Hastie et al., 2003].
 36 Using an asymptotic analysis, Ramdas et al. [2016] concluded that accuracy
 37 and location tests are equivalent with respect to their order of convergence
 38 to a consistent test, while they may differ in constants. Put differently, the
 39 (asymptotic) relative efficiency of the tests is not trivially 0 nor ∞ .

40 The relative efficiency, governing the power of the tests, may prove crucial
 41 when dealing with the finite sample sizes in neuroscience and genetics, and
 42 thus the focus of this study. We thus seek to study which test is to be
 43 preferred in finite samples? Our conclusion will be quite simple: *location*
 44 *tests almost always have more power than accuracy tests*.

45 The main argument for our statement rests upon the observation that
 46 with typical sample sizes, the accuracy test statistic is highly discrete. Dis-
 47 crete test statistics are known to be conservative [Hemerik and Goeman,
 48 2014], since they cannot exhaust the permissible false positive rate. For accu-
 49 racy tests, the degree of discretization is governed by the number of samples.
 50 In our running neuroscience example [Gilron et al., 2016], the classification
 51 is performed based on 40 trials, so that the test statistic may assume only 40
 52 possible values. This number of examples is not unusual if considering this
 53 is the number of subject in a genetic study, or the number of trial-repeats in
 54 an fMRI brain scan.

55 The discretization effect is aggravated if the test statistic is highly concen-
 56 trated. For an intuition consider the usage of the *train* accuracy test statistic
 57 (i.e., not cross validated). In Section 4 we then address our main question-
 58 which test has more power? Based on the finding that the location test is
 59 typically more powerful, we try to offer an intuition for this phenomenon in
 60 the Discussion section.

61 2 Problem setup

62 Adhering to our neuroscientific example, we now formalize terminology and
63 notation. Let $y \in \mathcal{Y}$ be a class encoding. In our vocal/non-vocal example
64 we have $\mathcal{Y} = \{-1, 1\}$. Let $x \in \mathcal{X}$ be a p dimensional feature vector. In our
65 vocal/non-vocal example p is the number of voxels in a brain region. We
66 thus have $\mathcal{X} = \mathbb{R}^{27}$.

67 Given n pairs of (x_i, y_i) , typically assumed i.i.d., a location test amounts
68 to testing whether $x|y = 1$ has the the same distribution as $x|y = -1$ (or
69 at least the same location). I.e., the multivariate voxel activation pattern
70 has the same distribution when given a vocal stimulus, as when given a non-
71 vocal stimulus. An accuracy test amounts to learning a predictive model $\hat{f}(x)$
72 from some assumed model class $\hat{f} \in \mathcal{F}$. The prediction accuracy, denoted
73 $T_{\hat{f}}^{acc}$, is defined as the probability of a given classifier \hat{f} of making a correct
74 prediction $T_{\hat{f}}^{acc} := Prob(\hat{f}(x) = y)$ when given a new, randomly drawn data
75 point, (x, y) . A statistically significant “better than chance” estimate of $T_{\hat{f}}^{acc}$
76 is evidence that the classes are distinct.

77 2.1 Candidate Tests

78 The design of a permutation test using the prediction accuracy, requires the
79 following design choices:

- 80 1. How to estimate accuracy?
- 81 2. Is the statistic cross validated or not?
- 82 3. For a K-fold cross validated test statistic: should the data be refolded
83 in each permutation?
- 84 4. Permute labels of features?
- 85 5. For a K-fold cross validated test statistic: should the data folding bal-
86 anced? (a.k.a. stratified).
- 87 6. How many folds?

88 We will now address these questions while bearing in mind that unlike the
89 typical supervised learning setup, we are not interested in an unbiased esti-
90 mate of the prediction error, but rather in the mere detection of a difference
91 between two groups, leading to a better-than-chance accuracy.

