Physical Origins of Residual Stress and Present Physical Techniques for Measurement

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Abstract
The classic technique for a nondestructive measurement of residual stress is the use of X-ray diffraction. It is really quite an old method. There is a classic paper by Norton and Rosenthal written back in 1943. The method was used during the second World War, I believe, in connection with the study of welded structures in ships. A more or less portable piece of equipment was built even in those days. The method did not come, as far as I know, into very widespread use. I think I now understand this better after hearing yesterday’s talks. It seems clear that a nondestructive testing or evaluation method must be cheap, quick and simple; the early X-ray techniques were none of those. They depended on the use of photographic film. The exposure had to be made; the film had to be developed. In many cases it had to be measured with a microdensitometer. It required a rather skilled person to do it, so it was not really a very attractive procedure.

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The talk I am going to give is a little different from the title since the previous speaker has quite thoroughly covered the subject of the origin of residual stresses. I will confine my talk to the problem of attempting to measure them in a nondestructive fashion.

The classic technique for a nondestructive measurement of residual stress is the use of X-ray diffraction. It is really quite an old method. There is a classic paper by Norton and Rosenthal written back in 1943. The method was used during the second World War, I believe, in connection with the study of welded structures in ships. A more or less portable piece of equipment was built even in those days. The method did not come, as far as I know, into very widespread use. I think I now understand this better after hearing yesterday's talks. It seems clear that a nondestructive testing or evaluation method must be cheap, quick and simple; the early X-ray techniques were none of those. They depended on the use of photographic film. The exposure had to be made; the film had to be developed. In many cases it had to be measured with a microdensitometer. It required a rather skilled person to do it, so it was not really a very attractive procedure.

In the fifties, there was a rather large resurgence of interest in the method because counter diffractometry became available and many people realized that with the use of counter diffractometry, the method could be made at least faster, cheaper and simpler. The idea of the method is really quite simple. One does not measure the stress, one measures the strain and infers the stress on the basis of, hopefully, known elastic properties of the material. The idea is that one makes at least two X-ray measurements on the sample. We make one measurement with Bragg scattering
from some microcrystallites in the material whose diffracting planes are parallel to the surface, and then another measurement for which we change the geometry to a very steep incidence to the surface and scatter from a different set of crystals in the material. We require a fine grain, polycrystalline material. The penetration of an X-ray beam is very small. One is looking at a very thin surface layer, and so the normal stress is essentially zero. The first measurement gives the interplanar spacing characteristic of the material in its unstressed condition. The second measurement determines the interplanar spacing in a direction close to that of maximum stress. From these two measurements, we can determine the stress in one direction. If we make similar measurements in various directions around the material, we can determine the surface stress distribution. This work is summarized very well in an SAE report. The displacement between the two peak positions for the two measurements is of the same order of magnitude as the width of the diffraction peak. This is one factor which made the film technique so unpleasant, the fact that it is very hard to measure the change in position of a line on a film when the change is only comparable to the width of the line. Only a very few minutes is required for these measurements and I believe there is some new, very fast apparatus on the market which can make such measurements in a matter of seconds, rather than minutes.

One obvious question is: "Do you believe what you get?" This can be investigated by a comparison of an X-ray measurement with a more traditional destructive measurement in which you remove material and observe the deflection due to the removal of material that had residual stress in it. In such measurements first the surface is examined, then a layer is removed, and the deflection measured and the new surface measured, and so on. The agreement is very good, and under favorable circumstances, one can measure residual stresses with an accuracy of about 5,000 psi in a very few minutes. I suspect the limiting time is probably the time required to position the apparatus on the sample.
There are, however, some difficulties. Of key concern is the matter of microstresses and pseudo macrostresses. One makes the measurements of the lattice parameter at a particular angle, not in all of the crystals of the sample, but only in a very few of them which are oriented correctly for the Bragg conditions for scattering. In an ideal isotropic material, these would be a representative sample, but in many cases they are not. Various kinds of inhomogeneities may exist in the material. In particular, in material which has been plastically deformed, there is a very large range of microstresses. The lattice parameters in some small regions are very different from the average value. This produces much of the line broadening referred to earlier resulting in measurements that are not on the line as a whole. What one measures is the position of the central region, which means one is sampling only those crystals which have been relatively little deformed, and these, as a result of complicated texture effects, do not always represent the bulk stress conditions. There is another case of pseudo-macroscopic stress which can occur in a two-phase material. If one has a two-phase material and studies the lattice parameter only in one of the phases, there may be a systematic stress in the other phase. What is really a microstress appears to be a macrostress.

There are also a few other catches to the X-ray technique. It is difficult to apply to cast materials. We have made the assumption that there are a large number of very small crystals available for our X-ray measurements, and in a cast material there are not. There can also be geometrical difficulties. You may be unable to get your counters in the necessary positions in order to make the measurements at almost glancing angles.

