The genesis of the *Non-Linear Field Theories of Mechanics*

by Walter Noll, July 2002*

Clifford Truesdell was a singularity among all prominent scientist-scholars of the twentieth century. He believed that the pinnacle of civilization had been reached in the 18th century and that things have gone downhill ever since. He had no television set and no radio, and I doubt that he ever used a typewriter, let alone a computer. Many of the letters he sent me were written with a quill pen. (However, he did not reject such modern conveniences as flush toilets and air-conditioning.) He loved baroque music and did not care very much for what was composed later on. He owned very fine harpsichords and often invited masters of the instrument to play, often before a large audience, in his large home in Baltimore, which he called the "Palazetto". He collected art and antique furniture, mostly from the 18th century. He even often dressed in the manner of an 18th century gentleman. Most importantly, he admired the scientists of the 18th century and, above all, Leonhard Euler, whom he considered to be the greatest mathematician of all time. Actually, in the 18th century, mathematics and physics were not the separate specialties they are today, and the term "Natural Philosophy" was the term then used for the endeavor to understand nature by using mathematics as a conceptual tool. C.T. tried and succeeded to some extent in reviving this term. That is why this meeting is a *Meeting of the Society for Natural Philosophy.*

Clifford Truesdell was extremely prolific and he worked very hard most of the time. When he didn't, he liked to eat well and drink good wine. This is perhaps one reason why he loved Italy so much, and spent extended periods of time there. Clifford Truesdell was not only an eminent scientist but also a superb scholar. He mastered Latin perfectly. He could not only read and understand the classical literature, which was written in Latin, but even wrote at least one paper in Latin. He was fluent not only in his native language, English, but also in French, German, and, above all, Italian. He wrote papers and gave lectures in all of these languages. All of these attributes are particularly rare for somebody born in Los Angeles, California.

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Clifford Truesdell was only in his early 30s in the early 1950s when he had already established himself as perhaps the world's best informed person in the field of continuum mechanics, which included not only the classical theories of fluid mechanics and elasticity but also newer attempts to mathematically describe the mechanical behavior of materials for which the classical theories were inadequate. In 1952, he published a 175 page account entitled "The Mechanical Foundations of Elasticity and Fluid Dynamics" in the first volume of the *Journal of Rational Mechanics and Analysis.*

The Springer-Verlag, with Headquarters in Heidelberg, Germany, has been an important publisher of scientific literature since the beginning of the 20th century. In the 1920s it published the *Handbuch der Physik*, a multi-volume encyclopedia of all the then known knowledge in Physics, mostly written in German. By the 1950s the *Handbuch* had become hopelessly obsolete, and the Springer Verlag decided to publish a new version, written mostly in English and called "The Encyclopedia of Physics". Here is Clifford Truesdell's own account, in a 1988 editorial in the *Archive for Rational Mechanics and Analysis*, of how he became involved in this enterprise:

"In 1952 Siegfried Flügge and his wife Charlotte were organizing and carrying through the press of Springer-Verlag the vast, new *Encyclopedia of Physics (Handbuch der Physik)*, an undertaking that was to last for decades. Joseph Meixner in that year had called Flügge's attention to my article *The mechanical foundations of elasticity and fluid dynamics...* On October 1, 1952, Flügge invited me to compose for the *Encyclopedia* a more extensive article along the same lines but presenting also the elementary aspects of continuum mechanics, which my paper ... had presumed to be already known by the reader. In the second week of June 1953, Flügge spent some days with us in Bloomington, mainly discussing problems of getting suitable articles on fluid dynamics for the Encyclopedia. ... He asked me to advise him, and by the end of his visit I had agreed become co-editor of the two volumes under discussion."

