The Stochastic Approach to Index Numbers: Needless and Useless

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14 December 2014

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Abstract

The New Stochastic Approach (NSA) – unjustly – pretends to promote a better understanding of price index (PI) formulas by viewing them as regression coefficients. As prices in the NSA are assumed to be collected in a random sample (what is particularly at odds with official price statistics), PIs are random variables so that not only a point estimate but also an interval estimate of a PI can be provided. However this often praised "main advantage" of the NSA is hardly of any use from a practical point of view. In the NSA goodness of fit is confused with adequacy of a PI formula. Regression models are mostly farfetched, stipulate restrictive and unrealistic assumptions, replicate only already known PI formulas and they say nothing about axioms satisfied or violated by a PI.

Preliminary remark: In what follows I raise some questions regarding the New Stochastic Approach (NSA),[1] for which I could not yet find a satisfactory answer in the relevant literature. What motivated me was a paper submitted to a journal where the editor asked me to write a referee report. When I reviewed the enormously confusing paper I saw that the author made a show of complicated equations (giving no hint how he managed to derive them). He also reported a host of computer-produced simulations with confidence intervals, and he mentioned a great number of assumptions allegedly indispensable for the regression models of the NSA. However, conspicuously he saw no need to address some obvious questions regarding the rationale of the NSA. In my referee report I therefore asked some quite simple questions regarding the author's equations and the conceptual foundation of the NSA, however, I only got vague and a bit arrogant remarks in return. Annoyed I decided to save myself more trouble with this unpleasant paper and let it through. I bet, it is published meanwhile (as that's all what counts), and I am sure, it is not the only paper, where use is made of unintelligible mathematics only to conceal an author's inability (or outright unwillingness) to study the fundamentals of a method. However, to exculpate such authors, I guess that it is in no small measure the NSA itself which is to blame for such confusing papers, and as I came to learn more about the NSA it began to appear more and more questionable if not utterly needless and useless to me.

1. **Better understanding of formulas owing to regression models?** The so called NSA – advanced in particular by Clements and Izan (1987)[2], or Selvanathan and Rao (1994)[3] – boasts itself of enhancing the understanding of index formulas like $P_{0t}^{L} = \Sigma p_{it}q_{i0}/\Sigma p_{i0}q_{i0}$ (Laspeyres) or $P_{0t}^{P}\ \Sigma p_{it}q_{it}/\Sigma p_{i0}q_{it}$ (Paasche) by showing that such index functions, may be viewed as regression coefficients β_L and β_P in a simple *homogeneous* (or *restricted*, i.e. with the restriction of no intercept, $\alpha = 0$) linear regression equation $y_{it} = \beta_L x_{it} + u_{it}$

   (1) $\quad \frac{p_{it}}{p_{i0}}\sqrt{w_{i0}} = \beta_L\ \sqrt{w_{i0}} + u_{it}, \qquad u_{it} = \varepsilon_{it}\sqrt{w_{i0}}$ (i = 1, 2,…, n, S+P, p. 52)

   where $w_{i0} = p_{i0}q_{i0}/\Sigma p_{i0}q_{i0}$ are (expenditure) weights – later also weights $w_{it} = p_{it}q_{it}/\Sigma p_{it}q_{it}$, $w_{i0}^{*} = p_{i0}q_{it}/\Sigma p_{i0}q_{it}$, and $\overline{w}_i = \frac{1}{2}\left(w_{i0} + w_{it}\right)$ will be used – and the disturbances ε_{it} are assumed to comply with the standard assumptions $E(\varepsilon_{it}) = 0$, $var(\varepsilon_{it}) = \sigma^2$, and $cov(\varepsilon_{it}\varepsilon_{jt}) = 0$ for all $i \neq j$ is assumed (which is certainly not realistic an assumption). It is said that P_{0t}^{L}

[1] The CPI Manual of the International Labour Office (ILO) in cooperation with IMF, OECD, UNEC, Eurostat and The World Bank (Geneva 2004) made a distinction between the early "unweighted" and the new "weighted" stochastic approach. We prefer to speak of an old (OSA) and a new (NSA) stochastic approach respectively.

[2] Clements and Izan, The Measurement of Inflation: A Stochastic Approach, Journal of Business and Economic Statistics, vol. 5, nr. 3 (1987), pp. 339 - 350 (henceforth *quoted as C+I*).

[3] Selvanathan E.A. and D.S. Prasada Rao, Index Numbers, A Stochastic Approach, Ann Arbour (The University of Michigan Press 1994 For the references and more details about much of what is said above I also refer to my book "Index Theory and Price Statistics", Frankfurt/Main 2007, pp. 78 - 90 (in what follows *quoted as S+P*).

is represented by $\hat{\beta}_L$ in (1) just like $P_{0t}^P = \hat{\beta}_P$ in an analogous equation for the Paasche price index.[4] Obviously the least squares estimate of β_L in (1) is given by

(1a) $$\hat{\beta}_L = \frac{\sum x_{it} y_{it}}{\sum x_{it}^2} = \sum y_{it} x_{it} = \sum \frac{p_{it}}{p_{i0}} w_{i0} = P_{0t}^L,$$

because $\sum_i x_{it} y_{it} = \sum \frac{p_{it}}{p_{i0}} \left(\sqrt{w_{i0}}\right)^2 = P_{0t}^L$ and $\sum x_{it}^2 = \sum \left(\sqrt{w_{i0}}\right)^2 = 1$, while Σx, Σy and Σy^2 are meaningless expressions. A note concerning the error term: $E\left(P_{0t}^L - \beta_L\right) = E\left(\sum_i x_{it} u_{it}\right)$ renders $w_{10} \cdot 0 + w_{20} \cdot 0 + \ldots = 0$ when $u_{it} = \varepsilon_{it}\sqrt{w_{i0}}$ and for all i and t $E(\varepsilon_{it}) = 0$. So the queer factor $\sqrt{w_{i0}}$ makes sure that $P_{0t}^L = \hat{\beta}_L$ is an unbiased estimator of β_L. Likewise for the variance of $\hat{\beta}_L$ that is for $E[\Sigma(xu)^2]$ we get $\sum w_{i0}\sigma_i^2 = \sigma^2$ when $E(\varepsilon_i\varepsilon_i) = \sigma^2$ and $E(\varepsilon_i\varepsilon_j) = 0$ ($i \neq j$). By contrast, $\hat{\beta}_L^*$ in the (unrestricted) model

(2) $$\frac{p_{it}}{p_{i0}}\sqrt{w_{i0}} = \alpha_L + \beta_L^* \sqrt{w_{i0}} + u_{it}^* \text{ is given by}$$

(2a) $$\hat{\beta}_L^* = \frac{nP_{0t}^L - \sum x \cdot \sum y}{n - \left(\sum x\right)^2} = \frac{\frac{1}{n} P_{0t}^L - \bar{x} \cdot \bar{y}}{\frac{1}{n} - \bar{x}^2}. \text{ Thus } \hat{\beta}_L^* s_x^2 = \tfrac{1}{n}\hat{\beta}_L - \bar{x} \cdot \bar{y}$$

Evidently $\hat{\alpha}_L = 0$ implies $\Sigma y = \hat{\beta}_L^* \Sigma x$, so that $\hat{\beta}_L^* = \hat{\beta}_L = P_{0t}^L$.