92 **How to estimate accuracy?** Given a predictor \hat{f} , a natural test statis-
 93 tic is some estimate of its accuracy $T_{\hat{f}}^{acc}$. Complicating matters: very low
 94 accuracies, even 0, is evidence that the classes are separated, and we only
 95 need to invert the predictions. We can thus consider $|T_{\hat{f}}^{acc} - 0.5|$ as the test
 96 statistic. This, however, implies that if the classes are identical, random
 97 guessing has a 0.5 accuracy. This is not true if the classes are not balanced.
 98 The chance level in which case is the prevalence of the dominant class, we
 99 denote by \hat{p}_{max} . This suggests the following test statistic $|T_{\hat{f}}^{acc} - \hat{p}_{max}|$. Since
 100 we will be aggregating these statistic over random data sets where the dom-
 101 inant class may have varying frequencies, it seems appropriate to standard-
 102 ize the scale of this statistic. We thus also consider the z-scored accuracy:
 103 $|T_{\hat{f}}^{acc} - \hat{p}_{max}| / \sqrt{\hat{p}_{max}(1 - \hat{p}_{max})}$.

104 **Cross validate or not?** Were we interested in an unbiased estimator of
 105 the prediction error, there is no question that some independent validation
 106 is in order. Since we are merely interested in detecting a difference between
 107 classes, a biased error estimate is not an issue provided that bias is consistent
 108 over all permutations. The underlying intuition is that if the exact same
 109 computation is performed over all permutations, then a permutation test
 110 will be “fair”, i.e., will not inflate the false positive rate. We will thus be
 111 considering both cross validated accuracies, and *train* accuracies as our test
 112 statistics.

113 **Refolding?** The standard practice in neuroimaging is to refold the data
 114 after each permutation. This is imperative if permuting labels while aiming
 115 at balanced data folds. This is not, however, imperative in general. For
 116 simplicity, we will adhere to the standard practice of refolding the data within
 117 each permutation.

118 **Permute labels of features?** While seemingly identical, the compound-
 119 ing of permutations with data foldings renders these two approaches distinct.
 120 As an example, consider balanced (stratified) K-fold cross validation where
 121 the initial data folding is balanced. After a label permutation, the original
 122 folds will probably not be balanced. If the *features* are permuted, then the
 123 labels conserve their original fold assignments, and the original folds are bal-
 124 anced after each permutation. Since we only report results while refolding the
 125 data in each permutation, then the only difference between permuting labels
 126 and permuting features seems to be a computational one. We thus adhere
 127 to the more common, albeit less efficient practice, of permuting labels.

128 **Balanced folding?** As already implied, a standard practice when cross
 129 validating is to constrain the data folds to be balanced (i.e. stratified). This
 130 is well justified when aiming at unbiased accuracy estimation. This also
 131 simplifies matter when aiming at signal detection, as can be seen from the
 132 above discussion of the appropriate test statistic. On the other hand, it
 133 may complicate matters, as can be seen from the above discussion on label
 134 versus feature permutation. We will report results with both balanced and
 135 unbalanced data foldings, only to discover, it does not really matter.

136 **How many folds?** Different authors suggest different rules for the num-
 137 ber of folds. We will be varying the number of folds. This will affect the
 138 concentration of permutation distribution of the estimated accuracy, which
 139 will have a crucial effect on the conservativeness of the accuracy test. Our
 140 intuition suggests that since more folds imply a less concentrated estimate,
 141 then leave-one-out should be the less conservative, and 2-fold should be the
 142 most conservative.

143 There are indeed many design choices when performing a permutation test
 144 using a cross validated statistic. The subset of tests we will be comparing is
 145 collected for convenience in Table 1.

146 3 Controlling the False Positive Rate

147 We start by verifying that the battery of tests in Table 1 control the false
 148 positive rate at the desired 0.05 level, with varying conservativeness levels.
 149 Figure 1 demonstrates that this is indeed the case. All our candidate tests
 150 control the type I error, with varying degrees of conservativeness. In particu-
 151 lar: (a) if the folds are balanced or not, (b) if the tuning parameters of some
 152 test statistic are varied, (d) if the number of folds is varied.

