

DISCUSSION SESSIONS

The material in this section was taken from tape recordings of discussion periods which were held at the conclusion of the Theory, Practice and Application Sessions with all the speakers in the session forming a panel to respond to questions and comments. Such questions as were asked directly after individual contributed papers, and their answers, are presented in the appropriate discussion section. Since a list of all participants and their affiliations is given at the outset, the respondents are identified only by name in the discussions. Each respondent was given the opportunity to review his comments prior to publication. Such changes as were made were primarily grammatical or for clarification of intent. The content is substantially unchanged. Editor's notes are also given where it is thought to benefit the discussion.

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1. THEORY SESSION

F. MAYER: I think the only speaker who addressed himself on theoretical grounds to the difference between what should be observed on (111) versus (100) material is Steve Fonash. I am interested to know what the other members of the panel expect from theoretical considerations of an orientation dependence.

W. SCHROEN: The multilayer analysis is independent of crystalline orientation. It considers only potential theory. However, I do not exclude that there could well be influences of the crystalline orientation and a complete theory would include crystalline orientation, stress effects, specific contact resistance, and other effects mentioned this morning. The pure potential theory neglects all these.

R. MAZUR: I do not want to leave the question hanging with some of you who are not so familiar with the question of crystallographic orientation but I have gone on record in the past as indicating that I have never seen a significant orientation dependence. Now I get disagreement there from all of IBM combined with all of Bell Labs and a few other places to boot. I may have to retract on this statement in the future; that is possible. I have not had the time to do extensive research with different orientation samples. Bernie Morris did supply me with a couple of wafers about six months ago and I have not had a chance to run them yet believe it or not. But the orientation dependence may, in many practical cases, be so small as to be unobservable from a practical point of view. I see it as a case of the jury still being out.

F. VIEWEG-GUTBERLET: We have had the same experience showing orientation dependence as Bernie Morris, and this is based on over 100 measurements. We got the same curves as shown this morning by Morris. We feel there is a dependence on orientation.

M. POPONIAK: In the last paper today [P-8], I will present some data showing the influence of probe loading on the orientation or I should say the orientation effect due the probe loading. We do see a difference and it is a function of probe loading. The lighter the loading the more sensitive it is to the orientation effect.

F. VIEWEG-GUTBERLET: Does that mean that with constant probe loading you will get another effect of impressions depending on the crystal orientation, i.e., if you set your probe loading with respect to crystal orientation, you should not get a difference for (111) or (100) material.

M. POPONIAK: What I am saying is that due to the atomic structure between (111) and (100) and their mechanical properties, there is a critical level of stress from which we get excessive fracturing, and that seems to be the problem.

K. BENSON: Steve Fonash this morning gave some numbers, about a factor of 10 I think for p-type (100) versus (111) and I would like to hear from anybody here that has made a calibration curve, does this number of a factor of 10 seem realistic?

S. FONASH: I forgot to mention [T-1] the fact that the zero bias resistance also in the contacts will depend on stress and I have a reference in my paper on that: Kramer and van Reuyven did some experimental work on this and showed that the zero bias resistance of the contacts does depend on stress also. I think this is rather well known but that is one particular reference. There is another paper in which I made some comments on this which appeared in Journal of Applied Physics in January of 1974. This effect also depends on crystallographic orientation so you have an interplay of those two effects both of which depend on crystallographic orientation. So regarding the factor of 10 which was brought up first there is this interplay of two effects which may change the factor of 10. Secondly, it is based on data that were determined at lower stress levels and I do not think the silicon is behaving elastically underneath these contacts and so I question the use of that data. I am just trying to show the existence of an effect which I think we better take a look at it.

H. MURRMANN: We will present [P-5] in the afternoon experimental data showing a difference between 100 and 111 material. We tried to split the data into mechanical phenomena including the contact radius and other physical phenomena. I would like to reintroduce a ρ dependent correction factor that Bob Mazur dropped some hours ago, [I-3] because I feel there is real physical reason for it.

