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Ceriotti G. A microchemical determination of desoxyribonucleic acid. *J. Biol Chem.* **198**:297-303, 1952; and Determination of nucleic acids in animal tissues. *J. Biol. Chem.* **214**:59-70, 1955.
[Div Exp. Chemotherapy, Sloan-Kettering Inst. for Cancer Res., New York, NY]

The deoxyribose of the purine moiety of DNA is hydrolyzed and made to react with indole by heating in HCl The color produced is purified by extraction of interfering products by chloroform Similarly, the ribose of the purine moiety of RNA is hydrolyzed and made to react with orcinol by heating for 40 minutes in HCl in the presence of copper chloride The color produced is purified and concentrated by extraction with iso-amyl alcohol To determine nucleic acid, tissues are first extracted with dilute perchloric acid in the cold, to eliminate possibly interfering substances The color reactions for both DNA and RNA are then performed on a subsequent extract made by 10 percent perchloric acid at 70°C [The *SCI*® indicates that these papers have been cited in over 905 and 1,180 publications, respectively, since 1955]

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Although my papers on DNA and RNA determination appeared in 1952 and 1955, respectively, my work had started long before in 1942. I was then working in bacteriology, and I was interested in the possibility of providing evidence for the existence of nucleic acids in bacteria, in possibly identifying them, and in studying their significance in bacterial growth. Knowledge about nucleic acids was rather scant in those days, and even the presence of DNA (then called thymusnucleic acid) in bacteria was questioned.

Then, in early 1943, I went to Vienna to the laboratory of the Biochemisches Institut, which was directed by Barrenscheen, who had worked on a modification of the Bial reaction for ribose and on purines and pyrimidines. My only reference books were Levene's manual on nucleic acids, Abderhalden's *Biochemisches Handlexikon*, and Beilstein. While learning microchemical techniques, I started investigating color reactions for pentose and hexose sugars, both free and bound.

Political events made me go back to Italy in a hurry, but I continued my research at the Institute of Physiology in Pavia, although under very difficult conditions. My first efforts were concentrated on DNA. I started from a publication by Thomas,[1] who described a reaction for DNA with tryptophan, but I was not able to replicate his results; I therefore tried other indole compounds. Indole, diskatole, and methylindole gave positive results, but I continued my research only on indole for obvious reasons. In the spring of 1944 the reaction was roughly done on a "thymusnucleinsaure" sample found on a shelf at the institute. However, the work was interrupted again by political events and then by lack of instruments. I had to wait for one year to get a lamp for a Pulfrich! When I got it, I resumed my work on the indole reaction, increasing its specificity by extraction procedures, and I started work on RNA to unify the hydrolysis conditions for the various ribose compounds and to increase the assay's sensitivity. However, I could do this only part-time, as I was employed in a pharmaceutical firm. Finally, in 1949, at the Sloan-Kettering Institute, I had both the time and instruments (a Beckman DU spectrophotometer) to complete my work on a satisfactory quantitative basis. There I also worked on techniques for extraction of nucleic acids from tissues to improve the specificity of the determinations. The work could not be finished in the US, and, back in Italy, its completion was hindered by the need to solve many urgent practical problems and by difficulties generated by insufficiently pure solvents. All these today almost incomprehensible difficulties and, maybe, my inborn meticulousness in writing, retarded the publication of my findings. Apparently they did not appear too late, since they have been useful to so many people in the many different areas of biology where nucleic acids have to be determined.

The importance of the subject, the fair sensitivity of the assay, and the handiness and reliability of these methods, confirmed by Wiener *et al.*,[2] are, in my opinion, the reasons for their wide acceptance. And this is the best reward for all the difficulties encountered and surmounted.

1 Thomas P. Farbreaktionen der Nucleinsäuren. *Hoppe-Seylers Z Physiol Chem* 199:10-12, 1931.
2 Wiener S L, Urivetzky M, Lendvai S, Shafer S & Mellman E. The indole method for determination of DNA condition for maximal sensitivity *Anal Biochem* 71 579-82, 1976 (Cited 2 times)