153 4 Power

154 Having established that all of the tests in our battery control the false pos-
 155 itive rate, it remains to be seen if they have similar power; Especially, when
 156 comparing the power of the location tests to the accuracy tests. The theo-
 157 retical results of Ramdas et al. [2016] suggest that power should be of the
 158 same order. On the other hand, the results of our previous sections suggest
 159 that the conservativeness of some of the considered tests can be considerable,
 160 rendering them underpowered.

161 [TODO: discuss power of various tests after finishing simulations]

Name	Basis	CV	Accuracy	Parameters
Hotelling	Hotelling	–	–	shrink=FALSE
Hotelling.shrink	Hotelling	–	–	shrink=TRUE
lda.CV.1	LDA	TRUE	accuracy	–
lda.CV.2	LDA	TRUE	z-accuracy	–
lda.noCV.1	LDA	FALSE	accuracy	–
lda.noCV.2	LDA	FALSE	z-accuracy	–
sd	SD	–	–	–
svm.CV.1	SVM	TRUE	accuracy	cost=1e1
svm.CV.2	SVM	TRUE	accuracy	cost=1e-1
svm.CV.3	SVM	TRUE	z-accuracy	cost=1e1
svm.CV.4	SVM	TRUE	z-accuracy	cost=1e-1
svm.noCV.1	SVM	FALSE	accuracy	cost=1e1
svm.noCV.2	SVM	FALSE	accuracy	cost=1e-1
svm.noCV.3	SVM	FALSE	z-accuracy	cost=1e1
svm.noCV.4	SVM	FALSE	z-accuracy	cost=1e-1

Table 1: This table enumerates the various test statistics we will be studying. Three are location tests: Hotelling, Hotelling.shrink, and sd. *Hotelling* is the classical two-group T^2 statistic. *Hotelling.shrink* is a high dimensional version with the regularized covariance in Schäfer et al. [2005]. *sd* is another high dimensional version of the T^2 , from Srivastava et al. [2013]. The rest of the tests are variations of the linear SVM, and Fisher’s LDA, with varying accuracy measures, cross validated or not, and varying tuning parameters. For example, *svm.CV.4* is a linear SVM, with *libsvm*’s cost parameter set at 0.1, using the cross validated z-scored accuracy ($|T_{\hat{f}}^{acc} - \hat{p}_{max}|/\sqrt{\hat{p}_{max}(1 - \hat{p}_{max})}$, see Section 2.1). Another example is *lda.noCV.1*, which is Fisher’s LDA, returning the train accuracy, without cross validation, and without z-scoring.

We see by now that the use of accuracy tests for signal detection is underpowered compared to location tests. Simulations alone cannot, however, support such a universal statement. We will thus verify on a neuroimaging dataset, and discuss the causes for this phenomenon with implications on the scope of our statement.

5 Neuroimaging Example

Figure 2 is an application of our battery of tests to the data of Pernet et al. [2015]. The authors of Pernet et al. [2015] collected fMRI data while subjects were exposed to the sounds of human speech (vocal), and other non-vocal sounds. Each subject was exposed to 20 sounds of each type, totalling in $n = 40$ trials in each scan. The study was rather large and consisted of

Figure 1: The power of a permutation test with various test statistics. The power on the x axis. Effect are color and shape coded. They are assumed to be equal in all the 23 dimensions, and vary over 0 (red circle), 0.25 (green triangle), and 0.5 (blue square). The various statistics on the y axis. Their details are given in Table 1. Simulation code available at [TODO].



about 200 subjects. The data was kindly made available by the authors at the OpenfMRI website².

To verify the observation that location tests have more power than accuracy tests, we perform permutation inference using the pipeline of Stelzer et al. [2013], which was also used in Gilron et al. [2016]. For completeness, the pipeline is described in Appendix A. To demonstrate our point, we compare the *sd* location test with the *svm.cv.1* accuracy test (see Table 1 for the definition of these statistics).