If $p_{it}/p_{i0} = 1$ for all i then Σx = Σy and $\hat{\beta}_L^* = \hat{\beta}_L = P_{0t}^L = 1$. To derive $\hat{\beta}_L$ in (1a) we see that[5] $\sum_i \hat{u}_{it}^2 = \sum_i y_{it}^2 - 2\hat{\beta}_L \sum_i x_{it} y_{it} + (\hat{\beta}_L)^2 \sum_i x_{it}^2$ or (using 1a and deleting subscripts $_{it}$)

(1b) $$\sum \hat{u}^2 = \sum y^2 - \left(\hat{\beta}_L\right)^2 = \sum y^2 - \left(P_{0t}^L\right)^2 \text{ in contrast to the unrestricted model (2)}$$

$$\sum \hat{u}^{*2} = \sum y^2 + n\hat{\alpha}^2 + \left(\hat{\beta}_L^*\right)^2 \sum x^2 - 2\hat{\alpha}\sum y + 2\hat{\alpha}\hat{\beta}_L^* \sum x - 2\hat{\beta}_L^* \sum xy$$

Using $\Sigma x^2 = 1$, $n\hat{\alpha} = \sum y - \hat{\beta}_L^* \sum x$ and $\Sigma xy = \hat{\beta}_L = P_{0t}^L$ this reduces to

(2b) $$\sum \hat{u}^{*2} = \sum y^2 - \left(\hat{\beta}_L\right)^2 - n\hat{\alpha}^2 + (\hat{\beta}_L^* - \hat{\beta}_L)^2 = \sum \hat{u}^2 - n\hat{\alpha}^2 + (\hat{\beta}_L^* - \hat{\beta}_L)^2 \neq \sum \hat{u}^2.$$

(2b) is a variance decomposition, (1b) is not.[6]

However, it can easily be seen: if $\hat{\alpha} = 0$ in model (2) it must be by implication $\hat{\beta}_L^* = \sum xy = \bar{y}/\bar{x} = \hat{\beta}_L = P_{0t}^L$, hence also $\sum \hat{u}_i^{*2} = \sum \hat{u}_i^2$ due to (2b).

As a rule, also Σu (unlike Σu*) will not vanish (except when $p_{it}/p_{i0} = 1$ for all i, where also $P_{0t}^L = \hat{\beta}_L = 1$) but yield $\sum \hat{u} = \sum y - \hat{\beta}_L \sum x = n\hat{\alpha} + \left(\hat{\beta}_L^* - \hat{\beta}_L\right)\sum x$.[7] A useful expression also is $\sum \hat{u} = \sum \sqrt{w_{i0}}\left(p_{it}/p_{i0} - P_{0t}^L\right) \neq 0$ since $P_{0t}^L = \sum w_{i0}\left(p_{it}/p_{i0}\right)$.

In the Appendix (p. 12) we present a numerical example in order to illustrate the considerations above, and to present another model (along the pattern of (1)) proposed by S+P.

[4] You will get the Paasche model with β_P instead of β_L when $(w^*_{i0})^{1/2}$ is substituted for $(w_{i0})^{1/2}$ in (1).

[5] As Σy^2 is not a meaningful expression it would be difficult to give an interpretation to $\Sigma u^2 = \Sigma y^2 - \beta^2$. It is useful now – using the hat notation – to make a distinction between the "true" u and the sample estimate of u.

[6] It is well known that the variance decomposition (and thus also R^2, the coefficient of determination) does not apply to homogeneous regression. Hence (1b) unlike (2b) is *not* the "variance decomposition", we are used to, where the *variance* of y (*not* Σy^2) is equal to the sum of an explained and a residual component. Also R^2 is not applicable to homogeneous regression. An analogon to R^2 then would be $\beta^2/\Sigma y^2$ rather than β^2.

[7] A zero sum of u-hat would imply Σy=ΣxyΣx or spelled out in detail $\Sigma(p_{it}/p_{i0})(w_{i0})^{1/2} = \Sigma(p_{it}/p_{i0})w_{i0}\Sigma(w_{i0})^{1/2}$. Note $\Sigma u(w_{i0})^{1/2} = 0$, not the sum of u-hat (in the numerical example of the Appendix it is 0.00070178 > 0).

Obviously the restricted regression is not a very useful model to "explain" a price index, to compile a confidence interval and to study the goodness of fit (using R^2).

More importantly, however, we should ask: Does knowing that formulas, like P^L and P^P may also be viewed as regression coefficients enable us to develop a deeper and broader *understanding* of the use and properties of such formulas (compared for example with exploring the axiomatic performance of them or the uses made of them in official statistics for the purpose of price level measurement (P^L) on the one hand and deflation (in the case of P^P) on the other hand? I never heard an answer to such simple questions.

My impression is, that it is indeed most unlikely to gain a better understanding of an index formula like P_{0t}^L by referring to a regressor $\sqrt{w_{i0}}$ and it is possibly not by coincidence that there seems to be no "new" index formula owing its discovery to the regression-model-technique of the NSA, although possible models of this type should abound. The NSA seems at best suitable for interpreting an index formula *already known*.*

* Though I must admit that I also don't know of a good example where an interpretation in terms of a regression model actually made a known index formula appear more meaningful.