R. MAZUR: I would like to make a comment here, Dr. Murrmann. I mentioned this to Bernie Morris at the coffee break. I regret mentioning the $K(\rho)$ factor in the way I did because it was misleading. We did not really drop the physical model involved in that $K(\rho)$ function. I was referring to the fact that we had worked out a way to avoid using cumbersome mathematics in making the corrections that get involved in having to plot an extra function: $K(\rho)$. You can treat it through the calibration data but the physical situation is still the same and I would agree with you on that.

J. KORVEMAKER: I would like to know if any of you have any results on other materials than silicon which has been discussed all morning.

R. MAZUR: There is data of course in the literature. I began making measurements on germanium. There is also data in the literature on germanium from T. L. Chu. Not in the scientific literature, but in Solid State Technology; that is a testimonial to the lack of clout of spreading resistance measurements several years ago. Ting Chu was unable to get it accepted by the prestigious journals, but it is good data and it is available in Solid State Technology of several years ago. We at Westinghouse, years ago, did make spreading resistance measurements on silicon carbide. There is a real question about spreading resistance measurements on gallium arsenide. There are ways to do it; whether it is easy or not may depend on a lot of things that would involve more discussion. That is about the size of what I know about it.

K. BENSON: There is also Witt and Gatos' paper looking at inhomogeneities in indium antimonide crystals. I believe spreading resistance was quite easy to use because this material was low resistivity.

B. MORRIS: I would say that probably in general as the band gap gets larger the measurement is going to become more difficult as in the case of gallium arsenide where you have a problem with four point probe measurements. From what I can see there should not be an appreciable problem on materials whose band gaps are similar to silicon or perhaps a little greater. If you can make four point probe measurements you should be able to make spreading resistance measurements.

P. LANGER: I would like to find out what the panel thinks is the lower limit in terms of the thickness to probe radius ratio to which one can extend the correction factor calculations on opposite conductivity type structures.

B. MORRIS: As I mentioned [T-5] we use a minimum value of 0.1 for t/a . This is somewhat arbitrary. I would not say that this minimum value of t/a is all that good. I do not know of any really good independent methods of experimentally verifying it. These methods would be the only ones I would believe. I would not tend to believe any calculations that close to the junction. Unfortunately the differential sheet resistance measurement has a lot of problems that close to the junction. I would be very interested in finding another method which would work in this region. The ion microprobe of course will go right through the junction but then you have first of all the chemical analysis technique and, instead of the net carrier concentration, you would have to subtract, say, the boron from the phosphorus profile and those readings as absolute numbers are not terribly good with the values that you usually get near a p/n junction for most microprobe analysis.

R. MAZUR: Paul should clarify whether he is asking the question of how close you can come to a junction with a good corrected value or whether he is asking the question of how thin a layer you can get a good corrected value on? That is, measurement on a beveled surface, or on top of a thin surface. They are not the same question and it sounds to me that he asked one question and Bernie answered another. On the subject of this layer measurement, Gil Gruber's paper which comes up tomorrow, I believe, will carry good evidence of quite reasonable results on certain sub-fractional micron layers in the range of 3,000 angstroms.

N. GOLDSMITH: I would like to add a comment to that. In my paper [A-7] on the last day of the session I will show an experimental model which you can use to test correction factors very close to an insulating boundary.

E. GARDNER: I heard the Schumann and Gardner paper mentioned quite often this morning so I think I should stand up at least and say hello. When Paul and I wrote the paper about 8 years ago we recognized a number of limitations in the spreading resistance technique and we tried to identify a number of them in our paper. I think many people today are recognizing those same limitations and going beyond our work and that was originally part of the intention of the work, to interest people in developing correction factors for the spreading resistance operation. It seems to me there are still a number of problems that have not really been identified and one is that we, in our analysis, did a finite layer type of structure in which we assumed a finite number of layers whether it was 2 or 40 and if we had been smarter we thought that there should be some way you could do this, not on finite, but on a continuous basis. That is problem #1. The second problem is one that people have alluded to and that is that you do not have a single contact but you have many contacts and how do you develop correction factors for many contacts? The third problem which is related to this, is, how can one explain the spatial resolution that you get whenever you assume that you have a 5 micron contact but you really show curves in which your resolution is 500 angstroms, or as some I saw this past week in which they were getting 50 angstroms resolution. So those are some of the problems I think that still remain with spreading resistance.