In agreement with our simulation results, the location test (*sd*) discovers more brain regions that encode information discriminating between vocal and non-vocal stimuli when compared to an accuracy test (*svm.cv.1*). The former discovers 1,232 regions, while the latter only 441, 399 of which are common to both as reported in Figure 2. We emphasize that both test statistics were compared with the same permutation scheme, and the same error controls, so that any difference in detections is due to their different power.

Having established that accuracy tests are underpowered both in simulation and in application, we wish to identify the conditions under which this will occur, and discuss implications on the practice of accuracy tests.

²<https://openfmri.org/>

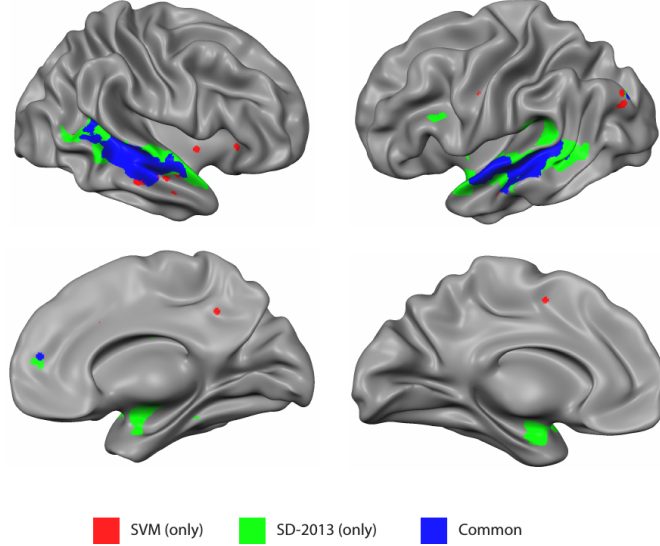


Figure 2: Brain regions encoding information discriminating between vocal and non-vocal stimuli. Map reports the centres of 27-voxel sized spherical regions, as discovered by an accuracy test (*svm.cv.1*), and a location test (*sd*). *svm.cv.1* was computed using 5-fold cross validation, and a cost parameter of 1. Region-wise significance was determined using the permutation scheme of Stelzer et al. [2013], followed by region-wise $FDR \leq 0.05$ control using the Benjamini-Hochberg procedure [Benjamini and Hochberg, 1995]. Number of permutations equals 400. The location test detect 1,232 regions, and the accuracy test 441. The overlap is such that 90% of the accuracy test regions, are also detected by the location test. For the details of the analysis see Appendix A and Gilron et al. [2016].

6 Discussion

We have set out to understand which of the tests is more powerful: the accuracy test or the location test. Using simulations, we have concluded that the location tests are preferable. We attribute this to the discretization introduced in finite samples by the accuracy test statistic. This also explains why an asymptotic analysis, such as Ramdas et al. [2016], did not find a qualitative difference.

At this point some reservations to the generality of our findings are in order. Firstly, not all accuracy tests are concerned with signal detection. Indeed, it is possible that the purpose of the test is not to detect a difference between classes, but to actually test if a particular classifier is better than chance. This would be the case, for instance, with brain-machine interfaces, where the detection of a signal is not enough [Olivetti et al., 2012]. In such cases, the performance of a particular classifier is the object of study,

205 rendering the accuracy test the appropriate choice.

206 Secondly, there may be cases where the accuracy test does have more
207 power than the location test. Our simulations were unable to point out such
208 a scenario, but the fact that in our neuroimaging example (Section 5) some
209 brain regions were detected with the accuracy test, and not the location test,
210 suggest that the accuracy test does have more power for particular types of
211 signal. [TODO: signal in scale? heavy tails?]

212 A very important point is the ease of implementation. The need for cross
213 validation of the accuracy test greatly increases its computational complexity.
214 Moreover, anyone who has actually implemented tests with discrete statistics,
215 will attest they are considerably harder to implement. This is because their
216 unforgiveness to the type of inequality. Indeed, replacing a weak inequality
217 with a strong inequality may considerably change the results. This is not the
218 case for continuous test statistics.

