BLIND SEARCH FOR THE REAL SAMPLE: APPLICATION TO THE ORIGIN OF ULTRA-HIGH ENERGY COSMIC RAYS

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ABSTRACT

We suggest a method for statistical tests which does not suffer from a posteriori manipulations with tested samples (e.g. cuts optimization) and does not require a somewhat obscure procedure of the penalty estimate. The idea of the method is to hide the real sample (before it has been studied) among a large number of artificial samples, drawn from a random distribution expressing the null hypothesis, and then to search for it as the one demonstrating the strongest hypothesized effect. The statistical significance of the effect in this approach is the inverse of the maximal number of random samples at which the search was successful. We have applied the method to revisit the problem of correlation between the arrival directions of ultra-high energy cosmic rays and BL Lac objects. No significant correlation was found.

Subject headings: catalogues – cosmic rays – methods: data analysis – methods: statistical

1. INTRODUCTION

Communications about effects detected at a marginally significant level constitute a considerable fraction of all scientific results. The scientific society usually treats such communication with skepticism. Indeed, too many marginally significant effects have not withstood the data accumulation.

High energy astrophysics gives a number of instructive examples of searches for marginally significant effects. Indeed, there are many detection of particles or transient gamma-ray events whose sources (i.e. objects known from observations at other wavelengths) are unknown. This stimulates intensive searches of various correlations between different classes of events and objects. For example, there is a number of works reporting detections of correlation between locations of gamma-ray bursts (or their sub-samples) and various objects: galaxy clusters (Kolat & Piran 1996), Galactic plane (Belli 1997), and the local galactic arm (Komberg, Kurt, & Tikhomirova 1997). None of these results has been confirmed. Another similar area is ultra-high energy cosmic rays (UHECRs) and searches for their hypothetic sources. A claim of significant autocorrelation in the arrival directions of UHECRs detected by the Akeno Giant Air Shower Array (AGASA) (Hayashida et al. 1996; Takeda et al. 1999) motivated searches of cross-correlations between UHECRs and various astrophysical objects. Particularly there were reported statistically significant cross-correlation signals between UHECRs and BL Lac objects (Tinyakov & Tkachev 2001, hereafter TT01, but see Evans, Ferrer & Sarkar 2003), super-galactic plane (Uchihori et al. 2000), radio-loud compact quasars (Virmani et al. 2002), highly luminous, bulge-dominated galaxies (presumably, nearby quasar remnants, Torres et al. 2002) and Seyfert galaxies (Uryson 2004).

The reason for abundance of detected correlations is quite evident: a number of various possible effects, which have been searched for with statistical methods, is large and it is not surprising that some of them demonstrate a marginally significant signal just by chance. The situation is even worse because typically a probed effect is somewhat uncertain and the researcher tries different versions of the hypothesis, varying parameters and applying various cuts to the data samples. This means that the researcher performs a number of tests of the same effect which are neither independent nor completely dependent. These numerous trials, again, increase the probability to observe a signal in one of the trials by chance and the analysis of this kind of bias is difficult. We illustrate this problem in Section 4 and in Fig. 1.

Does it mean that one should reject the possibility to manipulate the data samples with cuts and parameters? A blind test when all cuts and parameters in a statistical test have been set and motivated a priori, is a good style. But there are many situations when such a priori definition of a test is very problematic and the investigator sometimes really needs the rights to vary the testing procedure and to see what will happen.

In principle, the researcher can account for these numerous trials using random samples, representing the null hypothesis. Often it is done in the following way (see e.g. TT01). The investigator prepares a large array of $N$ random samples, $d_i$, and does the same estimate of the effect for each of these samples as he does for the real samples, $d^0$, in each statistical trial. Let a statistic associated with a confidence of the effect (e.g. $1 - p$, where $p$ is the probability to obtain the result from null hypothesis) be $S_j = F(d^0, C_j)$, where $C_j$ is a set of cuts and/or analysis parameters from the universe $C$ of all cuts and parameters. First, one finds the maximum for the real sample $S_{j_0}^0 = \max\{F(d^0, C_j)\}$ which is reached at $j = j_0$. Then, one performs similar search for random samples $S_{j_0}^0 = \max\{F(d^0, C_j)\}$. The significance can be defined as the fraction of random samples satisfying the condition $S_{j_0}^i > S_{j_0}^0$. This value differs

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from the straightforward (uncorrected for numerous trials) estimate of the significance by the “penalty factor”
\[ N(S_i^{\text{max}} > S_0^{\text{max}})/N(S_0^{\text{max}}), \]
where \( S_0^{\text{max}} = F(d^i, C_{j0}) \) and \( N(\bullet) \) is the number of samples satisfying a condition \( \bullet \). This procedure is sufficient if (i) the investigator follows the above procedure precisely; (ii) the investigator does not use the \textit{a posteriori} information on the real sample for the planning of the investigation strategy.

