

Outline

Selected Topics in Statistics for fMRI Data Analysis

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- I. Adjusting for Multiple Comparisons
- II. Modelling Data from Multiple Subjects
- III. Some Thoughts on Model Validation

I. Multiple Comparisons

- la. Bonferroni Approximation
- lb. Gaussian Random Field Assumption
- lc. False Discovery Rate

A Hypothetical Hypothesis Test

Consider a hypothesis test for which you obtain the t -statistic

$$T = 4.62,$$

with 50 degrees of freedom. The corresponding p -value is

$$1 - \Pr(-4.62 \leq T_{50} \leq 4.62) = 0.000027.$$

Is this necessarily cause for celebration?

The Rest of the Story ...

- The t -statistic on the previous slide was obtained by choosing the maximum of $64 \times 64 \times 16 = 65,536$ random draws from the *null distribution* of the test statistic (i.e., the T_{50} distribution).
- So one might typically expect to see a t -statistic this large or larger in a typical fMRI volume, even if what you're imaging is a bottle of water.
- We need to adjust p -values for the number of tests performed, a process which statisticians call *adjusting for multiple comparisons*.

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An Illustrative Example (Model)

- In order to illustrate many of the basic ideas, it is sufficient to consider an example of confidence intervals (or hypothesis tests) on just two parameters.
- Consider the simple linear regression model

$$y_i = \delta + \beta(x_i - \bar{x}) + e_i,$$

where $x_i = 0, 10, 20, \dots, 100$, $\delta = 0$, $\beta = 1$, and the $e_i \sim N(0, 10^2)$.

- (Aside: Note that the vectors $[1, 1, \dots, 1]^T$ and $[x_1 - \bar{x}, x_2 - \bar{x}, \dots, x_n - \bar{x}]^T$ are orthogonal.)

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Illustrative Example (Hypothesis)

- We are interested in testing, at the $\alpha = 0.05$ level, the null hypothesis

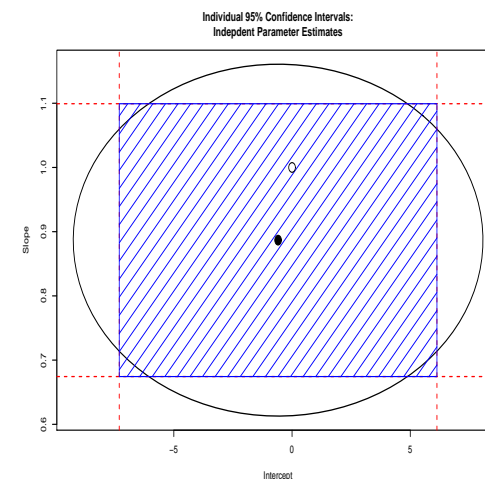
$$H_0 : \delta = 0 \text{ and } \beta = 1,$$

against the alternative

$$H_1 : \delta \neq 0 \text{ or } \beta \neq 1,$$

- A joint 95% confidence region for (δ, β) would provide a critical region for this test.

Confidence Region



Comments

- The box formed by the two individual confidence intervals is considerably smaller than the actual bivariate confidence region.
- Each confidence interval for a parameter has confidence level 0.95, so the region formed by the intersection of these intervals has confidence $0.95^2 = 0.9025 < 0.95$.

Comments (Cont'd)

- Over repeated future data, the probability that either parameter falls in it's interval is $1 - \alpha_* = 0.95$. Since the model has been set up so the the estimates $(\hat{\delta}, \hat{\beta})$ are independent, the actual probability of rejecting H_0 for the pair of confidence intervals

$$\alpha = \Pr(|T_1| \geq t_1 \text{ or } |T_2| \geq t_2) = 1 - (1 - \alpha_*)^2 = 1 - (1 - 0.05)^2 = 0.0975.$$

Comments (Cont'd)

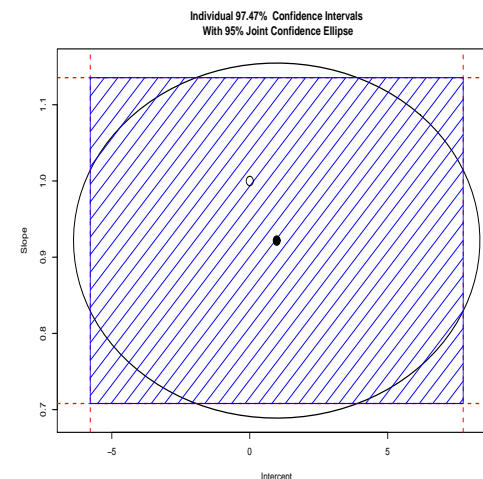
- Working backwards, if we choose α_* to be

$$\alpha_* = 1 - \sqrt{1 - \alpha} \approx \alpha/2,$$

then we will achieve our goal of an overall significance level of α .

- This is approach achieves exactly the desired significance if the test statistics are *independent*, but is conservative if the test statistics are *dependent*.

Bonferroni Intervals with 95% Confidence Ellipse



Bonferroni Correction

- **The Setup:** We have k independent test statistics T_1, \dots, T_k , corresponding to parameters β_1, \dots, β_k , respectively.
- For each test statistic, we reject the null hypothesis $H_i : \beta_i = 0$ when $|T_i| \geq t_i$, for constants t_1, \dots, t_k .
- We would like to calculate the probability of rejecting the null hypothesis

$$H_0 : \beta_1 = \beta_2 = \dots = \beta_k = 0$$

against the alternative that H_0 is not true.

Example Revisited: Alternative Parameterization

- Next we see what happens in our simple linear regression example if we don't subtract of the mean of the x s:

$$y_i = \tilde{\delta} + \beta x_i + e_i,$$

where $x_i = 0, 10, 20, \dots, 100$, $\delta = 0$, $\beta = 1$, and the $e_i \sim N(0, 10^2)$. To relate this to the previous parameterization, note that

$$\tilde{\delta} = \delta - \bar{x}.$$

- **(Aside:** Note that the vectors $[1, 1, \dots, 1]^T$ and $[x_1, x_2, \dots, x_n]^T$ *not* orthogonal! Consequently, the t -tests for $\tilde{\delta}$ and β will not be independent.)

Bonferroni Correction (Cont'd)

- This probability of rejecting H_0 is

$$\begin{aligned}\alpha &= P_0(|T_1| \geq t_1 \text{ or } |T_2| \geq t_2 \text{ or } \dots |T_k| \geq t_k) \\ &= 1 - \prod_{i=1}^k \Pr(|T_i| \geq t_i) = 1 - (1 - \alpha_*)^k.\end{aligned}$$

- Hence, we choose

$$\alpha_* = 1 - (1 - \alpha)^{(1/k)} \approx 1 - (1 - \alpha/k) = \alpha/k.$$

Alternative Parametrization (Cont'd)

- We are interested in testing the null hypothesis

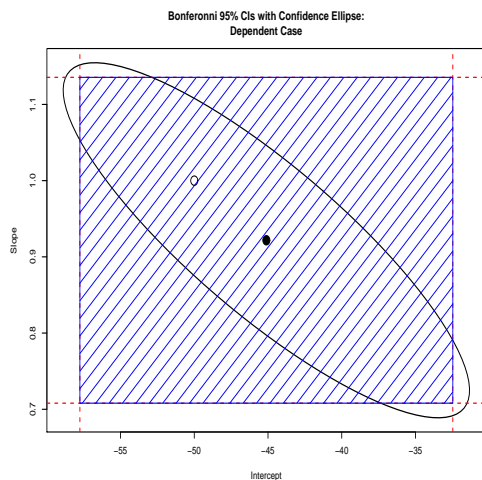
$$H_0 : \tilde{\delta} = -\bar{x} \text{ and } \beta = 1,$$

against the alternative

$$H_1 : \tilde{\delta} \neq -\bar{x} \text{ or } \beta \neq 1,$$

at the 0.05 significance level.

