Are Kraichnan's papers difficult to read? Part 1: Galilean Invariance

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When I was first at Edinburgh, in the early 1970s, I gave some informal talks on turbulence theory. One of my colleagues became sufficiently interested to start doing some reading on the subject. Shortly afterwards he came up to me at coffee time and said. 'Are all Kraichnan's papers as difficult to understand as this one?' The paper which he was brandishing at me was Kraichnan's seminal 1959 paper which launched the direct interaction approximation (DIA) [1]. I had to admit that Kraichnan's papers were in general pretty difficult to read; and I think that my colleague gave up on the idea. Shortly afterwards, Leslie's book came out and this was very largely devoted to making Kraichnan's work more accessible [2]; but I think that was too late for one disillusioned individual.

Recently I was reading a paper (might have been one of Kraichnan's) and I was brought up short by something like '… *and the variance takes the form:*' followed by a displayed mathematical expression. So it was rather like one half of an equation, with the other (first) half being in words in the text. So, I found that I had to remember what the variance was in this particular context, and then complete the equation in my mind. If I had been writing this, I would have used a symbol for the variance (even if just its definition as $\langle u^2 \rangle$) and displayed an actual equation. But what this reminded me of was my own diagnosis of the difficulty with Kraichnan's style. I suspected that he would get tired of always writing in maths, and would feel the need for some variety. The trouble was that sometimes he would put the important bits in words, with a corresponding loss of conciseness and precision. As a result there was a temptation to rely on secondary sources such as Leslie’s book [2] or Orszag’s review article [3]; and I was by no means the only one to succumb to this temptation!

The fact that it could be unwise to do so emerged when we produced a paper on calculations of the LET theory (compared with DIA) and submitted it to the JFM [4]. We discussed the idea of random Galilean invariance (RGI) and argued that its averaging process violated the ergodic principle.

We set out the procedure of random Galilean transformation as follows. Consider a velocity field $\mathbf{u}(\mathbf{x},t)$ in a frame of reference S. Suppose that we have a set of reference frames $\{S_0,\,S_1,\,S_2,\, \dots\}$, moving with velocities $\{C_0,\,C_1,\,C_2,\,\dots\}$, where the shift velocities are all constant and the sub-ensemble is defined by the probability distribution $P(C)$ of the shift velocities. In practice, Kraichnan took this to be a normal or Gaussian distribution, and averaged with respect to C as well as with respect to the velocity field.

However, Kraichnan’s response to our paper was ‘*that’s not what I mean by random Galilean transformations*’. But he didn’t enlighten us any further on the matter.

Around that time, a new research student started, and I asked him to go through Kraichan’s papers with the proverbial fine-tooth comb and find out what RGI really was. What he found was that Kraichnan was working with a composite ensemble made up from the members of the turbulent ensemble, each shifted randomly by a constant velocity. So the turbulence ensemble $\{\mathbf{u}^{i}(\mathbf{x},t)\}$, with the superscript i taking integer values, was replaced by a composite ensemble $\{\mathbf{u}^{i}(\mathbf{x},t) + C_i\}$. This had to be inferred from a brief statement in words in a paper by

Kraichnan!

The student then investigated this choice of RGT in conjunction with the derivation of theories and concluded that it was incompatible with the use of renormalized perturbation theory. In other words, Kraichnan was using it as a constraint of theory, once the theory was actually derived. But in fact the underlying use of the composite ensemble invalidated the actual derivation of the theory. It would be too complicated to go further into this matter here, but a full account can be found in Section 10.4 of my book [5], which references Mark Filipiak's thesis [6].

This experience illustrates the danger of relying too much on secondary sources, however excellent they may be. I will give another example in my next post but I can round this one off with an anecdote. When I first met Bob Kraichnan he told me that he had been very angered by Leslie's book. I think that he was unhappy at what he saw as an excessive concentration on his work, and also the fact that Leslie had dedicated the book to him. However, he said that various others had persuaded him that he was wrong to react in this way. I added my own voice to this chorus, pointing out that there was absolutely no doubt of his dominance as the father of modern turbulence theory; and the dedication was no more than a personal expression of admiration on the part of David Leslie.

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[3] S. A. Orszag. Analytical theories of turbulence. J. Fluid Mech., 41:363, 1970.
[4] W. D. McComb, V. Shanmugasundaram, and P. Hutchinson. Velocity derivative skewness and two-time velocity correlations of isotropic turbulence as predicted by the LET theory. J. Fluid Mech., 208:91, 1989.
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