2. **Simply restating known index formulas**: For some index functions the NSA not even provides a *regression* model with explicit regressors ("explanatory" or "independent" variables). For example it can easily be seen that (the least squares $\{\Sigma\varepsilon^2 \rightarrow \min\}$ or maximum likelihood) estimator $\hat{\theta}$ in

(3) $\frac{p_{it}}{p_{i0}} = \theta_t + \varepsilon_{it}$ (commodities i = 1, … , n) represents the unweighted arithmetic mean of the price relatives y_{it}, known as price index formula of Carli

(3a) $\hat{\theta}_t = \bar{y} = \frac{1}{n}\sum_i \frac{p_{it}}{p_{i0}} = P_{0t}^C$.

For those acquainted with P_{0t}^C the only novelty[8] is that P_{0t}^C can also be viewed as $\hat{\theta}$. Upon substituting the so called "log changes"[9] Dp_{it} for the relatives p_{it}/p_{i0} in (3) we have

(4) $Dp_{it} = \ln(p_{it}/p_{i0}) = \theta_t^* + \varepsilon_{it}$

where θ* is said to represent *the logarithm of* the common (underlying, or "general") inflation rate which leads {with $N(0,\sigma^2)$ distributed error terms ε_{it}} to the least squares and maximum likelihood estimator $\exp(\hat{\theta}^*) = \prod_i (p_{it}/p_{i0})^{1/n} = P_{0t}^J$, an unweighted geometric mean index known as Jevons' index. This relationship between $\ln(P^J)$ and Carli's index P^C is obvious as $\ln\left(\prod_i (p_{it}/p_{it})^{1/n}\right) = \frac{1}{n}\sum \ln(p_{it}/p_{it}) = \frac{1}{n}\sum Dp_i$ (the P^C index of the Dp_i's).

3. **Old and new stochastic approach**: Again we may ask: Does this really improve our understanding of either or both indices, P^C and P^J ? However, we can go a step further, noticing, that this very result was already known in the days the "old" (or "unweighted") stochastic approach (OSA). Jevons and Edgeworth are commonly viewed as "founders" of the OSA in the 19th century. They both advocated with great vigour the *geometric* mean that is P^J by contrast to Laspeyres who vehemently defended P^C and the *arithmetic*

[8] It is questionable whether this really is a novelty as such an interpretation (the general inflation rate as an average of price relatives) was already quite popular in the Old Stochastic Approach (OSA) of the 19th century.

[9] The Dp_i's are the logarithms of the price relatives (ratios) p_{it}/p_{i0}. They can be viewed as a sort of growth *rates* r = $(y_t - y_{t-1})/y_{t-1}$ while all index numbers are growth *factor* f = $y_t/y_{t-1} = (y_t - y_{t-1})/y_{t-1} + 1 = r+1$. Note, however, that it is not θ* (but exp(θ*)) what is meant to be a price index.

mean (the most frequently used index formula in these days).[10] No mention then was given to any sort of sampling (at random or otherwise) of price observations in this dispute.

It is generally recognized that the fundamental (or "fatal"[11]) flaw of the OSA was the lack of weights. Also treating prices as independent observations and ignoring their connectivity or "connexity" was already an early criticism of the OSA.

While in our present paper regression models are at the heart of the NSA the focus of the NSA is in other expositions (e.g. the CPI Manual) rather on selection probabilities. So for example as reported in the CPI Manual Henri Theil arrived at the Törnqvist index P^T (also known as Törnqvist-Theil index) with the following thought experiment:

> "Suppose we draw price relatives at random in such a way that each dollar of expenditure in the base period has an equal chance of being selected. Then the probability that we will draw the *i*th price relative is equal to [s_{i0}] the period 0 share for commodity i . Then the overall mean (period 0 weighted) logarithmic price change is Σ $s_{i0}\ln(p_{i1}/p_{i0})$. Now repeat the above mental experiment… in period t …This leads to the overall mean…Σ $s_{i1}\ln(p_{i1}/p_{i0})$. Each of these measures …seems equally valid, so we could argue for taking a symmetric average of the two measures…" (and so we eventually arrive at weights $(s_{i0}+s_{i1})/2$ for P^T.[12])

Both features of the NSA, i.e. the characterization of (price) index formulas P_{0t}

- in terms of *selection probabilities* in a random sample of prices, and
- P_{0t} in terms (of an estimate) of a *regression coefficient*

have in common to conceptualize the data input for a price index as random variable.[13]

4. **Thinking in terms of samples and random variables**:
It is appropriate now to briefly state the merits and the demerits of the NSA models:

Thinking in terms of samples and random variables

benefits
1. It becomes possible to estimate a **confidence interval** (CI) where hitherto only a point estimate could be provided
2. As we can "test" the fit of a regression model, for example by referring to the coefficient of determination R^2, the NSA is (ostensibly) able to provide a **measure for the adequacy of an index formula**

costs
Some restrictive (and questionable assumptions have to be made explicitly or (implicitly), not only concerning the random "**disturbance**" but also the **variables** (data, i.e. prices or price relatives)* of the regression equations. They are randomly selected and the result of independent observations of the same "general inflation" rate **.

* it is left a bit vague what this implies for the probability distribution of the variables of the regression

** the price index is not viewed as an the underlying ("latent") variable of a model but simply as the regression coefficient β_k reflecting the influence of the regressor x_k.

Before going into details of NSA's (alleged) advantages we may state right at the outset:

[10] Even Laspeyres made much more use of this formula than of his own formula P^L, which (so it seems) was for him only a more or less unimportant alternative to P^C. What is now seen as a major "flaw" of P^C, viz. the absence of "weights" to reflect different "relative importance" of goods was not yet an issue in his days. For more historical details and the Jevons-Laspeyres controversy see P. von der Lippe, Recurrent Price Index Problems and Some Early German Papers on Index Numbers, Notes on Laspeyres, Paasche, Drobisch, and Lehr, Jahrbücher für Nationalökonomie und Statistik (Journal of Economics and Statistics), vol. 233/3 (2013), pp. 336-366.

[11] As the CPI Manual put it a bit exaggerated in §16.76. There also Keynes' (1930) criticism of missing weights was quoted at length his.

[12] §16.80f in the CPI Manual; see also § 16.85 for the analogous consideration for the Laspeyres index P^L:

[13] The question may arise: is this due to the error term ε_{it} in the model (that is errors in equation), or due to a selection probability of less than unity (that is the observations are gained from a random sample)?