- A joint 95% confidence region for $(\tilde{\delta}, \beta)$ would provide a critical region for this test.



- In addition to requiring that p values be below a threshold, one can impose as an additional requirement that there be a minimum number of voxels *clustered* at any “active” location.
- There are obviously many ways to pair critical p -values with minimum cluster sizes.
- There is a stand-alone *C* program, AlphaSim that can determine cluster significance levels by simulation.
- AlphaSim is part of the AFNI distribution (Bob Cox, NIH, afni.nimh.nih.gov)

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Example AlphaSim Command Line

- A typical run of AlphaSim:

```
AlphaSim -nx 46 -ny 55 -nz 46 \
  -dx 4.0 -dy 4.0 -dz 4.0 \
  -sigma 0.65 \
  -rmm 6.93 \
  -pthr 0.05 -iter 10000
```

AlphaSim Command Line (Cont'd)

- `-nx -ny -nz`: Dimension of brain in voxels
- `-dx -dy -dz`: Voxel size in *mm*.
- `-sigma`: SD of Gaussian smoothing kernel
- `-rmm`: Two active voxels \leq *rmm mm* apart are considered to be in the same cluster.
- `-pthr`: Threshold p -value
- `-iter`: Number of simulations.
- (See AlphaSim documentation for other options.)

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Example AlphaSim Output

Data set dimensions:

nx = 46 ny = 55 nz = 46 (voxels)
dx = 4.00 dy = 4.00 dz = 4.00 (mm)

Gaussian filter widths:

sigmax = 0.65 FWHMx = 1.53
sigmay = 0.65 FWHMy = 1.53
sigmaz = 0.65 FWHMz = 1.53

Cluster connection radius: rmm = 6.93

Threshold probability: pthr = 5.000000e-02

Number of Monte Carlo iterations = 10000

Example AlphaSim Output (Cont'd)

Cl Size	Frequency	Max Freq	Alpha
1	15616950	0	1.000000
2	5123184	0	1.000000
3	2397672	0	1.000000
4	1320445	0	1.000000
38	228	210	0.113100
39	190	175	0.092100
40	140	134	0.074600
41	114	108	0.061200
42	91	87	0.050400
43	60	57	0.041700

Interpretation of AlphaSim Results

- Maximum active clusters of 42 or more below threshold $p = 0.05$ occur about 5% of the time under the null hypothesis of no activation.
- Note the following:
 - For a higher p -value threshold, the minimum significant cluster size will be larger.
 - This approach accounts for spatial correlation induced by smoothing, but not for and spatial correlation present in the unsmoothed data.

Summary: Bonferroni

- For an overall test at the α significance level, select individual voxels among N total as active if $p \leq \alpha/N$.
- Not a bad approximation if voxels are nearly independent.
- Can be *very* conservative if there is considerable spatial correlation among voxels.
- Using both a p -value threshold *and* a minimum cluster size via AlphaSim is one way to partially overcome this conservatism.

Gaussian Random Field

- A Gaussian random field is a stationary Gaussian stochastic process, usually in 2 or 3 dimensions.
- The one-dimensional case of GRF is Brownian motion (formally, a *Weiner process*).
- Unsmoothed BOLD activation is not well approximated as a GRF, so spatial smoothing is generally done if one is to use GRF theory.
- Smoothing is averaging, and averages of (almost) arbitrary random variables are approximately Gaussian. This is the essence of the *Central Limit Theorem*.

Expected EC

- By definition,

$$E(\text{EC}) = \sum_k k \Pr(\text{EC} = k)$$

- For high thresholds, the probability of more than one blob under H_0 is negligible, and we have

$$E(\text{EC}) \approx \Pr(\text{EC} = 1)$$

- For large u , $E(\text{EC})$ will approximate

$$E(\text{EC}) \approx \Pr(\max_i T_i > u).$$

Euler Characteristic

- If one thresholds a continuous GRF, the the *Euler Characteristic* is

$$\text{EC} = (\# \text{ Blobs}) - (\# \text{ Holes}),$$

- if the threshold is sufficiently high, then this will essentially become the (# Blobs).
- If the threshold is higher still, then the EC will likely be zero or 1.
- If we threshold high enough, then we might be able to assume, at an appropriate significance level, that all blobs are due to activation.

Expected EC (Cont'd)

$$E(\text{EC}) \approx \Pr(\max_i T_i > u).$$

- Either
 - Attempt to approximate this expectation for a choice of u (adjusted p -value), or
 - Select u so that $E(\text{EC})$ equals, say, 0.05 (adjusted hypothesis test).

Corrected p -Values via $E(EC)$

- We can obtain p -values by using

$$\begin{aligned}\Pr(\max_i T_i > u) &\approx E(EC_u) \\ &= \frac{R(u^2 - 1)e^{-u^2/2}}{4\pi^2(2\log(2))^{3/2}}\end{aligned}$$

- Where R is the number of *Resolution Elements*, defined to be a unit search volume, in terms of the full width at half maximum (FWHM) of the kernel used for spatial smoothing.
- (So *now* you know why SPM requires that you do spatial smoothing!)

Resolution Elements

$$R = \frac{S}{f_x f_y f_z},$$

where

- S is the search volume, in mm^3 ,
- and f_x, f_y, f_z are the FWHMs of the Gaussian spatial kernel in each coordinate direction, in mm .

Summary: Gaussian Random Fields

- GRF theory requires that we know the spatial correlation, at least approximately.
- In order to meet this requirement, we must do fairly hefty spatial smoothing (i.e., precoloring).
- This has the obvious disadvantage of blurring together brain structures with different functions, particularly if the smoothing is not done on the cortical surface.
- Compare with AlphaSim, another way for accounting for spatial correlation due to smoothing.

False Discovery Rate

- The Bonferroni and GRF approaches ensure that the probability of *incorrectly* declaring *any* voxel active is small. If any voxels “survive,” one can reasonably expect that *each one* is truly active.
- An alternative approach is to keep the *proportion* of voxels incorrectly declared active small. Among those voxels declared active, a predetermined proportion (e.g., 0.05), *on average*, will be declared active in error (“false discoveries”).

Implementing FDR

- Order the N p -values from smallest to largest:

$$p_{(1)} \leq p_{(2)} \leq \dots \leq p_{(N)}.$$

- Declare as active voxels corresponding to ordered p -values for which

$$p_{(i)} \leq qci/N,$$

where q is the selected FDR.

- The choice of c depends on the assumed correlation structure for the test statistics.

Values for c

- Two choices for c have been suggested in the literature
- For independent tests, or tests based on data for which the noise is Gaussian with non-negative correlation across voxels, use $c = 1$.
- For arbitrary correlation structure in the noise, use $c = 1/(\log(N) + \gamma)$, where $\gamma \doteq 0.577$ is Euler's constant.