The situation is not hopeless. One can determine a fairly accurate value for residual stress by making measurements at many angles rather than just two and then one can, by using a theory worked out by Marion and Cohen, determine the true macroscopic stress.
There is some interest in an alternative technique for measurements of residual stress. Collins of the University of Texas has been working for some years on an alternative method making use of the Mössbauer effect. Before going into this further, I would like to discuss the fundamental physics of the Mössbauer effect and the way in which it might conceivably be used as an alternative to the X-ray technique for the measurement of residual stress.

The Mössbauer effect in iron depends on $^{57}\text{Fe}$, which is one of the stable isotopes of iron. It is present in ordinary iron to about 2 1/2 per cent, which is adequate for the Mössbauer measurements. Nuclei, like atoms, have excited states, and transitions between these states are associated with emission or absorption of electro-magnetic radiation. In the case of atoms, the radiation is light or it is X-rays; in the case of nuclei it is called gamma radiation. It happens that $^{57}\text{Fe}$ has a first excited state of 14 kilovolts. This corresponds to radiation with a wavelength of a little under one angstrom in the ordinary X-ray range, but called gamma radiation because it originates in the nuclear transition rather than the atomic transition. We can get to this state by the decay of an isotope of cobalt.

One of the characteristics of these nuclear transitions is that the resonances are extremely sharp. The Q is something close to $10^{12}$, so although the energy is 14 kilovolts, one needs change it only by nano-electron volts to move in and out of resonance. That is normally accomplished in Mössbauer apparatus by a Doppler shift. If you move the source relative to the absorber at rather slow velocities, a few millimeters a second, one can scan through the resonances. This would be of only academic interest except for the fact that the surroundings of the iron atom containing the $^{57}\text{Fe}$ nucleus have a very small but measurable effect on the energy levels. A change in the iron environment changes the resonances, changes the spectrum, and enables us to study changes in the environment of iron. There are many other isotopes for which the Mössbauer effect can be observed, but iron is the nicest. From the point of view of a metallurgist that is a very convenient accident.
One of the important changes in the energy levels with the environment results from the interaction of the charge distribution within the nucleus. Nuclei have a finite size and, in this case, the excited state is a little bit smaller than the ground state; the interaction of the nucleus with the electron distribution in the atom produces a change of the energy levels. If one changes the chemical environment of iron, one gets a shift in the position at which resonance occurs. Also, if one changes the electronic situation simply by pressure, one changes the electronic density at the nucleus slightly. This can be part of the basis for a strain measurement; one measures, unfortunately, only the hydrostatic strain in this way. One does not get the directional information that one gets from an X-ray measurement.

Another effect of great use is the fact that in a magnetic material there is a Zeeman splitting of the nuclear levels, and instead of having just one possible transition, there are six. Again, there is an effect of pressure. If one changes the pressure and, therefore, the volume, one changes, again, the electronic density at the nucleus. This produces a change in the magnetic field of the nucleus and, therefore, a very small change in the splitting of the pattern. It is on these two effects that the measurements of Collins are based. The difficulty is that the effects are extremely small. It was bad enough in the X-ray case where the peaks moved by an amount comparable to the linewidth. In the Mössbauer case, the peaks move only about one or two per cent of their width, so one has the problem of measuring the position of peaks to an accuracy about two orders of magnitude better than their width. This is not impossible, but it is very difficult. The technique Collins used for doing this was to make use of temperature change to shift peak position. Changing the temperature of the source of the radioactive material produces some subtle changes in the nuclear energy levels of about the right order of magnitude. By measuring the change in the peak intensities as he changed the temperature of the source, he was able to get an effect dependent upon strain. The accuracy obtained is about \( \pm 10,000 \text{ psi} \), and the time required for measurement is of the order of half an hour.
At the moment, in cases in which the X-ray method is even remotely applicable, it seems it would be very hard for the Mössbauer technique to compete. There is, however, some slim possibility of getting some useful information about residual stresses with the Mössbauer effect by a somewhat different technique. The relative heights and areas of peaks in a hyperfine split pattern depend on the orientation of the magnetic domains in the material, and preferred domain orientation do correlate with the kind of deformation which produces residual stresses. In this approach one is now not looking for a very small shift in peak position, but a rather large shift in peak amplitude. It would not be necessary to determine the entire spectrum in this technique but simply a matter of making counting rate measurements at the positions of the two peaks. This is a method which is, in principle, fast and relatively cheap, but it is not clear how reliable it is. The result seems to depend not merely on what the residual stress pattern is, but how it was produced. It might be of some value in a quality control sort of measurement in which all the pieces supposedly had the same history and, therefore, should all give the same pattern. If they don't, it is an indication of something wrong even though you may not know exactly what went wrong. Except for such purposes, I am not terribly optimistic about the future of the Mössbauer measurements for residual stress measurements. I might put in a small plug that I think it has considerable future for other sorts of measurements. It is, for example, a very good way of measuring retained austenite or for other nondestructive analyses of multi-phase mixtures in materials containing iron.