It was C.T., in his capacity as co-editor, who invited James Serrin to write the basic article on *Fluid Mechanics*. C.T. himself decided to make two contributions dealing with the foundations of continuum mechanics, the first to be called "The Classical Field Theories of Mechanics" and the second "The Non-Linear Field Theories of Mechanics". Here is what he wrote about this decision, in the preface to a 1962 reprint of his Mechanical Foundations:

"By September, 1953, I had agreed to write, in collaboration with others, a new exposition for Flügge's *Encyclopedia of Physics*. It was to include everything in *The*

Mechanical Foundations, supplemented by fuller development of the field equations and their properties in general; emphasis was to be put on principles of invariance and their representations; but the plan was much the same. As the work went into 1954, the researches of Rivlin & Ericksen and of Noll made the underlying division into fluid and elastic phenomena, indicated by the very title *The Mechanical Foundations of Elasticity and Fluid Dynamics* no longer a natural one. It was decided to split the projected article in two parts, one on the general principles, and one on constitutive equations. The former part, with the collaboration of Mr. Toupin and Mr. Ericksen, was completed and published in 1960 ... In over 600 pages it gives the full material sketches in Chapters II and III of *The Mechanical Foundations*. The second article, to be called "The Non-linear Field Theories of Mechanics", Mr. Noll and I are presently engaged in writing. The rush of fine new work has twice caused us to change our basic plan and rewrite almost everything. The volume of grand and enlightening discoveries since 1955 has made us set aside the attempt at complete exposition of older things. It seems better now to let *The Mechanical Foundations* stand as final for most of the material included in it, and to regard the new article not only as beginning from a deeper and sounder basis than could have been reached in 1949 but also as leaving behind it certain types of investigation that no longer seem fruitful, no matter what help they may have provided when new."

In 1951 I started employment as a "Wissenschaftlicher Assistent" (scientific assistant) at an Institute of Engineering Mechanics (Lehrstuhl für Technische Mechanik) at the Technical University of Berlin. In late 1952 I came across a leaflet from Indiana University offering "research assistantships" at the Graduate Institute of Mathematics and Mechanics. I applied and was accepted to start there in the Fall of 1953. After arriving, I found out that I had become a "graduate student". At first I did not know what that meant, because German universities have nothing corresponding to it. But the situation was better than I expected, because no work was assigned to me, and I could pursue work towards a Ph.D. degree full time.

I asked Clifford Truesdell to be my thesis advisor, because he was the local expert on mechanics and I thought that would help me after my return to Germany. At the time I did not know that C.T. had already became the global expert on mechanics. I also did not know at the time that I was C.T.'s first doctoral student. I was told later that other graduate students did not select him as a thesis advisor because they thought that he was too tough. It is true that he was tough and asked much of me, but he was also extremely kind and helpful, and, in the end, he had more influence on my professional career than any other person. I learned from him not only the basic principles and the important open questions of mechanics, but also how to write clear English. In addition, his wife Charlotte and he did very much to make me feel comfortable in Bloomington. They invited me to their home very frequently to share the gourmet dinners prepared by Charlotte. As a thesis topic, C. T. gave me a paper written by S. Zaremba in 1903 and asked me to make sense of it. I found that, in order to do so, a general principle was needed, which I called "The Principle of Isotropy of Space". This principle also served to clarify many assertions in mechanics that had been obscure before (at least to me). C. T. had a wonderful sense of humor. One day he put up his son, then about 10 years old, to ask me: "Mr. Noll, please explain the Principle of Isotropy of Space to me". Actually, I think I have found a good explanation only recently, about 40 years later. I had a very good background in the concepts of coordinate-free linear algebra and used it in my thesis. At the time, C.T. was uncomfortable with this approach and forced me to add a coordinate version to all formulas. However, he did not have a closed mind and later, in a letter to me dated August 4, 1958, he wrote:

"I must also admit that the direct notations you use are better suited to fundamental questions than are indicial notations. Your present mathematical style is smoother and simpler than that in your thesis."