A Simulated Example

- Number of Voxels:**

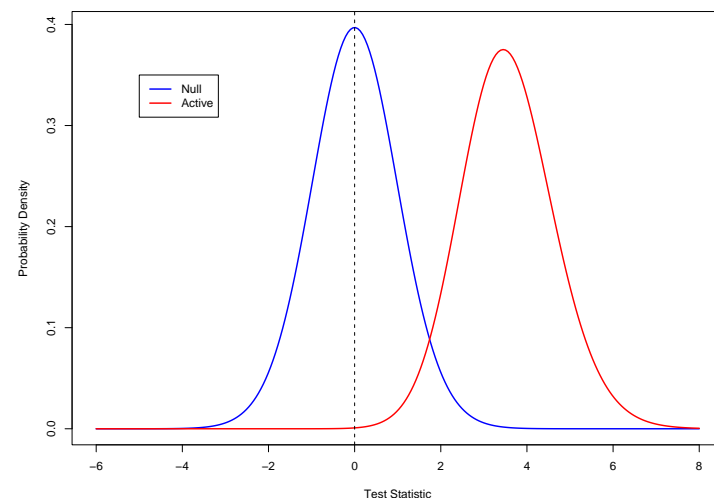
$$N = 64 \times 64 \times 16 = 65,536$$

- Number of Active Voxels:**

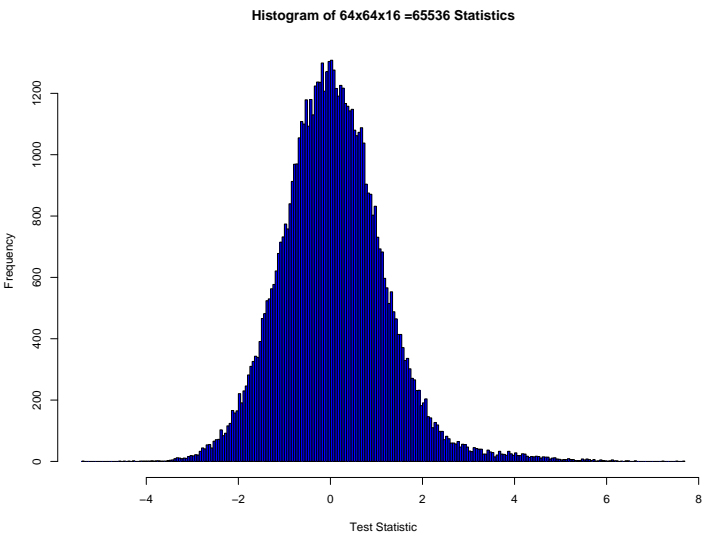
$$N_1 = 0.02N = 1,335$$

- "Inactive" statistics independently distributed t_{50} .
- "Active" statistics independently distributed *noncentral-t*, $t_{50}(\delta)$, where $\delta = 3.5$.

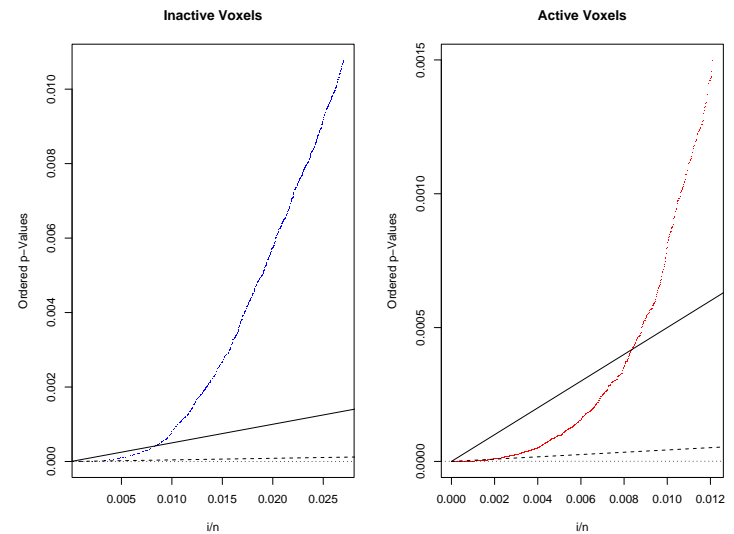
Densities for Active and Inactive Voxel Statistics



Histogram of the Voxel Statistics



Graphical Illustration of Results



Simulation Results

● FDR = $35/549 \doteq 0.064$, $c = 1$:
(Solid line in preceding figure)

	Discovered	
	Yes	No
Correct	514	64,166
Error	35	821
Total	549	64,987

Simulation Results

● FDR = $1/123 \doteq 0.008$, $c = 1/(\log(N) + \gamma)$:
(Broken line in preceding figure)

	Discovered	
	Yes	No
Correct	122	64,200
Error	1	1213
Total	123	65,413

Simulation Results

- Bonferroni ($FDR = 0$), $p = .05/N = 7.6 \times 10^{-7}$
(Not shown in preceding figure)

	Discovered	
	Yes	No
Correct	44	64,201
Error	0	1291
Total	44	65,492

Summary: False Discovery Rate

- Can be more sensitive at detecting true activation than Bonferroni without requiring the heavy spatial smoothing of GRF theory.
- But a change in philosophy is required: instead of making the likelihood of *any* voxel being falsely declared active small, one is willing to accept that a small proportion of voxels will *likely* be false discoveries, and instead attempt to control the size of this proportion.

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II. Analyses for Groups of Subjects

IIa. Fixed Effects

- Analysis on average maps.

IIb. Random Effects

- Usual two-stage approach
- Worsley et al. (*NeuroImage*, 2002)
- A Bayesian approach

IIc. Examples of Bayesian Two-Stage Random Effects Modelling

- Spatial visual cueing
- Passive viewing of angry faces

IId. Conjunction Analysis

Group Analyses

- We next consider approaches to data analyses which involve more than one subject.
- The first difficulty that one has to address in these situations is warping each subjects data onto a common template, such as Talaraich coordinates.
- This process can easily introduce and difficulties and distortions of its own, but these are beyond the scope of the present discussion.

Fixed Effects Analyses

- It is conceivable that one might want to make inference for only the subjects at hand, without any desire to extrapolate to a larger population.
- This might be the case for clinical applications of fMRI, for example, where the objective is to understand the subjects – patients – who are being studied or treated.
- *Fixed effects* models should be used in such cases.
- But since fMRI is presently a research tool, fixed effects analyses are usually less appropriate than *random effects* analyses, in which one is concerned with inferences valid for a population, or equivalently, for the “next” subject which one might obtain.