B. MORRIS: I have an answer to the last comment on how you can get the resolution that we do get. This is, I believe, due to the fact that instead of having one big flat contact for which one could very nicely calculate a correction factor from theory, we have a large number of small contacts, each of whose diameter is perhaps 1/10 micron, as opposed to 4 or 5 microns for the "flat" radius. Since the theory predicts a depth resolution of the order of magnitude of your radius, this allows us to get this very fine resolution in depth, while the effective radius of four microns is still the value that you get from the calibration as defined by values you measure for ρ and R . Thus, on one hand the micro-contacts add together so you can make real measurements, and on the other hand, you are also fortunate in that, for fine resolution, you may take an average value of the depth, a weighted average is more likely, and while this cannot be calculated it lets us make some very fine resolution measurements. I would say that from measurements with the Mazur probe using 45 gram loading that the physical penetration of the damage is approximately 1/10 of a micron. I cannot measure any better than that and perhaps this is an absolute limit as to how fine you can make the measurement.

J. ASSOUR: I have seen this morning several spreading resistance profiles through thin epitaxial layers and nobody said how these were produced in terms of surface polishing or lapping. I was wondering since we are worrying about correction factors and accuracy, how reproducible are the results in terms of surface finishing?

W. SCHROEN: You will hear data this afternoon [P-6] on this very question of how to prepare the sample surface and the probe tip, how reproducible the data are, and what we need to do to get those very shallow bevels.

P. SEVERIN: May I comment on this question invoking the micro-contacts, which I talked about in the Silicon Device Processing Symposium¹ in 1970. With our steel probe the micro-contact contribution, $\rho/4na$ (where n is the number of contacts which act in parallel to each other) acts in series with the term $\rho/4A$, where A is the total contact diameter. The micro-contacts represent about 5 times as large a resistance in the series combination as the large overall spreading resistance contact. In this case the resolving power of say 0.1 micron is easily explainable. That we have indeed these micro-contacts and that this has nothing to do with contact resistance in the common physical sense is explained by the fact that we find proportionality to ρ over 4 orders of magnitude which is a very unrealistic assumption for a contact resistance. Furthermore, micro-contacts are an extremely interesting subject because there the mechanics and physics enter. Although the term micro-cracking has been used today several times, direct evidence has been given in Russian literature^{2, 3, 4} that silicon behaves plastically at room temperature in a very shallow top layer. The thickness of this top layer increases with the applied load. Indirect evidence has been given earlier in various experiments of a different nature,^{5, 6, 7, 8}. I think that it is a pity that there are no people in this audience who have more experience on micromechanical properties. On a submicron scale you cannot just talk about the elastic

constant or hardness of silicon.

Ed. Note: In review of his comments, P. Severin supplied the following references.

1. Silicon Device Processing, NBS Spec. Pub. 337 (1970) 224.
2. V.P. Alekhin, O.V. Gusev, et al. - Sov. Phys. Dokl. 14, 894 (1970).
3. V.P. Alekhin, O.V. Gusev, et al. - Sov. Phys. Dokl. 14, 917 (1970).
4. V.J. Nitenko, M.M. Myshlyaev and V.G. Eremenko - Sov. Phys. Sol. St. 9, 2047 (1968).
5. G.R. Booker and R. Stickler - Phil. Mag. 8, 859 (1963).
6. T.R. Wilshaw - J. Phys. D. 4, 1567 (1971).
7. M. Renninger - J. Appl. Cryst. 5, 163 (1972).
8. W. Ridner and J. Braun - J. Appl. Phys. 34, 1958 (1963).