In our view it is doubtful

1. that both "advantages", a CI and R^2 are really ***two** independent results* of the NSA stochastic-model-approach to index numbers, and
2. in which way an interval estimation, or a significance test of a parameter β or $R^2 \approx 1$ really provides a proof of the correctness of a certain index formula.

Both, the CI and R^2 result from the existence of a sampling distribution of an estimate $\hat{\beta}$ (put differently: due to $\hat{\sigma}_{\hat{\beta}} > 0$), and R^2 is not a measure of the degree to which a certain formula deserves to be preferred to another formula. To illustrate this it appears opportune to quote now what S+P wrote in the introductory chapter of their book:

> "The numerical value of an index function always reflects some uncertainty and it is the aim of the stochastic approach to measure the degree of reliability associated with this estimate. It is only the stochastic approach that addresses this question of the level of confidence to be attached to the value of the index… This approach deals with the variability of results due to different formulas, even though no sampling and no measurement inaccuracy is involved. The stochastic approach therefore concentrates on the search of formulas. Different index formulas will produce different results whenever the rate of change in prices is disproportionate. It is the reliability of the index construction in the sense that different formulas using the same data, and it is not the reliability of the data that enter into the formula and that in practice always is subject to sampling errors and measurement errors, which is the concern of the stochastic approach."

Interestingly a distinction is made here[14]

variability (of results) due to

data	***formulas***
sampling errors and measurement errors	different formulas using the same data

Only the data-type of variability is involved in computing a CI or R^2. Sizeable errors usually entail wide CIs and an R^2 not differing significantly from zero. It is absolutely strange why S+P now say that it is ***not*** "the reliability" of the data, "which is the concern" of their approach. However, in order to assess the suitability of an index formula we have to study the second kind of variability, that is we have to compute *different formulas using the same data*. This is not the business of CI estimation. It seems perhaps to be tacitly assumed by the NSA that results in such situations may give an indication of the comparative advantages or disadvantageous of index formulas. We will, however, soon see that not even this can hold water. Not only

- should *fitting the data* (random *variability of observations*) be kept distinct from adequacy, or even *correctness of an index formula*, also
- it is a truism that "Different index formulas will produce different results" (S+P as quoted above) *when applied to the same data* is, but such calculations will not give useful information (as regards "the reliability of the index construction") because for unspecified data (almost[15]) any results may be acceptable.

We may be able to say which formula provides a better fit (or the best fit of a given set of formulas) but the consequences would be awkward: it could turn out for example that a

[14] The NSA claims to provide *uno actu* a confidence interval CI ("sampling errors … is the concern of the stochastic approach") of an index and a measure of the appropriateness of an index formula (the "stochastic approach therefore concentrates on the search of formulas") apparently by means of R^2.

[15] For the numerical example in the appendix the result should be in accordance with the mean value property of an index, that is within the interval $1.08 \le P_{0t} \le 1.22$ (a condition not violated by any of the three indices above).

Laspeyres price index proves the most appropriate formula for a consumer price index in Poland given the time series of prices 2000 – 2010 in Poland while the empirical regression analysis shows that for an index of producer prices in the UK (using prices sampled in 2005 – 2014) a Paasche price index model would be the best choice. To decide on index formulas this way in practice would of course be ridiculous.

Now consider correctly computed *different formulas for* precisely *the same data* (which allegedly allow assessments of competing formulas). We think it leads to nowhere when simply numerical results of such index computations are compared. What consequences (or conclusions) can and should be drawn from the fact that $P_{0t}^{C} = 5.75/5 = 1.15$, $P_{0t}^{J} = 1.1487$ and $P_{0t}^{L} = 1.1489$ in the numerical example of the appendix (see p. 12 below). Is for example P_{0t}^{J} better than P_{0t}^{C} because it yields a lower inflation rate? From $P_{0t}^{J} \leq P_{0t}^{C}$ nothing follows as this is necessarily the case because for geometric ($\bar{x}_G$) and arithmetic means ($\bar{x}$) always $\bar{x}_G \leq \bar{x}$ holds. Or is it convincing to say – considering the tiny difference between P_{0t}^{J} and P_{0t}^{L} – that weights are irrelevant?

It is well known, that nothing can be deduced from numerical results of index formulas as such, and it is for that reason that in the axiomatic index theory only *if-then-statements* are used throughout: *if* no price changes ($p_{it}/p_{i0} = 1\ \forall i$) *then* the index should be $P_{0t} = 1$ as well. The "if" should be so restrictive that from it only one "meaningful" result follows.

As to the interpretation of CIs and R^2 we have two objections:

1. Sampling allows measuring the goodness of *fit* (accounting for random *variation of observations* by R^2 for example) but this is to be kept distinct from assessment of index functions. The former refers to data, the latter to measurement.
2. Even if restricted to the latter aspect: correctly applied different index formulas to the same data, this so called "*variability of results*" will not allow any inference in the ways of *adequacy* (correctness or appropriateness) *of an index* formula.*

* Put differently: From the simple fact that results of different index formulas when applied to the same data will be different nothing follows as regards the pros and cons of the respective index formulas.

Experience also shows that very different formulas may yield surprisingly similar numerical results (especially in the case of an only moderately increasing price level).[16]

5. **Random sample**: The stochastic approach (unlike the axiomatic and the "(micro) economic" approach - both deterministic) requires
   a) a specific way of collecting empirical price quotations (observations need to be generated by *random* samples [otherwise there is no point in compiling confidence intervals]), and
   b) a number of distributional assumptions that are notoriously susceptible to misunderstandings.[17]

We are going to deal with a) now and with b) in the next paragraph, that is §6.

Looking at the practice of official price statistics we may ask: Can we reasonably think of price relatives generated by a *random sample* of possible observations? From which population and which sampling frame the sample is drawn? What is in the case of price statistics the "population" and its size N? Is it a set of outlets, commodities, acts of purchas-

[16] I already became aware of this by a numerical examples in my book "Index Theory…", p. 86, and here in the numerical example of the appendix of this paper (p. 13) this occurred again. We also should ask: how can we decide over index formulas when their numerical results entail overlapping confidence intervals?
[17] Do they apply to the disturbance term or also to the y variable (or even to the x variable(s))?

es/sales, or prices?[18] What is the sample size n: is it the number of commodities and "their" prices (over which summation takes place in the traditional index formulas), or is it the usually much greater number of price quotations collected (in the well known index formulas it is usually assumed that each commodity is represented by only one price).[19]

Moreover as a CPI is compiled on a monthly base, the "stochastic" approach should in principle require a new random sample drawn every month? Assume among vegetables the sampling frame contains apples. So there should be months with apples, and those (as it is a *random* selection) without apples, those with shoes and those without shoes.
As aforesaid some models for index functions explicitly work with the notion of "selection probabilities": for example if good i is selected with probability $\frac{1}{2}(s_{i0} + s_{it})$ the index of Törnqvist will result. It must be quite difficult to establish a random selection process that guarantees exactly such very specific selection probabilities.