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Fixed vs. Random Effects

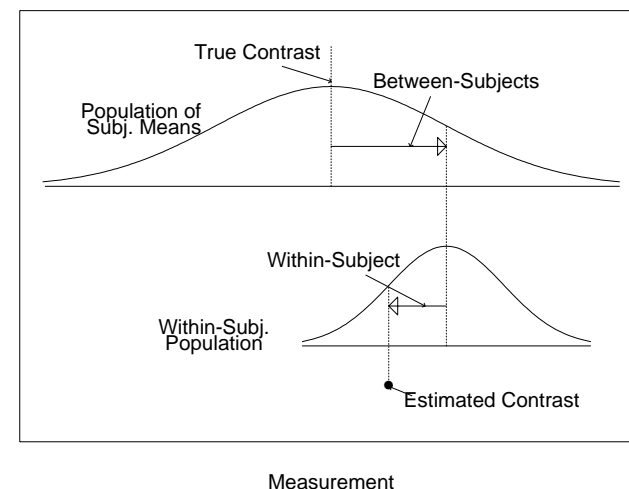
- Assume that several machines are used in a production environment. To fix ideas, let's say these machines are for DNA sequencing.
- If I have several of these machines in my lab, I would presumably be interested in quantifying the relative performance of each of them. **Fixed effects** models would be appropriate.
- On the other hand, if I owned the company that makes the machines, then I'd want to characterize the performance of *any one* of the machines, conceptually *drawn at random*. The machines would then constitute a population, and I'd use **random effects** analyses.

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The Random-Effects Idea

- A contrast at any given voxel is regarded as a sum of three components:
 1. The true (but unknown) contrast
 2. A random shift from the truth which depends **only** on the subject.
 3. A second random shift from the truth due to measurement uncertainty **within** a subject.
- In the limit of many subjects, (2) can be made arbitrarily small; in the limit of long scans, (3) can be made arbitrarily small (except perhaps for a measurement bias).

The Random-Effects Idea: Schematic

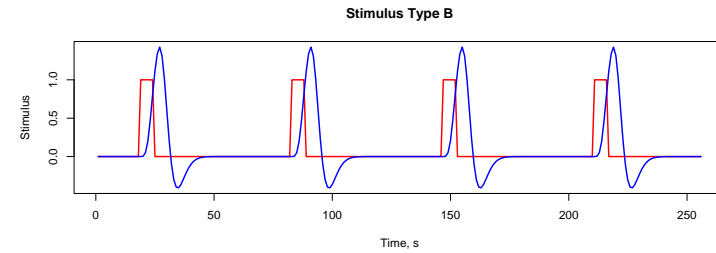
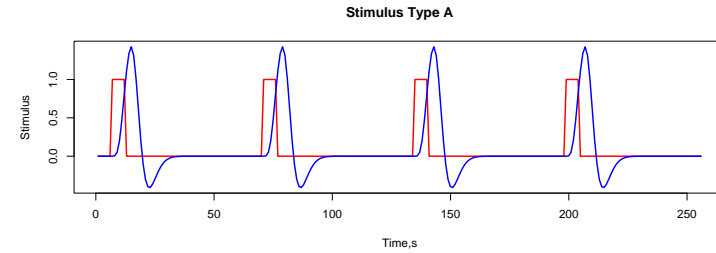


Schematic of Components of Observed Activation

An observed signal at a voxel, in principle, can be constructed as follows:

- Start with experimental paradigm
- Convolve with hemodynamic response
- Add nuisance trend (different for each subject)
- Add within-subject noise, probably with some temporal correlation
- Add random subject effect
 - Same for each voxel
 - Different for each subject
 - Averaging to zero over many subjects

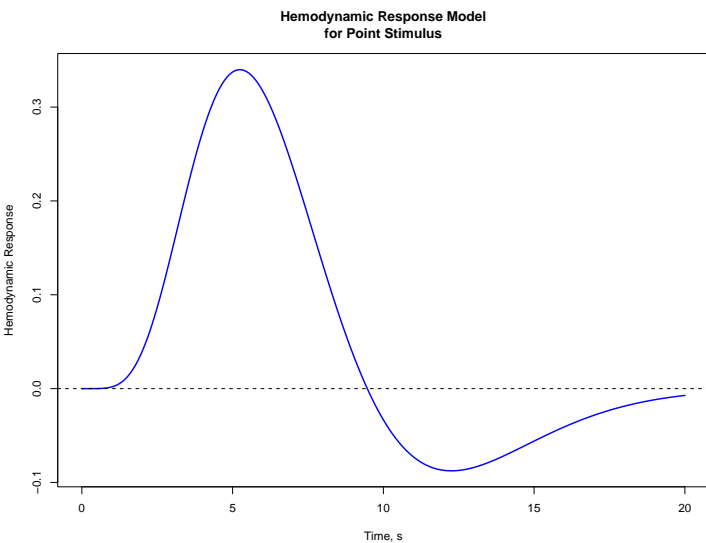
An Example Design With Two Stimulus Types



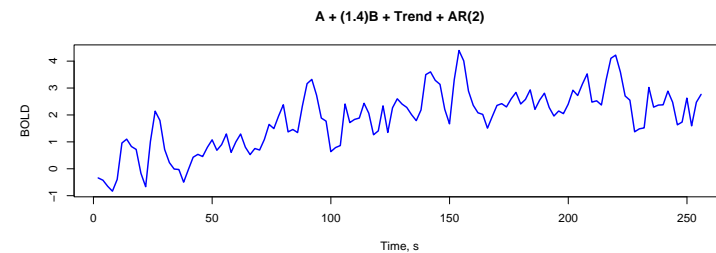
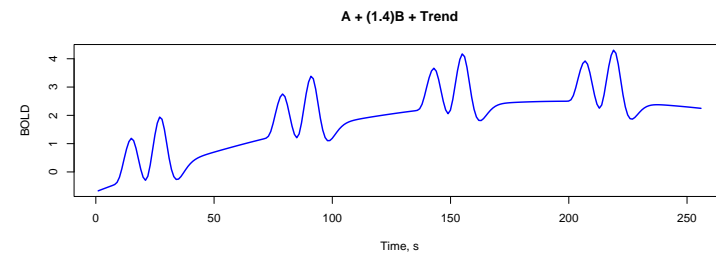
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HDR Used by Worsley et al. (2002) and SPM99



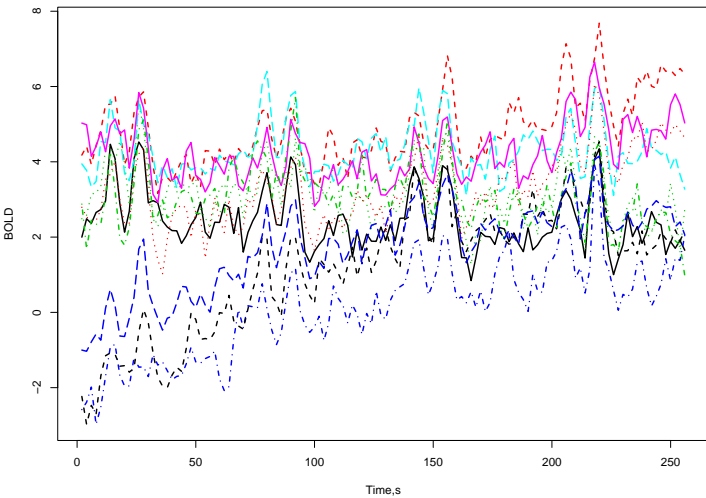
Adding Polynomial Trend and AR(2) Noise



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Simulated Voxel Timecourses: 10 Subjects

Simulated Timecourses from 10 Subjects:
No Between-Subject Variability



Fixed Effects Approach

- Stack together all the activation data from all the subjects (conceptually, at least) into one vector Y .
- Build a design matrix for all the data, with additional coefficients for the *group effect*. If the data are *subject-separable*, then the big X matrix will be block diagonal for within subject factors.
- Schematically (for two subjects):

$$\begin{bmatrix} Y_1 \\ Y_2 \end{bmatrix} = \begin{bmatrix} X_1 & 0 & 0 \\ 0 & X_2 & 1 \end{bmatrix} \begin{bmatrix} \beta_1 \\ \beta_2 \\ \delta \end{bmatrix} + e$$

Two Approaches to Data Analysis

- Fixed-Effects Analysis:** Average data over subjects, look at p -values for contrast on average map. (Degrees of freedom \approx number of **time points** in scan.)
- Random-Effects Analysis:** Estimate contrast map for each subject. Use these maps as “data” for a second-level analysis. (Degrees of freedom \approx number of **subjects**.)