R. MAZUR: I would like to answer - this is also to that last question. We cannot leave it said that way. I mean that I cannot just sit here and leave it in the record that Mazur did not say anything. Regarding the third part of Ed Gardner's comment I need to point out, of course, to be consistent, that the very high spatial resolution can be explained in many cases again because of the work function potential barrier which has a very, very shallow penetration. It is restricted to the immediate surface material. The second part of the question is that using what I call a conditioned probe, which you will remember I defined as one which although made up of multi-contacts, will act as a single contact if you get the little contacts close enough together and distribute them properly. I would refer anybody to a paper by Greenwood (Brit. J. A. P., 1966) which lays this out in great detail. I have pointed out in the past to many people that I think that when the questions come up about spreading resistance resolution, people are often talking about spreading resistance measurements with what I consider unconditioned contacts. If you damage the probes, you will go on and get profiles and you will make measurements and you can even write papers but I do not know that it is going to get anyone anywhere. So you have to be careful about whether the original work and data that went into it was valid or not.

J. KORVEMAKER: I would like to ask Dr. Severin a question. In regard to the plastic behavior, is it similar to the behavior of glasses?

P. SEVERIN: We are talking about monocrystalline structures which are something quite different from a glassy structure. I have given the reference of about 2 years ago about this microplasticity at room temperature of silicon.

D. WILLIAMS: The question that keeps bothering me is that we have talked about accuracy say, comparing spreading resistance to capacitance voltage, but I think in the latest report as published by NBS¹ they also say they have difficulty getting correlation between resistivities that are very close together and spreading resistance. If this is true, since you have calibrated on a set of samples, what are you correcting when you get your profile. Granted a profile may look very consistent but how valid is the calibration back to resistivity?

¹Ed. Note: W. M. Bullis, Methods of Measurement for Semiconductor Materials, Process Control, and Devices, Quarterly Report; April to June 1973; NBS Technical Note 806 (November 1973), pp. 9, 10.

R. MAZUR: This is a point that should be kept in mind; i.e. that fussing with 2 or 3 or even 5% on the thickness corrections and the other corrections may be senseless if the basic calibration data limits you to ± 10 or 15% accuracy. So you have to be practical. It is quite possible still, even at this late date, to get hung up on the micromechanics of that contact and fail to keep your eye on the main thing which is control of silicon processing, if it has to be 100% empirical then that is the way it is going to have to be.

2. PRACTICE

N. GOLDSMITH: Jim White, I noticed in your program you require the operator to input the thickness. Don't you derive any thickness measurements from the spreading resistance data itself?

J. WHITE: Well, we use the test set for two functions; when making surface measurements, of course, we cannot derive any thickness measurements. For depth dependent measurements we could determine the thickness from the spreading resistance data but we do not use that as an input because we could not correct our first values, we would have to store all the values and correct them later.

N. GOLDSMITH: That was my point.

F. VOLTMER: I wondered how you automatically align the probes at the bevel edge?

J. WHITE: We have an interference contrast microscope that is mounted a known fixed lateral distance from the wafer stage. We align the bevel edge with the microscope cross hair and we move laterally by the known distance to position the probes.

J. ASSOUR: I do not know if you said it or if I missed it. How do you measure the depth again in the case of the groove? By staining technique?

J. WHITE: We measure the total width of the groove and from the width of the groove and knowing the diameter of the grooving mandrel you can calculate the total depth and then from each lateral distance you move you can calculate an incremental depth.

J. ASSOUR: I take it that the depth measured by the grooving method was proven by other techniques?

J. WHITE: Yes.

J. KORVEMAKER: I noticed in all the talk about spreading resistance that you never said anything about the quality of the silicon slice. In our company we have been looking at slices, how much of the slice is really good and how much of the slice is not good. Does this have any effect on the spreading resistance measurements?

F. VOLTMER: What do you mean by good?

J. KORVEMAKER: We have been doing x-ray analysis of the slice to get an overall view of what part the slice would be suitable to get good devices from, and there is a fair amount which is not suitable because there are impurities or strains or stresses. How do these affect spreading resistance measurements? I think I have also seen this reported in the Microelectronics and Reliability magazine that is from the United Kingdom. There is, I would say, at least 30 to 40% of a wafer as such is not useable.