After I received my Ph.D. in September 1954 and returned to Germany, C.T. and I stayed in contact by mail, and I saw him again when he gave a lecture in Berlin in June 1955 ("Das ungelöste Hauptproblem der endlichen Elastizitatstheorie"). The term of my position in Berlin ran out in the Fall of 1955 and I needed a new job. On C.T.'s recommendation I was offered an Associate Professorship at Carnegie Institute of Technology (now Carnegie Mellon University), where I moved in the Fall of 1956 after having spent a year at the University of Southern California. Here is an excerpt from a letter he wrote to me on February 14, 1956, after I accepted the position at Carnegie Tech:

"I think you deserve your position at Carnegie and hope you will like it. If I might offer a word of advice, it would be to maintain the level set by your two papers in the *Journal*. Nowadays so many people are publishing like rabbits that volumes of papers make little impression. Many young people publish too much when they find out how easy it is to do something others have not done, especially something a senior colleague is too dull to see. It is of course necessary to avoid setting such high standards that one's publication ceases entirely, as is the case with many well known savants."

Later, in December of the same year, he wrote me the following letter:

"Dear Noll,

Ericksen and I have long promised to write a second article, *The non-linear field theories of mechanics*, to cover exact work on elastic, fluid, and plastic phenomena.

Ericksen is far behind on the first article, and therefore we have not even outlined this second one. Although I have not yet consulted Ericksen, I feel sure that he would be relieved if you would consent to join us and do a major part of the second article. The order of authors would be according to the amount of work provided, and if you could do most of the work, you would be the senior author.

You know both the old *Mechanical Foundations* and the new *Classical Field Theories* thoroughly. Therefore you have as much background as anyone else, and you have made important contributions yourself. I have not been able to think of a good organization. All I can say is that I should prefer to cut down on the amount of space given to special proposals prior to1948 and to emphasize general work such as you and Rivlin have done. Whether it is better to treat very general visco-elastic theories first or to give a definitive presentation of classical finite elasticity first is not clear to me. The article should be exhaustive, but for the work prior to1948 I think we need only condense the *Mechanical Foundations* or change the emphasis, as I believe the list of sources cited there is virtually complete. The presentation should be concise, but the length is not critical.

If this proposal is acceptable to you, I will put it up to Ericksen at once. Also, if you could start right away on the organization and suggest a distribution of material among the three authors it would help. I believe I can begin active work within two months, but I do not expect to be able to prosecute exclusively this one project in the near future."

Here is an excerpt from my answer, written on Jan. 2, 1957:

"I shall be glad to accept your proposal and join you and Ericksen as co-author of "the non-linear field theories of mechanics". It will be a challenging task for me, but I shall do the best I can. I have thought a little bit about the organization. Here is very roughly what I would like to propose:

The first section should contain general remarks concerning constitutive equations, their classification, invariance requirements, constraints, definitions of terms like "material", "isotropic", "aelotropic", etc. I have been thinking about this subject for quite a while, and I plan to include my ideas on it in a paper on the axiomatic foundation of mechanics, which I plan to write in the near future.

I think it is best to give then an account of finite elasticity and non-linear fluid theory (Reiner-Rivlin). These are the simplest and best known non-linear theories. Next, more general stress-strain relations (as those proposed by Rivlin and Ericksen) could be treated. Finally, constitutive equations involving stress rates (as those proposed by Oldroyd, Cotter and Rivlin, and in my thesis) and their special cases (hypo-elasticity) could be worked out, and the connection of these general theories with the simpler ones could be established.

I would be willing to do the general remarks and also most of the treatment of the various constitutive equations in general. However, I would appreciate some help with the special cases and solutions. I would think, for instance, that Ericksen would have little trouble to present all special solutions valid for an arbitrary strain energy function in finite elasticity by taking his two papers (ZAMP 5, 466-489 and J.Math.Phys. 2,126-128) as a basis. Also, it is probably easier for one of you to deal with finite deformations of shells, anisotropic elastic bodies, and similar material. I would think, too, that you would want to do the section on hypo-elasticity yourself.

I am open to alternative and more detailed proposals concerning the organization as well as the distribution of the material to the authors, and I am looking forward to have your opinion."