Fixed Effects Approach (Cont'd)

- The coefficient δ models the shift in mean activation. of subject 2 with respect to subject 1. (Why only one δ , though we have 2 subjects?)
- If $\beta_1 \neq \beta_2$, then we have an interaction between effect and subject. Hence, one hypothesis to test is

$$H_0 : \beta_1 = \beta_2$$

- If $\delta = 0$ then we don't have a significant subject effect; so another useful test is

$$H_0 : \delta = 0$$

Random Effects: Two-Stage Analyses

- The simple (and most common) way to do a random-effects group analysis is by a *two-stage* method. This requires the following assumptions:
 - **Balance:** The design matrix is the same for each subject.
 - **Equal Variance:** The error variances are the same for each subject.
 - **Separability:** The GLM model used must be subject-separable; i.e., the combined design matrix for all subjects is block diagonal, with one block for each subject.

A (Very) Simple Example

- To better understand the distinction between fixed- and random-effects, and to see how a two-stage analysis might work for the random case, it helps to consider the simplest possible model: One-way ANOVA, for a single voxel.
- Fixed Effects Model:

$$y_{ij} = \mu + \beta_i + \epsilon_{ij}$$

where $i = 1, \dots, s$ indexes subjects, $j = 1, \dots, n$ indexes acquisitions within a subject. The noise term, ϵ_{ij} is assumed independent $N(0, \sigma^2)$.

Two-Stage Analyses: Assumptions

- The **subject separability** assumption is probably essential.
- We will see later that the **balance** and **equal variance** assumptions can be relaxed if one is willing to employ **standard error** maps, in addition to contrast maps, for each subject.

A (Very) Simple Example (Cont'd)

- Random Effects Model:

$$y_{ij} = \mu + b_i + \epsilon_{ij}$$

where $i = 1, \dots, s$ indexes subjects, $j = 1, \dots, n$ indexes acquisition times within a subject. The noise term, ϵ_{ij} is assumed independent $N(0, \sigma^2)$. The subject effects b_i are independent $N(0, \sigma_b^2)$.

Fixed-Effects Estimates: One-Way ANOVA

- Grand Mean:

$$\hat{\mu} = \bar{y}_{..} \equiv \sum_{i=1}^s \sum_{j=1}^n y_{ij} / (ns)$$

Conventional notation: dot indicates summation over index, bar indicates average.

- Subject Effects:

$$\hat{\beta}_i = \bar{y}_{i.} - \bar{y}_{..}$$

(Note that $\sum_i \hat{\beta}_i = 0$.)

- Variance:

$$\hat{\sigma}^2 = \frac{\sum_{i=1}^s \sum_{j=1}^n (y_{ij} - \bar{y}_{i.})^2}{s(n-1)}$$

$$\sim \sigma^2 \chi_{s(n-1)}^2 / [s(n-1)]$$

Random-Effects Estimates: One-Way ANOVA

- Grand Mean:

$$\hat{\mu} = \bar{y}_{..} \equiv \sum_{i=1}^s \sum_{j=1}^n y_{ij} / (ns)$$

- Variance Components:

$$\hat{\sigma}^2 = \frac{\sum_{i=1}^s \sum_{j=1}^n (y_{ij} - \bar{y}_{i.})^2}{s(n-1)}$$

$$\sim \sigma^2 \chi_{s(n-1)}^2 / [s(n-1)].$$

$$MS_b = \sum_{i=1}^s n(\bar{y}_{i.} - \bar{y}_{..})^2 / (s-1)$$

$$\sim (n\sigma_b^2 + \sigma^2) \chi_{s-1}^2 / (s-1)$$

One-Way FE ANOVA (Cont'd)

- Inference for μ :

$$\hat{\mu} \sim N [\mu, \sigma^2 / (ns)]$$

Under $H_0 : \mu = 0$

$$\frac{\hat{\mu}}{\hat{\sigma} / \sqrt{ns}} \sim T_{s(n-1)}$$

One-Way RE ANOVA (Cont'd)

- Inference for μ :

$$\hat{\mu} \sim N [\mu, \sigma_b^2 / s + \sigma^2 / (ns)]$$

$$MS_b / (ns) \sim [\sigma_b^2 / s + \sigma^2 / (ns)] \chi_{s-1}^2 / (s-1)$$

Under $H_0 : \mu = 0$

$$\frac{\hat{\mu}}{\sqrt{MS_b / (ns)}} \sim T_{s-1}.$$

Comments on One-Way ANOVA

- The *estimates* (here, $\hat{\mu}$) are the same for both the random and fixed analyses.
- The *standard errors* are different. The standard error of $\hat{\mu}$ under a FE model is σ/\sqrt{ns} . Under a RE model it is $\sqrt{\sigma_b^2/s + \sigma^2/(ns)}$. Note the between-subject component, which depends only on s , not on n .

One-Way ANOVA Comments (Cont'd)

- The estimated standard error of $\hat{\mu}$ for the FE analysis makes use of all of the data:

$$\hat{\sigma}^2 = \frac{\sum_{i=1}^s \sum_{j=1}^n (y_{ij} - \bar{y}_{..})^2}{s(n-1)}$$

- The estimated standard error of $\hat{\mu}$ for the RE analysis uses *only the group means*:

$$\text{Var}(\hat{\mu}) = \frac{\sum_{i=1}^s n(\bar{y}_{i.} - \bar{y}_{..})^2}{(s-1)ns}$$

Two-Stage Approach for Random Effects

- Stage 1: Obtain the a map of effects for each subject.
- Stage 2: Use these effect maps (by analogy, the estimates y_i in our one-way ANOVA example).
- Form the t -statistic for an overall test of significance of the effect or contrast.
- Note why the subject-separability, balance, and equal variance assumptions are required: we need to be able to estimate the individual subject maps independently, and these maps need to enter into the second stage on “equal footing”.

Critique of Usual Two-Stage Approach

- The usual two-stage approach to multi-subject analyses treats the contrast estimate maps from each subject as given data, without consideration of the uncertainty in these values, which may be considerable and which may differ from subject to subject.
- A better approach is two summarize a contrast of interest by *two* maps: a contrast estimate map, and a corresponding standard error map. This is the approach advocated by Worsley (*NeuroImage* (2002)), for example.

- **Within-run analysis:** Fit linear model with cubic regression spline terms for trend, assuming AR (p) error structure. Prewhiten using estimated covariance matrix, and refit.
- Covariance matrix is estimated by implicitly solving Yule-Walker equations; correlations are corrected for bias and spatially smoothed.
- For a contrast of interest, summarize each run with a contrast map and a SE map.