We would like to notice that both conditions are not so easy to satisfy once the investigator studied the real sample and feels which combination of cuts or model parameters will provide the most significant signal. Then he can find the most favorable trial intuitively, avoiding a large number of unfavorable ones. In other terms, the investigator can introduce a bias in the choice of \( C_j \) and overestimate the significance of the effect using \textit{a posteriori} knowledge. We should emphasize that the investigator can introduce such bias not deliberately. This is a serious disadvantage of the approach. Such kind of bias is difficult to trace and we consider this method to be insufficiently credible.

In this work, we suggest a new approach giving a simple way of avoiding this “pressure” of the \textit{a posteriori} information. The investigator can hide the real sample inside a large array of random null hypothesis samples \textit{prior to any data analysis}. Now we have a single array of \( N \) samples \( d^i \).

One of them is real (the investigator does not know which), other are random. The problem is thus inverted: instead of confirming the hypothesis using the real sample, the investigator must find the real sample in the array using the hypothesis that the verified effect exists, i.e. find \( i_{\text{max}} \) corresponding to the maximum value of \( S_j^{\text{max}} = F(d^i, C_j) \). This is a blind test: the investigator does not know where is the real sample and he can feel free to perform numerous trials. If the investigator finds the real sample, the significance of the effect is just the inverse of the number of samples in the array.

An alternative to our method is the cross-validation method, where the search for an effect is carried out on a fraction of the data sample. Therefore it is less sensitive. Below we demonstrate our method applying it to the problem of UHECRs – BL Lac correlation.

2. Procedure

2.1. Catalogs

We used the AGASA sample of UHECRs with 58 events above \( 4 \times 10^{19} \, \text{eV} \) and a catalog of Véron-Cetty & Véron (2003) containing 876 BL Lac objects. We do not combine the AGASA sample with the data from other experiments because other samples are smaller and problems associated with the non-uniform structure of a joint sample would overweight the statistical gain. The BL Lac catalog has been cut in declination at \(-10^\circ\) and was subject to various brightness cuts. We also tried a sub-catalog of confirmed BL Lacs which includes 491 objects. Actually it is not clear which catalog is more relevant (TT01 used a confirmed sub-catalog) and therefore we try both variants.

2.2. Null hypothesis and random samples

Null hypothesis in our case is just the isotropic distribution of arrival directions of UHECRs convolved with the AGASA exposure function. The latter is a function of declination and does not depend on right ascension. This provides a simple way to prepare random, null-hypothesis samples avoiding possible uncertainties in the latitude exposure function: to sample the right ascension uniformly keeping the actually observed declination for each event. We, nevertheless, have dispersed the declinations of UHECRs by \( \pm 3^\circ \) around their real values in order to destroy a possible small-scale latitude correlation, if the latter exists. Such small dispersion does not distort a much wider exposure function.

When performing the test we have distributed roles: one of the coauthors acts as an “investigator”, another plays a role of “examiner”. Examiner has prepared an array of 99 random samples as described above and inserted the real sample into the array keeping the sequential real sample number in secret from the investigator. He did not participate in the data analysis until the investigator made his final choice.

2.3. Measure for the correlation signal

We used usual two-point correlation function counting the number \( n_e \) of UHECRs within angle \( \delta \) from any BL Lac of a given catalog. Then we compare this number with expectation \( n_\text{e} \) for the null hypothesis:

\[
n_\text{e} = N_{\text{BL}} N_U \frac{1 - \cos \delta}{1 - \cos(-10^\circ)},
\]

where \( N_{\text{BL}} \) is the number of BL Lacs in the catalog, \( N_U = 58 \) is the number of UHECRs, \(-10^\circ\) is the declination cut on BL Lacs. Note that this expectation implies an isotropic distribution of at least one sample. This is not the case because the AGASA sample has a latitude anisotropy and BL Lac catalog is anisotropic respectively to the galactic plane (selection effect) and the cosmological large-scale structure. A more accurate estimate differs from that given by equation (1) by a factor

\[
F = \frac{\sum_{i=1}^{N_{\text{BL}}} \xi(\theta_i)}{N_{\text{BL}}(\xi)}, \tag{2}
\]

where \( \xi(\theta) \) is the AGASA exposure function. The exposure function depends on particle energy and is hardly known better than one can extract from the latitude distribution of detected UHECRs. Takeda et al. (1999) use a polynomial fit to the observed latitude distribution of events above \( 10^{19} \, \text{eV} \). We prefer to use the observed distribution of the available AGASA sample (above \( 4 \times 10^{19} \, \text{eV} \)) in a form of histogram in \( \cos \theta \) with the bin width 0.1 since this is a simplest option that can be easily reproduced.