219 Given all the above, we find the popularity of accuracy tests quite puz-
220 zling. We believe this is due to a reversal of the inference cascade. Re-
221 searchers first fit a classifier, and then ask if the classes are any different.
222 Were they to start by asking if classes are any different, and only then try
223 to classify, then location tests would naturally arise as the preferred method.
224 As put by Ramdas et al. [2016]:

225 The recent popularity of machine learning has resulted in the ex-
226 tensive teaching and use of prediction in theoretical and applied
227 communities and the relative lack of awareness or popularity of
228 the topic of Neyman-Pearson style hypothesis testing in the com-
229 puter science and related “data science” communities.

230 References

- 231 Y. Benjamini and Y. Hochberg. Controlling the false discovery rate: a prac-
232 tical and powerful approach to multiple testing. *JOURNAL-ROYAL STA-*
233 *ISTICAL SOCIETY SERIES B*, 57:289–289, 1995.
- 234 Y. Fujikoshi, V. V. Ulyanov, and R. Shimizu. *Multivariate Statistics: High-*
235 *Dimensional and Large-Sample Approximations*. John Wiley & Sons, Aug.
236 2011. ISBN 978-0-470-53986-6.
- 237 R. Gilron, J. Rosenblatt, O. Koyejo, R. A. Poldrack, and R. Mukamel. Quan-
238 tifying spatial pattern similarity in multivariate analysis using functional
239 anisotropy. *arXiv:1605.03482 [q-bio]*, May 2016.

- 240 P. Golland and B. Fischl. Permutation tests for classification: towards statis-
241 tical significance in image-based studies. In *IPMI*, volume 3, pages 330–341.
242 Springer, 2003.
- 243 T. Hastie, R. Tibshirani, and J. Friedman. *The Elements of Statistical Learn-*
244 *ing*. Springer, July 2003. ISBN 0-387-95284-5.
- 245 J. Hemerik and J. Goeman. Exact testing with random permutations.
246 *arXiv:1411.7565 [math, stat]*, Nov. 2014.
- 247 W. Jiang, S. Varma, and R. Simon. Calculating confidence intervals for
248 prediction error in microarray classification using resampling. *Statistical*
249 *Applications in Genetics and Molecular Biology*, 7(1), 2008.
- 250 N. Kriegeskorte, R. Goebel, and P. Bandettini. Information-based functional
251 brain mapping. *Proceedings of the National Academy of Sciences of the*
252 *United States of America*, 103(10):3863–3868, July 2006. ISSN 0027-8424,
253 1091-6490. doi: 10.1073/pnas.0600244103.
- 254 E. Olivetti, S. Greiner, and P. Avesani. Induction in Neuroscience with
255 Classification: Issues and Solutions. In G. Langs, I. Rish, M. Grosse-
256 Wentrup, and B. Murphy, editors, *Machine Learning and Interpretation*
257 *in Neuroimaging*, number 7263 in Lecture Notes in Computer Science,
258 pages 42–50. Springer Berlin Heidelberg, 2012. ISBN 978-3-642-34712-2
259 978-3-642-34713-9. doi: 10.1007/978-3-642-34713-9_6.
- 260 F. Pereira, T. Mitchell, and M. Botvinick. Machine learning classifiers and
261 fMRI: A tutorial overview. *NeuroImage*, 45(1, Supplement 1):S199–S209,
262 Mar. 2009. ISSN 1053-8119. doi: 10.1016/j.neuroimage.2008.11.007.
- 263 C. R. Pernet, P. McAleer, M. Latinus, K. J. Gorgolewski, I. Charest, P. E. G.
264 Bestelmeyer, R. H. Watson, D. Fleming, F. Crabbe, M. Valdes-Sosa, and
265 P. Belin. The human voice areas: Spatial organization and inter-individual
266 variability in temporal and extra-temporal cortices. *NeuroImage*, 119:164–
267 174, Oct. 2015. ISSN 1053-8119. doi: 10.1016/j.neuroimage.2015.06.050.
- 268 M. D. Radmacher, L. M. McShane, and R. Simon. A Paradigm for
269 Class Prediction Using Gene Expression Profiles. *Journal of Computa-*
270 *tional Biology*, 9(3):505–511, June 2002. ISSN 1066-5277. doi: 10.1089/
271 106652702760138592.
- 272 A. Ramdas, A. Singh, and L. Wasserman. Classification Accuracy as a Proxy
273 for Two Sample Testing. *arXiv:1602.02210 [cs, math, stat]*, Feb. 2016.