- **Between-Subject Analysis:** Fit a second-level model, fixing the “within” errors at their estimates, and estimating (EM/REML) “between” variance σ^2 , and possible second-level fixed-effect covariates.
- Regularize σ^2 by spatially smoothing between/within ratio. Estimate approximate degrees of freedom of smoothed σ^2 using Gaussian random field theory, form T - or F -statistic map for second-level covariates.

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A Bayesian Approach

- Assume χ^2 and normal contributions to the likelihood for the within-subject variances and contrast estimates, respectively.
- Model the between-subject effects as normally distributed with mean zero and unknown variance.
- Use non-informative prior distributions for within-subject standard deviations, contrast estimates, and usually (but not necessarily) for the between-subject standard deviation.

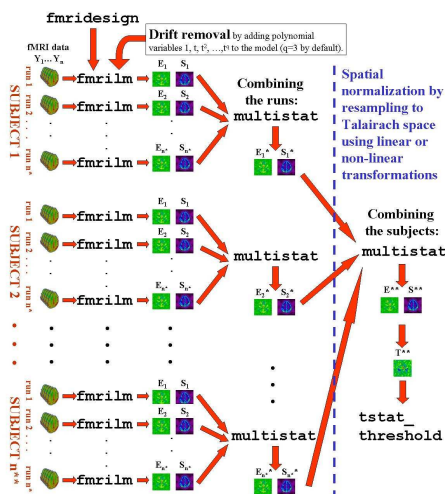


Figure 1: *Fmristat* flow chart for the analysis of several runs (only one session per subject): E = effect, S = standard deviation of effect, $T = E/S = T$ statistic.

- Calculation of posterior distribution of contrast is straightforward by numerical integration.
- Introducing subject-level covariates (e.g., age, treatment) is easy in principle, though simulation (“Gibbs Sampler”) will have to replace exact integration.

$i = 1, \dots, k$ indexes subjects

$j = 1, \dots, n_i$ indexes time points

$$p(x_{ij} | \delta_i, \sigma_i^2) = N(\delta_i, \sigma_i^2)$$

$$p(\sigma_i) \propto 1/\sigma_i$$

$$p(\delta_i | \mu, \sigma^2) = N(\mu, \sigma^2)$$

$$p(\mu) \propto 1$$

$$p(\sigma) \propto 1$$

-p.7

-p.7

Posterior for μ given $\sigma = 0, k \geq 1$

The General Case: $\sigma \geq 0$

Given $\sigma = 0$, then the posterior distribution of the consensus mean μ is proportional to a product of scaled t -densities:

In general, $p(\mu | \sigma, \{x_{ij}\})$ is proportional to a *product* of the distributions of the random variables

$$p(\mu | \{x_{ij}\} | \sigma = 0) \propto \prod_{i=1}^k \frac{1}{t_i} T'_{n_i-1} \left(\frac{x_i - \mu}{t_i} \right)$$

$$U_i = x_i + \frac{s_i}{\sqrt{n_i}} T_{n_i-1} + \sigma Z,$$

- where T_{n_i-1} is a t -distributed random variable with $n_i - 1$ degrees of freedom, Z is distributed $N(0, 1)$, and T_{n_i-1} and Z are independent.
- $t_i = s_i / \sqrt{n_i}$ is within-subject SE; x_i is within subject mean.

-p.7

-p.7

A Useful Probability Density

Let T_ν and Z denote independent Student- t and standard normal random variables, and assume that $\psi \geq 0$ and $\nu > 0$. Then

$$U = T_\nu + Z\sqrt{\frac{\psi}{2}}$$

has density

$$f_\nu(u; \psi) \equiv \frac{1}{\Gamma_{\nu/2}\sqrt{\pi}} \int_0^\infty \frac{y^{(\nu+1)/2-1} e^{-y\left[1+\frac{u^2}{\psi y+\nu}\right]}}{\sqrt{\psi y+\nu}} dy.$$

Example 1: Spatial Visual Cueing

Pollmann, S. and Morillo, M. (2003). "Left and Right Occipital Cortices Differ in Their Response to Spatial Cueing," *NeuroImage*, 18, 273-283.

Neumann, J. and Lohmann, M. (2003). "Bayesian Second-Level Analysis of Functional Magnetic Resonance Images," *NeuroImage*, 20, 1346-1355.

Posterior of (μ, σ)

- Assume $\delta_i \sim N(\mu, \sigma^2)$, $\sigma \sim p(\sigma)$, $p(\mu) \propto 1$, $p(\sigma_i) \propto 1/\sigma_i$.

- Then the posterior of (μ, σ) is

$$p(\mu, \sigma | \{x_{ij}\}) \propto p(\sigma) \prod_{i=1}^p \frac{1}{t_i} f_\nu \left[\frac{x_i - \mu}{t_i}; \frac{2\sigma^2}{t_i^2} \right].$$

- The posterior of μ given $\sigma = 0$ is a product of scaled t -densities centered at the x_i , since

$$\frac{1}{t_i} f_\nu \left[\frac{x_i - \mu}{t_i}; 0 \right] = \frac{1}{t_i} T'_\nu \left(\frac{x_i - \mu}{t_i} \right).$$

- We will take $p(\sigma) = 1$, though an arbitrary proper prior does not introduce additional difficulties.

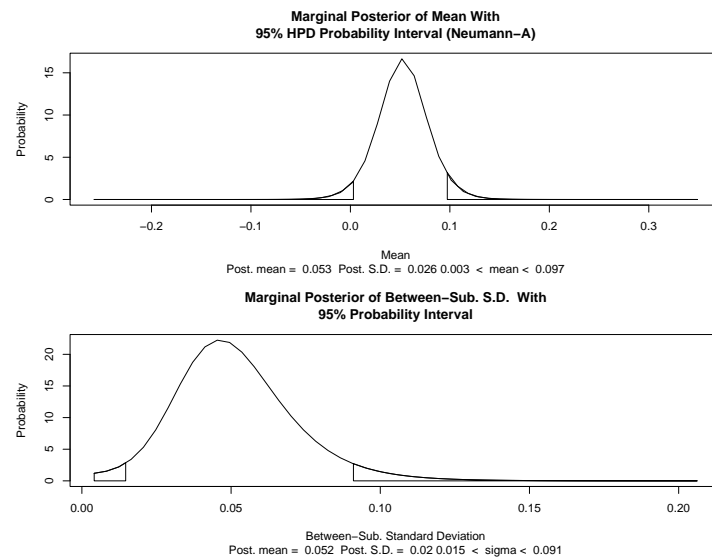
Occipital Cortex and Spatial Cueing

- Visual cue (large or small) on one side of screen (left or right).
- Subject told to fixate on center of screen, but pay attention to side where cue appeared.
- Target appeared either on same side as cue (valid trial) or opposite side (invalid trial)