The new task of working on the *Non-Linear Field Theories of Mechanics* (NLFT) changed my professional life forever, because it focussed my attention on the foundations of continuum mechanics. C.T. was very good at reading, digesting, and summarizing existing literature. I am not. I cannot help rethinking and recreating whatever subject I wish to understand. This had perhaps the effect of improving the NLFT in the end, but it also slowed down the progress towards completion. Sometimes C.T. became very impatient with me, and with good reason. The low point was reached when C.T wrote the following letter to me:

"Dear Walter,

Upon my return from the West, I did not find any manuscript of our article. As I said in Pittsburgh, I feel now that you should withdraw from the article. This is the third period of half a year or more in which you have not sent me a line, and again the subject is slipping away from us. My having had to disturb you every few weeks or days for the past six months so as to get your assurance that a large section of manuscript would be ready in a few days has been very painful, the more so since fruitless. I have put in this treatise the better part of all my work in the past five years, and as I prepare for another trip to Europe, I cannot drag this weight of responsibility any longer.

I plan to finish the article by intensive work in January and February, and I request you to send me all material I gave you before I went to Europe last spring.

Surely, you know that I feel this loss deeply. The parts written by you are far better than anything I could write, and your criticism and correction of the parts I wrote have been of the highest value. There is no one else who could do the job you are capable of doing, but you have not done it, and some sort of article must be published nevertheless. There is too much invested in it already."

Well, I pulled myself together and I helped to finish NLFT.

During our collaboration we were in constant contact by mail or in person. I visited Bloomington in November 1958. C.T. came to Pittsburgh in June 1959 and gave lectures at the Mellon Institute (later a part of Carnegie Mellon University). In August 1961, C.T. left Indiana University and he and Charlotte moved to Baltimore where he joined the Johns Hopkins University with the title Professor of Rational Mechanics. In September 1961, C.T. visited Pittsburgh again. During the academic year 1962/63, C.T. arranged for me to come to Johns Hopkins University as a visiting professor.

Occasionally, C.T. and I had disagreements on terminology. Perhaps the most interesting concerned what we finally called "The Principle of Material Frame-Indifference". Here are some excerpts from letters that we exchanged on this subject:

C.T. to W.N. , August 1, 1958

"I was just planning to write something mentioning your old "isotropy of space". It seems to me the term "principle of objectivity" shares one of the undesirable features of the old term, namely, it is too vague. I was going to use "the principle of material indifference". However, it would be bad for there to be three names running around for the same principle. If you find my suggestion appealing, let me know right away; otherwise, I will use your present name."

W.N. to C.T. , August 5, 1958:

"I have switched from "isotropy of space" to "objectivity" because the former suggests only that there are no preferred directions in static space, which is much less than is implied by the principle of objectivity.

If "objectivity" means independence of the observer, as I believe it does, then it seems to me that it is the correct term. One could remove some of the vagueness you mentioned by using "principle of objectivity of material properties" (or perhaps, "principle of material objectivity"). If you think that this is advisable, please insert "of material properties" after "principle of objectivity" in the table of contents (section 11), in line 2 of page 3, in the title of section 11 on page 20, and in line 15 from the bottom of mage 21. I think that no additional changes are necessary because the other references to the "principle of objectivity" cannot lead to confusion."

C.T. to W.N., August 9, 1958 (handwritten):

"Dear Noll,

I have to confess that my dislike of "objective" is subjective. Psychologists, sociologists, and all sorts of other charlatans and fakers use "objective" when the only sense I can find for what they claim is "nonsensical" or "thoughtless". Thus I exclude, banish and ostracize "objective" as the opposite of "subjective" in my own usage, but of course there is no reason why this should influence you.

Cordial regards, CT"

C.T.'s objection did influence me and I am now glad it did. I suggested that we change *indifference* to *frame-indifference*. C.T. accepted that and I believe now that the term we finally used expresses the idea better than any of the ones used before.