Factor \( F \) depends on the BL Lac catalog and therefore on cuts. According to our estimates with equation (2), \( F \) is close to 1 for radio-bright objects and \( \sim 1.2 \) for optically-bright objects (probably due to anisotropy caused by galactic absorption). We introduce the measure of the signal, \( p \) (which depends on \( \delta \) and cuts in the BL Lac catalog), as the probability to sample \( n \) or more hits from the Poisson distribution at expectation \( F n_\text{e} \).

Note that for autocorrelated samples the distribution of \( n \) is not Poisson, therefore this measure is not exact. In order to correct this probability for the actual autocorrelated distribution of BL Lacs on the sky, we perform Monte-Carlo simulations using a large number of random UHECR samples. The maximal disagreement between the Poisson and Monte-Carlo probabilities is by a factor of 2.
Thus, we use the Poisson probability, $p$, for preliminary estimates and recalculate the probability for the leading samples (given in Table 1) with Monte-Carlo simulations.

| R or O | ID | $n_{\text{obj}}$ | $C_r$ or $C_o$ | $\delta^*$ | $p \times 10^4$ |
|--------|----|-----------------|---------------|------------|----------------|
| R 90   | 256| 0.04            | 2             | 2.6        |
| R 40   | 139| 0.16            | 3             | 3.2        |
| R 11   | 35 | 0.79            | 2             | 5          |
| O 90   | 153| 17.5            | 2             | 3.12       |

| Confirmed BL Lacs |
|-------------------|
| R 11 | 6 | 0.79 | 2 | 1.1 |
| R 4  | 197| 0.02| 1.5| 3.47 |
| R 90 | 6 | 0.79| 3 | 8   |
| O 4  | 118| 18 | 1.5| 1.15|

- **Cut applied in radio, R, or optical, O, brightness**
- **Identification number of a sample giving the strongest correlation signal**
- **Number of objects passing the cut**
- **Optimal cut in 6 GHz radio flux or visual magnitude**
- **Optimal correlation angle**
- **Significance level**

3. SEARCH FOR THE BEST-CORRELATING SAMPLE AND ITS RESULTS

Optimizing cuts in all existing parameters we can fit a BL Lac catalog to any set of locations in the sky so that it will demonstrate a highly significant correlation (see Sect. 4). Therefore, if our objective is to find the real sample, we have to try the most relevant cuts. The apparent radio- or optical brightness of objects (represented in the catalog by their observed radio flux density measured in Jy and the visual magnitude $V$) seem to be good indicators of particle acceleration to ultra-high energies. To avoid “over-optimization” of random samples in two-dimensional scan, we performed two separate scans:

1. We optimized cut $C_r$ in the 6 GHz radio flux within the limits 0.01 Jy $< C_r < 2$ Jy, varying it with the step 0.1 in decimal logarithm. No cuts in optical brightness was applied. This scan is marked with letter R in Table 1.

2. We optimized cut $C_o$ in visual magnitude within the range from $V = 12$ to $V = 24$ with the step $\Delta V = 0.5$. No cuts in radio flux was applied and we excluded objects with no data on their radio brightness. This scan is marked with letter O in Table 1.

The proper correlation angle $\delta$ is somewhat uncertain. The most significant correlation should not certainly appear at a correlation angle equal to 1σ experimental error (the latter depends on the particle energy). If UHECRs are charged, then the correlation could appear at $\delta$ corresponding to a typical angle of particle deflection. We optimized $\delta$ between 15° and 5° with the step 0°.5. The samples that give the most significant correlation are listed in Table 1. In addition, we also tried a scan over the intrinsic radio luminosity as was done in TT01. The strongest effect gave sample #11: $p = 4 \times 10^{-4}$ with 25 intrinsically brightest BL Lac objects and $\delta = 3°$.

With these results at hand, the investigator had to make a choice concerning the real sample. All best samples (except #4) have a reasonable value of optimal $\delta$ ($2°$ and $3°$), which is close to the angular resolution of AGASA of $2°$.3. Finally, the “investigator” used sample #11 as the first choice. The second option was sample #90.

The second task is the test for autocorrelation of the UHECR arrival directions. It was performed with the same array of random samples before the “investigator” was informed about the results of his choices in the first test. The autocorrelation signal is estimated in a similar way as described above for the cross-correlation signal:

$$n_e = \frac{N_U(N_U - 1)}{2} \left(1 - \cos\delta\right), \quad F = \frac{\sum_{i=1}^{N_U} \xi(\theta_i)}{N_U(\xi)},$$

where factor $F = 1.4$.