In view of all this the NSA seems to be at odds with the practice of official price statistics in most, if not all countries, where predominantly use is made of non-random selections. Furthermore The NSA not only requires unrealistic assumptions regarding random selections but also regarding the specification of its regression models.

6. **Assumptions concerning variables and the stochastic term**: The NSA clearly assumes the dependent variable y to be a *random variable* (due to the undisputed random nature of the "disturbance" term, u_{it} or ε_{it}). However, there is some vagueness when we ask: does this also imply y (or even x) to be *distributed according to a specified probability distribution*? In regression analysis it is infrequent to make assumptions concerning the distribution of empirical data (x variables and y variable). Also when in general a normal distribution $u_{it} \sim N(0, \sigma^2)$ is assumed it would be uncommon to refrain from running a regression $y_{it} = \alpha + \beta x_{it} + u_{it}$ only because y_{it} is not normally distributed. Hence assumptions referring to the *distribution of the disturbance terms* clearly impact on the distribution of the y-variable, but the exact nature of it is not spelled out in detail. In "applied" work on NSA therefore an awful lot of confusion seems to exist as to which distributional assumptions (if any) y- and/or the x-variable(s)[20] have to meet (aside those regarding the error term u_{it}) and why such assumptions are (ostensibly) so essential.

So we read for example that a model requires *the population* of prices p_{it} (or price relatives p_{it}/p_{i0}) to be distributed according the normal distribution or the log-normal distribution, without demonstrating why this allegedly ought to be so,[21] and saying anything about the consequences when the assumptions are not met. Anyway a regression equation requires some assumptions to be met referring to the function, variables and distributions:

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[18] In other words, sampling (selection) may refer to other units than the subsequent analysis, and also selection can be performed at random selection, or using other methods

[19] We refer here to the long neglected "low level aggregation" problem (which only recently gained attention), that is the simple (without weights) averaging of *a number of* price quotations for the same good. In practice it is only after this first, or "low" level, that formulas for (weighted) price indices come into play.

[20] Assumptions concerning the x-variable are usually not needed in "classical" regression analysis, as this variable can also be treated as "exogenous" (nonstochastic). The weights w_{i0} usually are considered fixed (for a more or less long interval in time), at least not random. So P^L is a linear transformation (in effect a weighted arithmetic mean with weights w_{i0}) of price relatives p_{it}/p_{i0}. We ignore here the case of "endogenous" regressors.

[21] As far as I know the log normal distribution is usually assumed for price *ratios* (= price *relatives*, i.e. "mere" dimensionless figures) not for absolute prices (expressed in €, $ or so). Anyhow to conceive prices arising from a random experiment is somewhat strange at first glance: quoting a price of 1€ for a bottle of water, or 5€ for a bottle of wine is different from casting a dice showing 1 or 5 respectively (with both events equally probable). Also a distribution of the population should be kept distinct from the sampling distribution. When the central limit theorem applies we can assume the sampling distribution of *an average of* the $y_i = p_{it}/p_{i0}$ to be asymptotically ($n \to \infty$) normal whichever the distribution of y_i's in the population may be.

- an equation like (2) for example is a correctly specified regression model, with no omitted/redundant variables, a correctly (in view of the population) specified functional form (e.g. linear) of the sample regression, and constant (over the observation period) regression coefficients (no breaks);
- the stochastic disturbance term u is generally assumed to meet $E(u_{it}) = 0$, $var(u_{it}) = \sigma^2_{u_{it}} = \sigma^2_u = \sigma^2, \forall i, t$ (homoscedasticity), no autocorrelation,[22] and
- the *regressor(s)* x_{it} (or the only regressor as in (1)) *need to be* "*exogenous*",[23] or with K > 1 regressors they must not be linear dependent on each other and should not be sizably correlated.

7. Another disturbing feature of the NSA is that **minor variations concerning the error term will yield quite different index formulas**: It is not surprising that a switch to another regression model in the sense of other regressors, or another functional form will result in different regression coefficients. However, in the framework of the NSA it happens that some tiny modifications regarding the error term in a regression only will make a great difference as to the index formula "explained" by the model in question.

   It is for example said in the relevant literature, that substituting $\xi_{it} = \varepsilon_{it} / \sqrt{\overline{w}_i}$ (an assumption obviously made so that $var(\xi_{it}) = var(\varepsilon_{it}) / \overline{w}_i$) for ε_{it} in the P^J model of eq. (4) will result in $\ln\left(P^T_{0t}\right) = \sum \overline{w}_i \ln(p_{it} / p_{i0})$, that is the logarithm of the Törnqvist's index instead of $\ln(P^J_{0t})$.[24]

   The puzzle now is not only the unduly large effect of a relatively small "cause" but rather

   - to give a plausible interpretation of the underlying relation between the magnitudes of $\left(\sqrt{\overline{w}_i}\right)^{-1}$ on the one hand and the dispersion (variance) of the dependent variable Dpi on the other hand. The challenge is also that
   - such an interpretation should be applicable to *all* goods included in the index.[25]

   Quoting Selvanathan and Prasada Rao, we see that it is precisely giving examples only, what they do (in S+P, p. 341) when they state that this "…implies that the variability of a relative price falls as the commodity becomes more important in the consumer's budget". So for example a "good having a large budget share such as food" yields a small variability while a good with a "smaller share such as cigarettes" should entail a larger variability in order for the Törnqvist model to be applicable.

   However, even if this might apply to all goods, that is whenever $\sqrt{\overline{w}_i}$ is large/small then the variability is small/large (so that always applies precisely what for to food/cigarettes applies) this of course could not give a sufficient justification for a switch from P^J or P^L to P^T. A choice among index functions has to be based on the properties of the respective index function (axioms met and axioms violated) and not on empirical observations.