- 274 J. Schäfer, K. Strimmer, and others. A shrinkage approach to large-scale co-
275 variance matrix estimation and implications for functional genomics. *Sta-*
276 *tistical applications in genetics and molecular biology*, 4(1):32, 2005.
- 277 R. Simon, M. D. Radmacher, K. Dobbin, and L. M. McShane. Pitfalls in the
278 Use of DNA Microarray Data for Diagnostic and Prognostic Classification.
279 *Journal of the National Cancer Institute*, 95(1):14–18, Jan. 2003. ISSN
280 0027-8874, 1460-2105. doi: 10.1093/jnci/95.1.14.
- 281 M. S. Srivastava. On testing the equality of mean vectors in high dimension.
282 *Acta et Commentationes Universitatis Tartuensis de Mathematica*, 17(1):
283 31–56, June 2013. ISSN 2228-4699. doi: 10.12697/ACUTM.2013.17.03.
- 284 M. S. Srivastava, S. Katayama, and Y. Kano. A two sample test in high
285 dimensional data. *Journal of Multivariate Analysis*, 114:349–358, Feb.
286 2013. ISSN 0047-259X. doi: 10.1016/j.jmva.2012.08.014.
- 287 J. Stelzer, Y. Chen, and R. Turner. Statistical inference and multiple test-
288 ing correction in classification-based multi-voxel pattern analysis (MVPA):
289 Random permutations and cluster size control. *NeuroImage*, 65:69–82, Jan.
290 2013. ISSN 1053-8119. doi: 10.1016/j.neuroimage.2012.09.063.
- 291 G. Varoquaux, P. R. Raamana, D. Engemann, A. Hoyos-Idrobo, Y. Schwartz,
292 and B. Thirion. Assessing and tuning brain decoders: cross-validation,
293 caveats, and guidelines. working paper or preprint, June 2016.

294 A Analysis pipeline

295 Here is the analysis pipeline of Stelzer et al. [2013] we for the auditory data in
 296 Gilron et al. [2016]. Denoting by $i = 1, \dots, I$ the subject index, $v = 1, \dots, V$
 297 the voxel index, and $s = 1, \dots, S$ the permutation index. Since regions³ are
 298 centred around a unique voxel, the voxel index v also serves as a unique
 299 region index. Algorithm 1 computes a region-wise test statistic, which is
 300 compared to its permutation null distribution computed by Algorithm 2.

Algorithm 1: Compute a group parametric map.

Data: fMRI scans, and experimental design.
Result: Brain map of group statistics: $\{\bar{T}_v\}_{v=1}^V$

```

1 for  $v \in 1, \dots, V$  do
2   for  $i \in 1, \dots, I$  do
3      $T_{i,v} \leftarrow$  test statistic for subject  $i$  in a region centered at  $v$ .
4    $\bar{T}_v \leftarrow \frac{1}{I} \sum_{i=1}^I T_{i,v}$ .
```

Algorithm 2: Compute a permutation p-value map.