Now, sample #67 showed maximum signal of $p = 5 \times 10^{-3}$ at $\delta = 2.5$ (8 hits). The second sample showing strong autocorrelation was #30 with $p = 1.7 \times 10^{-3}$ at $\delta = 2.1$. The choice of the investigator was #67.

The real observed sample of UHECRs had sequential number #67. Therefore the test at 99 per cent confidence level was unsuccessful for UHECRs–BL Lacs correlation and successful for UHECRs autocorrelation. Then we checked sample #67 for the cross-correlation with BL Lacs by varying $C_r$ and have not found any significant signal.

4. INTERPRETATION OF THE RESULTS

We can confirm that the autocorrelation signal in AGASA sample with the given energy threshold has a significance of at least $10^{-2}$. To find the significance level we would have to vary the size of the random array and to find the limit when we are able to find the real sample. This objective is beyond the scope of this work. Probably, according to the correlation signal in the second best sample, the significance is around $3 \times 10^{-3}$ in agreement with Finley & Westerhoff (2004). One should notice, however, that this result refers to a specific sample with the energy cut of $4 \times 10^{19}$ eV (see Finley & Westerhoff 2004, for the discussion). To estimate the significance of real autocorrelation one has to perform the same procedure with an untruncated sample of UHECRs varying the energy cut in a reasonable range.

Our negative result on cross-correlation with BL Lacs does not mean that we have found a quantitative disagreement with the results of TT01. They have found a positive signal with an another catalog of the confirmed BL Lac objects. Their cuts were: $z > 0.1$ or unknown, $C_r = 0.17$ Jy, $C_o = 18^{m}$. At these cuts the positive signal still exists at $p = 1.9 \times 10^{-2}$ and $\delta = 2.5$ (with factor $F = 1.24$, see Eq. 2) and the real sample #67 is the second significant among 99 random samples (having similar significance with three other samples including sample #11).
We just demonstrated that using the most straightforward assumptions, blindly, one can hardly find the correlation signal. Regarding more specific cuts, like in TT01, one meets a problem of interpretation of the signal whether it is real or is just a consequence of cuts optimization (see also Evans, Ferrer & Sarkar 2003, 2004). The claim, that a given cut was motivated independently rather than was optimized, is not convincing unless the motivation has been done a priori.

Now let us demonstrate how the multiple cuts optimization can actually mimic a significant signal. In this demonstration we use $10^4$ random UHECRs samples as described in Sect. 2.2 and the BL Lac catalog with cuts, optimized for each random sample. Fig. 1 shows the fraction of random samples $\eta$ which demonstrated a “significance of correlation” higher than $p$, after cuts optimization. If we fix all the cuts (curve 1), then there is an approximate agreement between $\eta$ and $p$. If we optimize one cut, $C_t$, then we obtain $\eta$ a few times greater than $p$ (actually, the ratio $\eta/p$ can be interpreted as the penalty factor discussed above). With two cuts optimization, adding a scan over visual magnitude, the ratio $\eta/p$ reaches almost two orders of magnitude and one out of 5 samples demonstrates $p < 0.01$. If we add an optimization for the correlation angle $\delta$, then every third random sample demonstrates a “significance” of $10^{-2}$, every tenth gives $p < 10^{-3}$, and one out of thousand gives $p = 10^{-6}$!

5. Summary

We presented a method of a blind search for a hypothetical effect where various trials with different sub-samples or model parameters do not affect the stated significance level. We believe that a tradition to use this method, when possible, would dramatically reduce the number of unconfirmed claims of marginally significant effects. The method is especially useful when: (i) there is a clear null hypothesis and a way to prepare random samples representing it; (ii) there exists a convenient measure of the statistical significance of the effect; (iii) the effect is uncertain in some respects, otherwise a test with the blind a priori formulation (i.e. it is a priori clear which data should be used and how the effect should look) is sufficient. Such problems as searches for cross-correlation between two classes of astrophysical objects usually satisfy all three conditions. We would like to emphasize that the proposed method is, in principle, applicable in any field of science.

In this work, we performed a demonstration for only one size of the array of random samples. To find the significance level of the effect, one should make several trials with different array size starting from a larger one, then reducing its size until the real sample is found. The examiner should not disclose the real sample after unsuccessful trials.

An effect detected with this method is credible because it ensures a researcher against unintentional overestimation of the significance. The only possible source of errors that can mimic a positive result is a wrong null hypothesis distinguishing random samples from the real sample. In the case considered in this paper, this could be for example a wrong exposure function of the UHECR detector. Otherwise, a positive result would have an explicit meaning: the chance that the effect does not exist is the inverse of the size of array of samples at the successful search.

As an application of the proposed method, we analyzed a possible UHECR–BL Lac correlation. We found no significant correlation, but cannot claim, of course, that correlation does not exist.

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