   In § 4 we already emphasized the fundamental difference between fitting data and evaluation of a measurement instrument (for the latter peculiarities of given data are not relevant, otherwise a choice among methods of measurement would be situation- rather than principle-driven. To sum up:

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[22] Note that for estimating β–coefficients it is not necessary to assume u_{it} to be normally distributed, although this is convenient in order to make use of normal- or t-distribution tables for confidence interval estimation and hypothesis testing.

[23] Of course this is hardly the case with $(w_{i0})^{1/2}$ as the expenditure shares depend on prices p_{i0} just like p_{it}/p_{i0}.

[24] Again we may ask: what can we learn from this seemingly minor modification of the model (4) regarding the relationship and the conceptual difference between P^T and P^J.

[25] It is not enough to give some (carefully selected) examples in which the tacitly assumed relationship appears plausible. Moreover it ought to be the *same* postulated relationship (or function) for all goods of the index.

Why should a very specific assumption about the error term decide over the reasonableness of an index formula? It is difficult to accept the rationale of the fact that different assumptions (the motivation of which are not at all self evident) regarding the error term will result in different index formulas. Again it should be emphasized: description of data should be kept distinct from choice among measurement instruments.

Apart from assumptions concerning the (unobserved) error term it should be kept in mind that assumptions concerning the observed variables are already restrictive enough: so we already mentioned (in §3) the early criticism of the stochastic approach advanced by J. M. Keynes stating that it is not justified to consider each price relative p_{it}/p_{i0} or each log-change $\ln(p_{it}/p_{i0})$ a realization (and independent replication) of a random experiment[26] (let alone to postulate specific probability distribution for it). This may make sense in the case of a number of price quotations referring to essentially the *same* (constant in the passage of time) commodity collected e.g. in different outlets, at different places, or different points in time, but it definitely does not apply to *different* commodities, much less to *all* n goods and services normally included in a consumer (or other) price index.

8. **Sampling distribution and the usefulness of confidence intervals**: Returning to model (3) for Carli's index P^C it is clear that the model yields the (asymptotically normal) sampling distribution $N\left(\theta, \sigma_y / \sqrt{n}\right)$ for $\bar{y} = \hat{\theta} = \hat{P}^C$, with n as sample size,[27] and then a confidence interval (CI) is easily been calculated because the sampling distribution of $\hat{\theta} = \bar{y}$ is asymptotically normal with the standard deviation $\sigma_{\hat{\theta}} = \sigma_y / \sqrt{n}$.[28] With $\hat{\sigma}_{\hat{\theta}}$ at hand we already *have* an *interval estimation* with bounds $\hat{\theta} - z_\alpha \hat{\sigma}_{\hat{\theta}}$, and $\hat{\theta} + z_\alpha \hat{\sigma}_{\hat{\theta}}$ (with z_α for example 1.96 as $1-\alpha = 0.95$ percentile of the normal distribution). The point now only is:
   a) what is the benefit of being able to dispose of a confidence interval (CI), and
   b) do we need NSA in order to be able to compile a CI?

   Again it can be seen that the answer to both questions is not particularly in favour of the NSA. To begin with a) we see: From a practical point of view (again from the perspective of official price statistics) we may well question

   - Should we indeed communicate in official statistics as a routine an *interval* as *the* result of a monthly CPI statistic?[29] I actually doubt that the ordinary user of CPI statistics will see any benefit in a CI as opposed to the familiar point estimate (also monetary policy will find orientation in a specific inflation *rate* rather than a range of probable inflation rates). Also monetary policy will prefer a single inflation rate to a multitude of probable inflation rates.
   - By the same token we have in official statistics provisional, revised and final estimates of the GDP, but only a first (and also final) estimate with inflation rates.
   - Note that the bounds $\hat{\theta} \pm z\sigma_{\hat{\theta}}$ are *random* variables because $\hat{\theta}$ is a random variable. In all other parts of official statistics (for example in Population Surveys or National Accounts) it is totally uncommon to refer to randomness when an office comments on its results (even though of course all empirical observations are subject to measurement and other errors which may call for such intervals).

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[26] Or being a manifestation of an underlying general inflation rate θ, "disturbed" only by an error term.
[27] See above §5 for the problem on which grounds, if any, we can claim to have a "random sample" and what in particular is the sample size n in this case.
[28] The problem is to estimate σ_y, but this can easily be done with the sampled values $y_1, y_2, \ldots, y_n$.
[29] This of course not only applies to "inflation rates" as measured by a CPI, but to all other index numbers.

Furthermore the well known advantages of having a sampling distribution (and therefore a CI) will raise questions like:

- Can we actually increase the sample size[30] n in order to make the interval smaller and/or to obtain a better estimate $\hat{\sigma}_y$ in the case of a price index?
- What could be a reasonable (worth being tested) hypothesis concerning $\theta = P^C$ and on which ground can we distinguish for example a Carli index θ_0 (hypothesis H_0) and θ_1 (according to H_1) and specify acceptable (or desirable) levels of the corresponding error risks, α and β respectively.
- What is the percentage change of P^C from t to t+1 when we have at both times, t and in t+1 an *interval* (that is a *number* of figures rather than only one, $\hat{P}^C_{0,t}$ and $\hat{P}^C_{0,t+1}$ respectively, in which case the change simply is $\hat{P}^C_{0,t+1}/\hat{P}^C_{0t}$) of possible results for $\hat{P}^C_{0,t}$ as well as $\hat{P}^C_{0,t+1}$.

9. **Once we have a sample we can do without NSA models**: The major merit of this approach is often said to be able to provide an interval, not only a point estimate of a price index (as $\hat{P}^C_{0t}$ or $\hat{P}^L_{0t}$ for example). To derive a confidence interval (CI) clearly requires
   - a random sample of price relatives being drawn (as assumed in the NSA), and
   - a *sampling* distribution of an *estimator* like $\hat{P}$.

   The functional form of the sampling distribution of an estimate $\hat{P}$ of a price index and its parameters depend on the function of the price index P. What we need to know is this function, for example $P^L_{0t} = \sum_i w_{i0}\left(p_{it}/p_{i0}\right)$, a weighted arithmetic mean of price relatives ($\pi_i = p_{it}/p_{i0}$) with a fixed "basket" (constant expenditure weights $w_{i0} = p_{i0}q_{i0}/\Sigma p_{i0}q_{i0}$).[31] There is no need to know in the context of which regression model we have a regression coefficient β representing P^L_{0t}. It is not the regression model which allows (or only facilitates) the derivation of the exact function of the sampling distribution and thus also to estimate a CI, it's only the index function itself[32]. The funny thing now is

   Once we really have a random sample of price relatives and can make use of a not too complicated price index function* P of which the (asymptotical) sampling distribution is known we are prpared to compile a CI without knowing anything about NSA models that possibly might "explain" P. Ironically, once the assumptions of (random sample) are met, we need no NSA theory** in order to enjoy the advantage of getting an interval estimate.