Data: fMRI scans of 20 subjects, experimental design.
Result: Brain map of permutation p-values: $\{p_v\}_{v=1}^V$

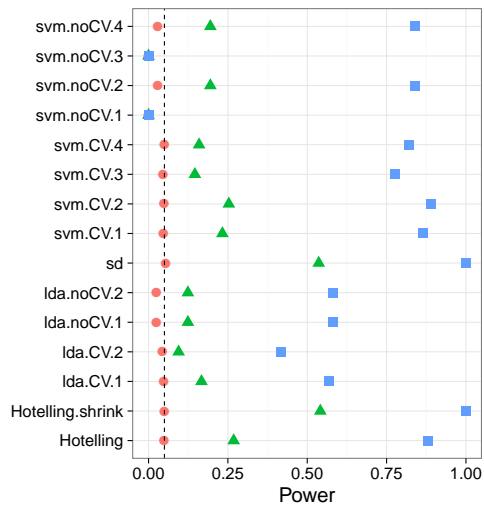
```

1 for  $s \in 1, \dots, S$  do
2   permute labels;
3    $\bar{T}_v^s \leftarrow$  parametric map
```

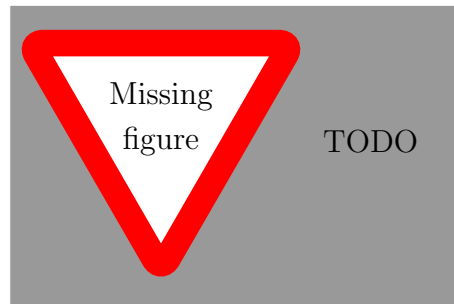
³*searchlight* or *sphere* in the MVPA parlance

B More Simulations

Figure 3: [TODO].



(a) 2 Folds.

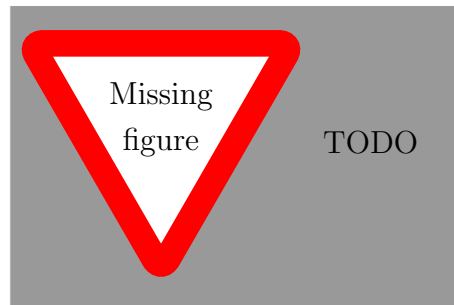


(b) 20 Folds.

Figure 4: [TODO].

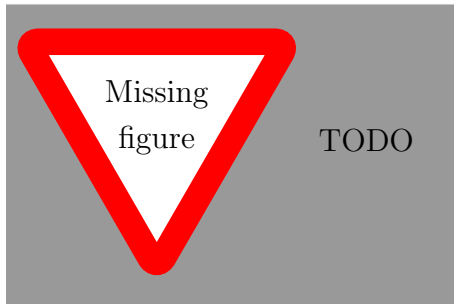


(a) Scale Change.

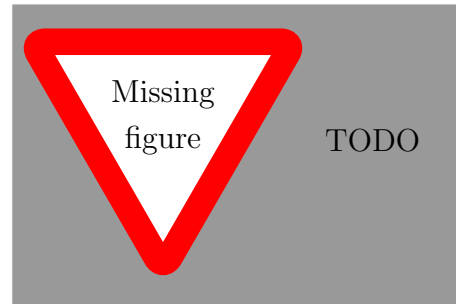


(b) t Null

Figure 5: [TODO].

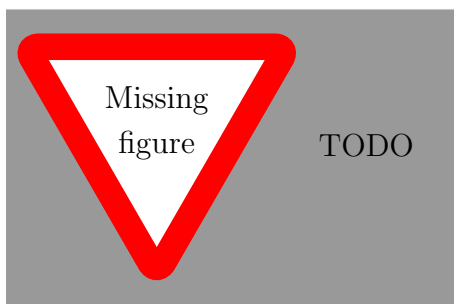


(a) Compound symmetry

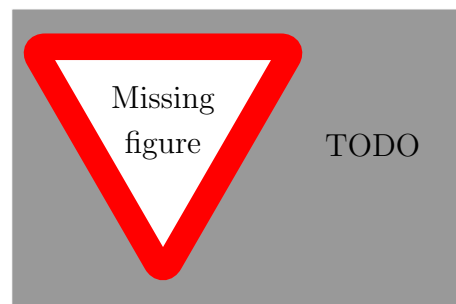


(b) AR(1)

Figure 6: [TODO].



(a) $n=400$



(b) ?