The entire task of writing the NLFT turned out more extensive than was expected, and C.T. called it the "monsterino". (He called the earlier

Classical Field Theories the "monster".) The *monsterino* was finally published in 1965. There were many people who helped us with the work by reading parts of the manuscript and offering corrections, critique, and suggestions. They include B.D.Coleman, J. Ericksen, M.E. Gurtin, R. Toupin, K. Zoller, C.C. Wang, D.C. Leigh, C.C. Hsiao, W. Jauzemis, and A.J.A. Morgan. On June 25, 1961 C.T. wrote to me: "I now have an excellent secretary, so that the preparation of the manuscript should be easier." That helped, too.

Clifford Truesdell was very meticulous about attributions. I believe he bent over backwards in my favor when he described, in a footnote to the Introduction to NLFT, how the work was divided between us:

"Acknowledgment. This treatise, while it covers the entire domain indicated by its title, emphasizes the reorganization of classical mechanics by Noll and his associates. He laid down the outline followed here and wrote the first drafts of most sections in Chaps. B, C, and E and of a few in Chap. D. Among the places where he has given new results not published elsewhere, shorter proofs, or major simplifications of older ideas may be mentioned (21 items). The larger part of the text was written by Truesdell, who also took the major share in searching the literature. While Noll revised many of the sections drafted by Truesdell, it is the latter who prepared the final text and must take responsibility for such oversights, crudities, and errors as may remain."

The Non-Linear Field Theories was reprinted in 1992. Here is an excerpt from the preface to the second edition:

"This volume is a second, corrected edition of *The Non-Linear Field Theories of Mechanics*, which first appeared as Volume III/3 of the *Encyclopaedia of Physics*, 1965. Its principal aims were to replace the conceptual, terminological, and notational chaos that existed in the literature of the field by at least a modicum of order and coherence, and second, to describe, or at least to summarize, everything that was both known and worth knowing in the field at the time. Inspecting the literature that has appeared since then, we conclude that the first aim was achieved to some degree. Many of the concepts, terms, and notations we introduced have become more or less standard, and thus communication among researchers in the field has been eased. On the other hand, some ill-chosen terms are still current. Examples are the use of "configuration" and "deformation" for what we should have called, and now call, "placement" and "transplacement", respectively. (To classify translations and rotations as deformations clashes too severely with the dictionary meaning of the latter.) We believe that the second aim was largely achieved also. We have found little published before 1965 that should have been included in the treatise but was not. However, a large amount of relevant literature has appeared since 1965, some of it important. As a result, were the treatise to be written today, it should be very different. On p. 12 of the Introduction we stated "... we have subordinated detail to importance and, above all, clarity and finality". We believe now that finality is much more elusive than it seemed at the time. The *General theory of* *material behavior* presented in Chapter C, although still useful, can no longer be regarded as the final word. *The Principle of Determinism for the Stress* stated on p. 56 has only limited scope. It should be replaced by a more inclusive principle, using the concept of *state* rather than a history of infinite duration, as a basic ingredient. In fact, forcing the theory of *materials of the rate type* into the general framework of the treatise as is done on p. 95 must now be regarded as artificial at best, and unworkable in general. This difficulty was alluded to in footnote 1 on p. 98 and in the discussion of B. Bernstein's concept of a *material* on p. 405. This major conceptual issue was first resolved in 1972 ["A New Mathematical Theory of Simple Materials" by W. Noll, *Archive for Rational Mechanics and Analysis*, Vol. 48, pp. 1-50], and then only for simple materials. The new concept of material makes it possible, also, to include theories of *plasticity* in the general framework, and one can now do much more than "refer the reader to the standard treatises", as we suggested on p. 11 of the Introduction.

About three years ago, the Springer-Verlag sold the right to translate NLFT into Chinese. I recently received copies of the Translation.[1]

[1] After this paper was written, I was informed by the Springer-Verlag that a second reprinting will appear in 2003.