Pollman and Marillo, Results

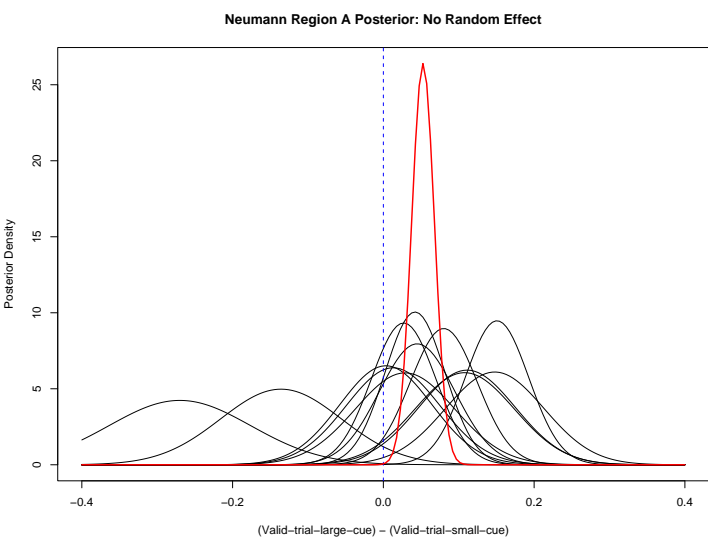
- Main results: Contrast of valid-trial LHS with valid trial RHS showed significant differences in bilateral lingual gyrus and lateral occipital gyrus, and IPS/TOS.
- Second contrast: **valid-trial-small-cue** with **valid-trial-big-cue** significant in three regions from Bayesian analysis of Neumann and Lohmann (2003).

Region A: *valid-small-cue* vs *valid-large-cue*



-p. 8

Posterior A: *valid-small-cue* vs *valid-large-cue*



Example 2: Sensitization to Angry Faces

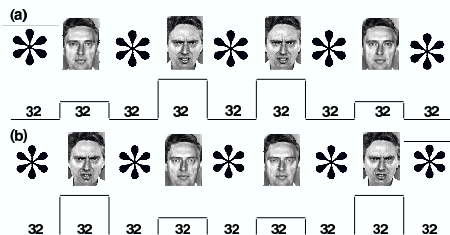
Strauss M., Makris N., Kennedy D., Etcoff N., Breiter H. (2000). "Sensitization of Subcortical and Paralimbic Circuitry to Angry Faces: An fMRI Study," *NeuroImage* 11, S255.

Strauss, M.M. (2003). "A Cognitive Neuroscience Study of Stress and Motivation," Phd Dissertation, Department of Psychology, Boston University.

Sensitization to Angry Faces

Eight participants passively viewed alternating blocks of angry and neutral Ekman faces, with fixations in between.

Figure 1: Experimental Paradigms



Angry Faces: Design

Subject	Sequence			
A	1	2	1	2
B	1	2	1	2
C	2	1	2	1
D	2	1	2	1
E	1	2	2	1
F	1	2	2	1
G	1	2	2	1
H	2	1	1	2

... where NAAN = 1 and ANNA = 2.

Habituation vs. Sensitization

- One typical aspect of block designs (such as the “angry faces” study) is that subjects tend to *habituate* to the stimulus, with consequent decreased BOLD activation.
- An interesting aspect of the present data is that, in many regions subjects tended to have a *stronger* BOLD response in the second half as compared to the first. This is called *sensitization*.

A Regression Model

- For “representative” voxels in each subject:

$$\log(y_t) = \beta_0 + \beta_{\text{half}} + \beta_{\text{type}} + \beta_{\text{half}} \times \beta_{\text{type}} + \epsilon_t$$

- where β_{type} is a 3-level factor for face type (Angry, Neutral, Fixation); β_{half} (levels 1 and 2) compares the first and second half of the experiment, and ϵ_t is (for simplicity) here modeled as white noise.

Habituation/Sensitization Contrast

- For models of log of the data, contrasts become dimensionless ratios. (Only BOLD changes have real meaning.)
- The following contrast is useful for testing for sensitization/habituation:

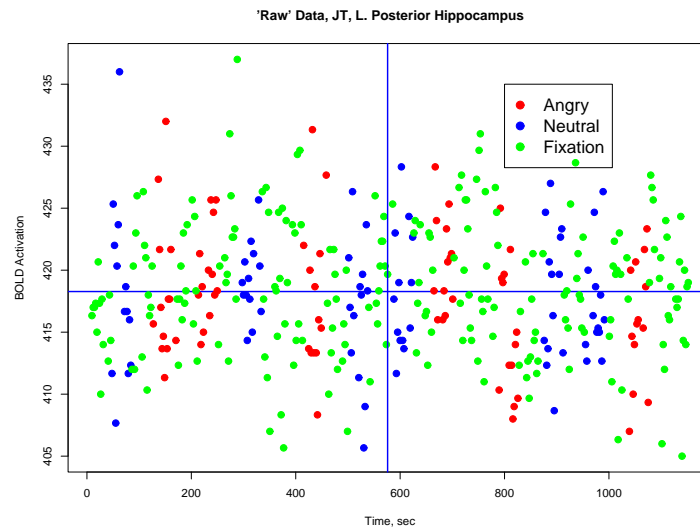
$$c_S = \exp[(\beta_{A,2} - \beta_{N,2}) - (\beta_{A,1} - \beta_{N,1})]$$

- We also looked at

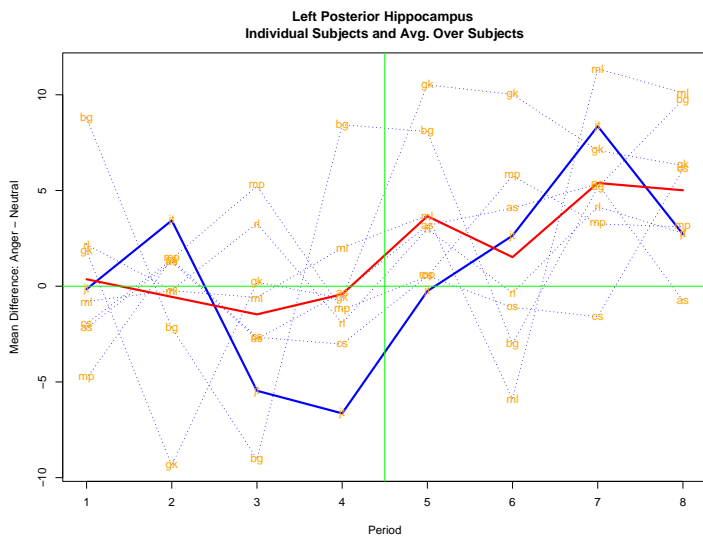
$$c_H = \exp(\beta_{N,2} - \beta_{N,1})$$

- Data from each subject are summarized by contrasts estimates and standard errors, which are used as input to a second-level Bayesian analysis.