   * It is unlikely that NSA can offer a model in such cases of very complicated index formulas.
   ** In general we also need no assumptions concerning distribution of data (*variables* such as prices or price relatives)

   So if n prices were in fact selected at random from N prices on a monthly basis in the case of a monthly Laspeyres CPI, we then have all we need in order to calculate a CI for such a price index. There is clearly no more any need for the NSA theory. Once we have a sample and the function of the sampling distribution of an estimate $\hat{\theta}$ of a parameter θ we will also have its standard deviation $\sigma_{\hat{\theta}}$, and hence are in a position to calculate a CI for θ. As a rule the sampling distribution of $\hat{\theta}$ depends somehow on the *observed* variability of the sample (given for example by the variance of price relatives π_i) and the sam-

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[30] We already pointed out (in §5) that it is anything but clear what n is in the case of index numbers

[31] Most of the other popular index functions are also simple functions of price relatives. And where this is not the case, for example with Fisher's index $P^F = (P^L P^P)^{1/2}$ it is again only the (now fairly complicated) index function which determines the sampling distribution (interestingly there seems to be no NSA regression model for P^F).

[32] And this function is always the same, viz. $\Sigma\pi_i w_{i0}$ whichever model we might bring into play.

ple size n. This also applies to all those estimates (functions) $\hat{\theta}$ that represent price index formulas[33]

10. **Price index as regressor or as dependent variable:** Our final argument is that the NSA makes use of regression in a highly unorthodox and not at all sensible way. The "usual" or predominant use made of regression analysis is:

    - the purpose of regression is to gain insights in (or to "explain") *data* y, x_1, x_2 ,…, for example in factors influencing prices (or a time series of price levels, measured by a sequence of price indices P_{01}, P_{02},…) as the "dependent" variable y (viewing y as generated by "explanatory", "independent" or "regressor" variables $x_1, x_2,\dots, x_K$); not in focusing on regression coefficients;
    - decisions concerning the choice of regressors (x-variables) are based on economic theory, or reasoned expectations gained from prior experiences;
    - it is requisite that there are no prepossessions as to the "results" of an empirical application of the regression method; though we are interested in regression coefficients β_k we should be and in fact are open to which statistical figures (numerical values) the data will yield for the $\hat{\beta}_k$ coefficients.

    By contrast in the NSA an entirely different use of the method is made: a model now is devised (or concocted) for the sole purpose of getting a specified function for a *regression coefficient*. The emphasis is clearly on the β coefficients, not on a description of the data. There is no theoretical (economic) foundation when the variables are selected that will constitute the model. It is unlikely that anybody can convincingly recourse to economic theory when the task is to explain for example why $x_{it} = \sqrt{w_{i0}}$ should be in any sense (causal or other) be related to $y_{it} = (p_{it}/p_{i0})\sqrt{w_{i0}}$, not to speak of how to motivate an error term like $u_{it} = \varepsilon_{it}\sqrt{w_{i0}}$.[34]

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[33] As we already pointed out it is anything but clear what n is in the case of index numbers. Moreover it is questionable that we have *in addition* to the CI an independent measure of goodness of fit. Or is the width (length) of the CI the required measure of goodness of fit? The claim to possess both, a CI and a measure of goodness of fit may be justified in a multiple-regression-model with R^2 (as indicator of goodness of fit) *and* K CIs for each regression coefficient $\beta_1,\dots, \beta_K$ (one of them representing an index function). In models with a simple regression (one regressor only) we only have as a rule a CI, and r^2 does not convey any information beyond the CI for β.

[34] The term u_{it} is specified in such a way that an estimation of the regression coefficients is rendered possible, not because by some reason the error u_{it} should be small/large when the expenditure share w_{i0} is small/large.

Appendix

a) "Expenditure based regression model"

The model in §1 above is called a "budget share weighted average" model by S+P. An alternative is the following "expenditure based regression model"

(A1) $p_{it}q_{i0} = \gamma_t p_{i0}q_{i0} + \varepsilon_i$, $(y_i = p_{it}q_{i0}, x_i = p_{i0}q_{i0})$

likewise considered by Selvanathan and Prasada Rao (S+P, p. 60). Again the sample estimate of γ_t (a "constant with respect to commodities, that is the same for all commodities") turns out to be P^L. Upon division of both sides of eq. (A1) by $(p_{it}q_{i0})^{1/2}$ we get $y_{it} = \gamma_t x_{it} + u_{it}$ where $y_{it} = p_{it}(q_{i0}/p_{i0})^{1/2}$, $x_{it} = (p_{i0}q_{i0})^{1/2}$, $u_{it} = \varepsilon_{it}/(p_{it}q_{i0})^{1/2}$, and $\text{var}(u_{it}) = \sigma_t^2$. Again least squares estimation of γ_t yields $\hat{\gamma}_t = P^L_{0t}$, and again one may well call in question the usefulness of such a model.[35]

There are also some strange consequences: on the one hand it follows from $\sum \hat{\varepsilon}_i = 0$ that

(A2) $\hat{\gamma}_t = \sum p_{it}q_{i0} / \sum p_{i0}q_{i0} = P^L_{0t}$, but on the other hand $\varepsilon_i \to \min$ gives

(A3) $$\frac{d\left(\sum \hat{\varepsilon}_i^2\right)}{d\gamma} = -\sum x_i y_i + \hat{\gamma}\sum x_1^2 = 0 \Rightarrow \hat{\gamma}_t^{(2)} = \sum p_{it}p_{i0}(q_{i0})^2 / \sum (p_{i0}q_{i0})^2 \neq P^L_{0t}.$$

b) Numerical example

The restricted model (homogenous regression)

w_{i0}	$x_i = (w_{i0})^{1/2}$	p_{it}/p_{i0}	y_i^*	$x_i y_i$	$\hat{y}_i$	$u_i = y_i - \hat{y}_i$	u_i^2
0,2	0,4473	1,2	0,5367	0,24	0,5138	0,02285	0,000522
0,35	0,5916	1,15	0,6804	0,4025	0,6797	0,00066	4,235E-07
0,15	0,3873	1,08	0,4183	0,162	0,4450	-0,02669	0,000712
0,12	0,3464	1,22	0,4226	0,1464	0,3980	0,02463	0,000607
0,18	0,4243	1,1	0,4667	0,198	0,4874	-0,02075	0,000430
Σ	2,1968		2,5246	P^L_{0t}=1,1489	2,5239	0,00070	0,002272