I should emphasize again that this technique is essentially limited to iron-containing materials. There are other isotopes that show the Mössbauer effect, but none of them are of real metallurgical importance except, possibly, tin.

Thank you.
References:


DISCUSSION

DR. HARRIS MARCUS (Science Center, Rockwell International): Paul, isn't the fact that iron is basically the only really usable isotope also a very large limiting factor in using the Mössbauer effect for NDE for residual stresses?

DR. FLINN: Except for the fact that by far the main tonnage of practically all materials are iron-based alloys.

DR. MARCUS: Except in the airframe business.

DR. FLINN: Yes, it is not going to do you much good with titanium.

DR. MARCUS: Or aluminum.

DR. FLINN: Or aluminum, but it is usable for the ferromagnetic materials or, in principle, the nonmagnetic stainless. Again, one would get the isomer shift effect.

I should also like to take advantage of this time to mention something I should have said in the talk itself. Another catch with effects based on small shifts in peak position is that small changes in chemical composition produce changes in both the peak position and in the magnetic field and, in fact, the kind of tolerances which one has in commercial alloys cover composition ranges which would pretty much blanket the effect of residual stress measurements. In many cases unless you could measure the same peaks before and after, it would be very risky to try and use the Collins technique for residual stress measurements. I don't want to say it is hopeless, but I don't certainly regard it as very likely to displace X-rays. X-ray methods work on essentially anything, not merely metallic materials. You can use them on reasonably fine-grain ceramics.

DR. GERALD GARDNER (Southwest Research Institute): One question on the apparatus you used. It wasn't clear to me whether it was your apparatus or that of Collins.
DR. FLINN: The apparatus I used was mine. The apparatus Collins used was quite different because he was trying to measure these very small shifts, and so instead of a mechanical drive, he simply used a physically fixed source and changed the temperature.

DR. HENDRIKUS VANDERVELDT (Naval Ship Research & Development Center): Two comments I would like to make. First of all, I am a little surprised that you didn't mention the hole-drilling technique as the way to get residual stresses.

DR. FLINN: I will comment on that. It is not nondestructive. That's why I didn't mention it.

DR. VANDERVELDT: That is true, but it is a technique that is used for measuring residual stresses and from the title, I gathered that is what you were after. You didn't indicate it wasn't going to be nondestructive. The second comment I think is more important. As I understand it, the difference between the experimental results and the theoretical results using the X-ray technique is less than one percent.

DR. FLINN: Oh, yes.

DR. VANDERVELDT: So, therefore, it would seem to me that that difference is insignificant compared to, for example, the effects of machining. Milling operations will produce residual stresses in the surface layer or grinding will that are far in excess of less than a percent.

DR. FLINN: Certainly. Those are the residual stresses that one is often interested in measuring. Now certainly the changes in lattice parameters are less than a percent because one is measuring basically strains and the residual strains you could get have to be below, essentially, the yield strain of the material. So one is typically measuring things down to $10^{-4}$, but it is precisely for studying things like grinding, residual stresses due to grinding, or machining, that the X-ray method is useful.
DR. VANDERVELDT: Well, for example, you have in the case of welding, residual stresses in which no machining is involved. That is a place where, for the Navy, it is exceedingly important to determine residual stresses. As you know, machining or removal of the surface layer is not always possible, and yet you can't use a rough surface.

DR. FLINN: I am not, by any means, an expert in that area. My impression is that the technique used is electropolishing in order to obtain a reasonably good surface in the case of studying welding. For a while there was even a worry as to whether electropolishing produced a stress-free surface. Now, I am aware that there is a serious problem in studying welds because the method is a measurement of the surface stress, and in welding you are concerned with things that are going on deep below the surface. On the other hand, for people who are interested in fatigue properties, it is precisely the surface condition that they are interested in. And this, I believe, is the reason for the considerable effort in the automotive field.

DR. DENNIS CORBLY (Air Force Systems Command, WPAFB): In the summary of residual stress measurement methods, I wondered why ultrasonic stress velocity measurements were not included?

DR. FLINN: Quite honestly, I don't know anything about them.

DR. LAWRENCE DE VRIES (University of Utah): I want to ask you a question here. You mentioned that the tensile strength was not changed. That is true just for ductile materials, is that not right? Glasses and materials like this are drastically affected by the residual stress?

DR. FLINN: That's right. I might amplify that. You have to specify completely brittle materials.

DR. DeVRIES: Yes, brittle in the sense of glass.