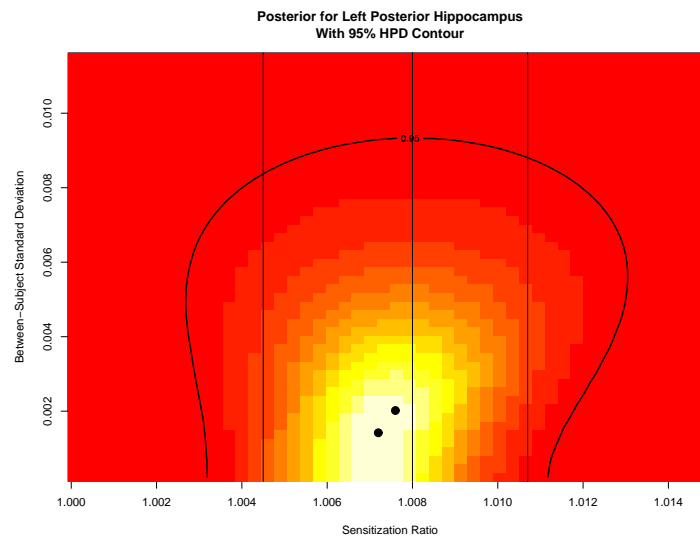
Typical 'Raw' BOLD Timecourse



Block Averages For All Subjects

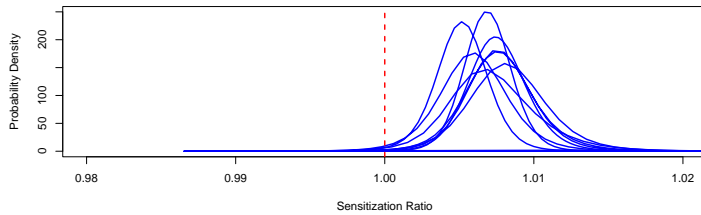


Posterior for LPHIP Sensitization

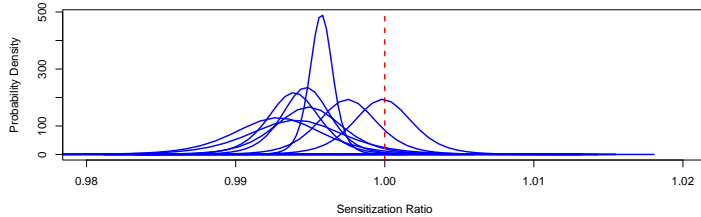


A/N: Sensitization N/N: Habituation

Anger – Neutral Interaction With Block



Neutral – Neutral Interaction With Block



Not Enough Subjects (Cont'd)

- It's important to realize that the large standard errors for RE analyses with few subjects is usually not a fault of the methodology. Rather, one is incorporating σ_b^2 in the standard errors of the estimates, and this is quantity which can't be well estimated except under two conditions:

- You have lots of subjects, and so σ_b^2/s is reasonably small, and your t -test for effect significance has adequate degrees of freedom.
- You *regularize* the estimate of $\hat{\sigma}_b^2$ by including information which isn't in the data. This can be done explicitly, via a prior distributions and a Bayesian analysis, or implicitly, as in Worsley (2002).

The Problem of Not Enough Subjects

- Random-effects models include variability between subjects into the standard errors of estimates.
- If you only have a few subjects (e.g., 5 or so), then there is not much information in the data to estimate this variability!
- So your standard errors are large, and it's much harder to establish significance than it is with FE analyses. (Note the degrees of freedom of the t -statistics in our example: $n(s - 1)$ for FE; $s - 1$ for RE. So the t -distribution is more diffuse, *and* the standard error has the extra σ_b^2/s term.)

Typicality

- Friston, Holmes and Worsley (*NeuroImage*, 1-5, 1999) introduce the concepts of *typicality* and *conjunction analysis* as a way to make inference with respect to a population in a fixed-effects context.
- If one has a small sample of subjects, and a certain feature is observed in several of these subjects (adjusting for multiple comparisons), then one can say, qualitatively, that this feature is "typical," and thus likely to be present in a population.
- This is to be contrasted from quantitative assessment of what the "average" effect is in a randomly selected subject from a population.

Conjunction Analysis

- In *conjunction analysis*, one attempts to find what activation is statistically significantly in all (or, perhaps, most) subjects.
- This feature can then be thought of as typical, i.e., more likely than not to be present in the population from which the subjects are drawn.

III. Model Validation

- The GLM is a very powerful tool, but like any modeling tool, it is only good to the extent that the modeling assumptions are valid.
- If assumptions are grossly violated, then inferences can be seriously misleading.

Linear Model (GLM) Assumptions

- The assumptions underlying the model include:
 - The form of the model for the mean.
 - The temporal correlation structure, and equal-variance assumptions.
 - Gaussian errors.
 - Separation of signal from noise (e.g., What part of the trend in a time course is a “nuisance effect” to be filtered out, and what part of it is slowly varying signal?)

The Form of the Model

- If your X matrix does not appropriately model the factors contributing to mean activation, then your estimates can be seriously biased.
- This bias can, in principle, be detected by looking at the residuals.
- Think of the example of a straight line fit to data for which a parabola would be much
- How would the residuals (deviations from the fit) tell you that your model is inappropriate?

Error Variance Assumptions

- Inappropriate modeling of temporal correlation can give you a biased estimate of the uncertainty in effects, and grossly incorrect estimates of degrees of freedom for voxel t - or F -statistics.
- In principle, one can test this by looking to see if the residuals at each time course are (at least approximately) white noise.

- p. 10

Error Variance Assumptions (Cont'd)

- How does the temporal autocorrelation vary from voxel to voxel? Is it adequate to use the same model for each voxel?
- Assuming equal within-voxel variances when these variances differ considerably is also something that one might want to look out for, though checking the correlation estimates is probably more important.

- p. 10

Gaussian Errors

- When doing inference, we assume that the noise in our data follows Gaussian distributions.
- (This assumption is necessary for determining standard errors of estimates; it is not required for the estimates themselves.)
- Fixed effects analysis are not very sensitive to violation of this assumption. The central limit theorem implies that averages tend to be Gaussian in many situations, and coefficient estimates are essentially weighted averages. The central limit theorem also tells us that standardized contrasts will have approximate t -distributions under fairly general conditions (provided the standard errors and degrees of freedom are appropriately estimated)

- p. 10

Gaussian Errors (Cont'd)

- This robustness, unfortunately, does not extend to random effects. Estimates of variances between subjects, for example, will likely be sensitive to to the assumption of Gaussianity. That being said, Gaussian random-effects models are very widely used, because there are not good alternatives.

- p. 10

Separation of Signal from Noise

- A necessary step in any fMRI analysis is to remove nuisance effects from the data.
- Usually these results are low-frequency trends, and they are removed either by high-pass filtering, or by explicit modeling via covariates in the GLM.
- Always keep in mind that if you have signal which looks like the trend being removed, then you might be “throwing the baby out with the bathwater.”
- One example might be a nuisance physiological effect, which you’d like to model and remove. If this effect is, at least in part, associated with an experimental stimulus, then you could be discarding important signal with the noise.

- p. 10

Model Selection

- In any course in regression analysis, one learns how to choose a “best” model from within a family of interesting candidate models.
- Part of this approach involves examining candidate models for goodness-of-fit, mostly by examining residuals as discussed earlier.
- Another part of this approach is model comparison, which involves fitting a “large” model, with perhaps too many parameters, and then comparing this fit to a “smaller” model in which some of these parameters are constrained, either to equal zero, or else perhaps to equal each other.

- p. 10

Model Selection (Cont’d)

- Model comparison thus reduces to hypothesis testing, in the simplest textbook situations, to F -tests.
- This approach can be applied to fMRI, although instead of a single F -test, we will have F maps and associated p -value maps to interpret.
- More general model comparison tools compare the reduction in residual sum of squares between nested models, penalizing for complexity due to adding parameters. Two such criteria are AIC and BIC (Akaike Information Criterion; Bayesian Information Criterion).

- p. 10