*$y_i = x_i (p_{it}/p_{i0})$

$\Sigma u_i = 0{,}00070178 \neq 0$, $\Sigma(u_i)^2 = 0{,}00227179$, and because $\Sigma u_i \neq 0$ we also have $\hat{y}_i$ (they differ by .[36]

*The unrestricted model (now $\alpha \neq 0$ is possible*e)

$$\hat{\alpha}_L = \begin{vmatrix} \sum y & \sum x \\ \sum xy & \sum x^2 \end{vmatrix} \Big/ \begin{vmatrix} n & \sum x \\ \sum x & \sum x^2 \end{vmatrix} = 0{,}00070178/0{,}17409551 = 0{,}00403099 \text{ and } \hat{\alpha}_L$$

$$\hat{\beta}_L^* = \begin{vmatrix} n & \sum y \\ \sum x & \sum xy \end{vmatrix} \Big/ \begin{vmatrix} n & \sum x \\ \sum x & \sum x^2 \end{vmatrix} = 0{,}19847667/0{,}17409551 = 1{,}14004474$$

which is quite similar to the result of the homogeneous regression P^L_{0t} = 1,1489. We also get

$r^2 = R^2$	0.018866	coefficient of determination
r	0.137356	coefficient of correlation

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[35] Not only the dependent variable $y_{it} = p_{it}(q_{i0}/p_{i0})^{1/2}$, also a quotient $(p_{it}q_{i0})^{1/2}$ in u_{it} explaining a random variation of the y_{it} seems strange. The model of S+R (p. 60) for P^P is $p_{it}q_{it} = \gamma_t p_{i0}q_{it} + \eta_{it}$ is a model in which observable expenditures ($p_{it}q_{it}$) are "explained" by fictitious constant-prices expenditures (or "volumes" $p_{i0}q_{it}$) and we see that again the "message" of the model heavily depends on assumptions concerning the random variable η_{it}. $\neq \varepsilon_{it}$.

[36] The difference between the two figures equals Σu_i.

y_i	$\hat{y}_i$	u_i^*	u_i^{*2}
0,5367	0,5139	0,02278	0,000519
0,6804	0,6785	0,00186	3,45E-06
0,4183	0,4456	-0,02729	0,000745
0,4226	0,3990	0,02367	0,000560
0,4667	0,4877	-0,02102	0,000442
2,5246	2,5246	1,4E-14*	0,002269

Note the difference between Σu_i^* (actually 0; this is because now $\Sigma \hat{y}_i \neq \Sigma y_i$ and $\Sigma u_i = 0{,}00070178$ above. However, $\sum u_i^{*2}$ is not much different from $\Sigma u^2 = 0{,}002272$. To derive the CI we calculate $\hat{\sigma}^2_{\hat{\beta}^*_L} = \frac{\sum u_i^{*2}/(n-2)}{\sum (x_i - \bar{x})^2} = \frac{0.000756}{0.806964}$ $= 0.00093724$, hence $\hat{\sigma}_{\hat{\beta}^*_L} = 0.0306$, so that the 95% CI ($z = \pm 1.96$) is the interval is (1.08004; 1.20005) which is tantamount to an increase of the price level between 8% and 20%. Because $t = (\hat{\beta} - 1)/\hat{\sigma}_{\hat{\beta}} = 4{,}57 > 1.96$ we have a significant inflation (can reject the null: $\beta = 1$). For the Carli index we get $\hat{\sigma}_{\bar{y}} = \hat{\sigma}_y / \sqrt{n} = 0.02433$ so that the 95% CI is (1.1023; 1.19769), thus smaller than the P^L- CI. Nonetheless perhaps nobody will now conclude that P^C is better than P^L).[37] Note that the variance of the weights is $0.2328/5 - (0.2)^2 = 0.00656$

c) Variant of the numerical example

In what follows we modify the weights (increasing their variance by the factor 5.15)

former weights	0.2	0.35	0.15	0.12	0.18	variance 0.00656
new weights	0.3	0.5	0.15	0.02	0.03	variance 0.03276

These changes will not affect Carli's index and its CI, but it does have an effect on

	P^L_{0t}	$\hat{\beta}^*_L$	$\hat{\sigma}_{\hat{\beta}^*_L}$	95% CI for β^*_L	$\hat{\alpha}_L$
now	1.1544	1.166255	0.0251	1.11713; 1.21538	-0.006058
before	1.1489	1,140045	0.0306	1.08004; 1.20005	0,004031

Remember the CI of Carli's index was (1.1023; 1.19769): although now the point estimates of P^L and $P^C = 1.15$ are closer to one another the interval estimate of PC is no longer fully embedded in the CI of P^L. The fit of the modified model is obviously totally unsatisfactory

$r^2 = R^2$	0.004080	r	0.063875

but would this justify to recommend P^L in the original model and to reject it for the data of the modified model? Even after having made a modification of the example so that both indices become more dissimilar (by increasing the variance of the weights) it is now still

- not at all easy to differentiate (in terms of which should be preferred over the other) between P^C an P^L from an empirical point of view,
- though the modification produced a much smaller r^2 for the unrestricted model (β*), the smaller $\hat{\sigma}_{\bar{y}}$ for $\bar{y} = \hat{\beta}^*$ seems to indicate a better fit (smaller CI).

Hence the alleged usefulness of the variability of numerical results for different formulas applied to the same data seems to be illusory, and all this might suffice to demonstrate that the NSA is – in our view at least – indeed of very limited practical value. It is a misconception.

[37] P^C of course continues to be 1.15 which is significantly higher than unity (t = 6.165). Furthermore the P^C-CI is fully within the borders of the P^L-CI, one interval is "nested" in the other, so that there is little reason to infer that the the formulas (P^L and P^C respectively) yield different results although the variance of the weights w_{i0} increased to $0.3638/5 - (0.2)^2 = 0.03276$ (compared to 0.00636 before) which should make a difference.