DR. FRANKLIN ALEX (COAMA, Hill Air Force Base): I have a question here. It has been brought up that the residual stresses, of course, affect
hydrogen embrittlement. Has anybody measured whether there are any residual stresses induced by embrittlement?

DR. GARDNER: I can answer that. Studies have been made which are at the lattice deformation level, that is to say, people have actually studied deformation used in the presence of hydrogen in lattices using magnetic resonance techniques.

DR. WILLIAM SCOTT (Naval Air Development Center): I would like to address my question to Paul Flinn. I wonder if you would comment on the selection of values for Young's modulus in determining residual stresses in many materials such as iron-based alloys and copper-based alloys? Since you are measuring along a single crystal orientation, sometimes you may have factors of two between the Young's moduli, in various directions, and it is very often nontrivial to select what value you should use for Young's modulus. I have had examples of people coming up with a 34,000 psi difference in residual stress because of the selection.

DR. FLINN: That is a very good point. I have a couple of comments. One is that I think it is very important to make clear that one is measuring strain and calculating stress. I think in many cases the engineer might concentrate on the fact that he really is being given a strain number. The other point is that the traditional thing to do, if at all possible, is to obtain a sample of at least roughly the same material, put a strain gauge on it, and deform it with a known stress and make X-ray measurements on a sample which has known stresses in it.

DR. SCOTT: Well, this sometimes doesn't work for things like shot-peened materials where you have a texture in a very thin layer and it may actually have an effective modulus. It may not even be isotropic.

DR. FLINN: It certainly wouldn't be isotropic, and again, I don't know a good answer to that one. It is certainly a good problem.
DR. GARDNER: Jim Gubernatis is working on some of the problems connected with how you go about correctly producing nominal bulk properties from materials in which you have to take into account the stochastic variation. I wonder if he has any remark that he would like to make about this problem of deriving an effective Young’s modulus when there is a lot of texturing and, in addition, the lattice is strained?

DR. JAMES GUBERNATIS (Cornell University): Probably the only remark I can make would be that when you incorporate texture, the simple averages for anisotropic materials using single grain values don't really give you good results. I am currently trying to see how well various techniques which are proper to solid-state theory can be applied to the problem of calculating the effect of elastic modulus of materials whose single grain values are highly anisotropic and whose bulk properties are also anisotropic. The basic statistical idea is the same in each case. The principal difficulty is mainly computational.

DR. GARDNER: We will be anxious to find out what results you have in getting that done because Bill Scott’s points are extremely well-taken. The inference of stress from a measurement of the lattice strain is simply not straightforward, and you do get radically different answers of the order of 30, 40, 50 thousand pounds per square inch, depending on what you think the texturing and/or the preferred grain orientation happens to be, and also what you happen to think is the particular way in which the grains fit together.

DR. WILLIAM WALKER (AFOSR): I would like to address this question to Dr. Ebert, and that is with respect to residual stresses and the utilization of residual stresses in locating flaws and with respect to the concept of proof-loading structures in the nondestructive evaluation.

PROF. LYNN EBERT (Case Western University): Well, I really don't know very much about the use of ultrasonics in residual stress determinations. I have a feeling that they rely on the elastic properties
of the material to some degree. Again, the anisotropic nature of
the material could be a hindrance. I know of no work being done or
published, at least, in that area. There is, perhaps, a possibility.
I mentioned there is no change in modulus because of those stresses.
I was talking about the average modulus in the limits of engineering
need in the use of plus or minus half a million psi that is adequate
here. Now, there are ultrasonic techniques and others that will
discriminate locally in these modulus changes. That is a possi-
bility, very definitely. Proof-testing is an area that I have done
a little bit with. Actually it turns out to be in the composite
area and it turns out further that you can actually get a big benefit
in raising the modulus in the elastic limit of oriented fiber com-
posite material by proof-testing, proof-loading it. It turns out
that in a closely packed material, 50 volume per cent and up, you
have a state of residual tractual tension in the matrix. Now, you
cannot remove that no matter what. But you can change the relative
intensity by proof-loading, prestraining it slightly, a thousandths of
an inch per inch. In so doing, you change the relative intensity.
In some cases, the sign of the stresses can be changed. Now, what
happens is that you do produce a small amount of plastic load in
the matrix because the matrix is at the point of yielding. The
radial stresses are high. While the fibers are very brittle, they
also have a very high strength, which means the fracture strain is
relatively high and of the order of two or three per cent. We
published a paper on this, incidentally, in the A.S.M.E. As a
matter of fact, Howard Hamilton of Rockwell International, B-1
Division, did a large part of the work. That is a kind of proof-
test or proof-loading, and in this one particular case, at least,
had a very marked beneficial effect, and it is also predictable.
We can predict how much the benefit will be with great accuracy,
completely analytically.