M. POPONIAK: I would like to comment. I think in one of the references by D. Gupta of Sylvania, he showed that higher dislocation areas of a slice gave him different spreading resistance values if that is what you mean. Well, he has shown it. Jimmy Hu and I have looked at a microscopic type effect. We looked at copper precipitates using the close space probing and an infrared microscope so we were able to directly correlate spreading resistance data as affected by a copper precipitate near the silicon surface and that was published 2 years ago (*Phys. Stat. Sol.* 18, K5, 1973), so there is a sensitivity to this type of a defect.

P. LANGER: I would like to ask some of the people who discussed other types of equipment and modifications of the Mazur equipment what they have experienced in terms of precision for two types of specimens. Let's say $1\ \Omega\cdot\text{cm}$ *n*-type and $1\ \Omega\cdot\text{cm}$ *p*-type specimens that are uniform throughout. Let's go around the table for those people who have automatic probing equipment and consider precision on a long term basis, say several months.

H. MURRMANN: As I pointed out, we have done these measurements on our calibration blocks for (111) and (100) material. For (111) material we approximately get a reproducibility of $\pm 13\%$. For the (100) surface the reproducibility of the measurements was remarkably better, approximately $1/2$ of this. The reason for that we really do not know. It looks like the (111) surface is more sensitive either to surface contamination or condensation of humidity. In addition, there is some indication that the (111) samples are more sensitive to the degree of misalignment between the exact (111) orientation and the measured surface plane.

F. VOLTMER: Most of our calibration deals with the entire block which is 22 samples and we do not very often compare an individual sample on that block. But from those times we have done so, I feel that our reproducibility on a given sample is something like $\pm 5\%$ over a long term. That is over a period of weeks. This must be qualified, however, since it applies to calibration samples which are chosen to be as free as possible from microsegregation.

J. WHITE: We took an ion implanted slice which we had found to be rather uniform by making repetitive probes across the diameter and got a short term reproducibility, say a one day reproducibility, of maybe 10 different measurements, 5 mils apart, of $\pm 1.3\%$. A long term reproducibility would be around 5%.

P. SEVERIN: We use N-type (111) silicon substrate slices for calibration, and since I never have come across a substrate slice which has a resistivity uniformity better than 1 or 2 percent and since you cannot probe the same spot twice I think the reproducibility should be determined as follows. When you measure a track and the next day make a track at 25 μm distance parallel to it, which is the most similar situation you can arrive at, then you find the measured profiles are different by 1 or 2%. The main criterion however, by which I think we should compare performances and quote precision is how much "grass" is to be seen on a smooth spreading resistance curve. In our case this spurious signal amounts to about 1%.

M. POPONIAK: All I have to say is that I agree with Severin. We have done some implanted structures also and by independent measurements across the silicon wafer, say with resistor type structures there were claims of 1% reproducibility or grading across the wafers and what I saw with the probe is less than 2%. It becomes very hard to interpret data less than 2% so I am saying we are nearly at the limit either because of electronics or mechanical stability of the probing system.

J. KORVEMAKER: I noticed that most of the panel speakers are of rather large multinational companies and I wonder if they have any intercompany relations as far as how accurately they can measure, or at different plants how accurately they compare, or is there just one location where all these measurements are done.

F. VOLTMER: Within our plant we have two different probes in different buildings and we are able to get within 10% reliability on those two probes measuring the same samples when they are calibrated by the same procedure. So between two completely different kinds of probes we can get within 10% on a given sample.

J. WHITE: At Western Electric - Allentown we have essentially two measurement systems also, because there is a Mazur set in Bell Laboratories in the same building with us. I do not have a number to compare the two probes but we use the same calibration samples. We are in good agreement that way.

F. MAYER: The question was raised about intercompany comparison. We ran a pilot round robin in Subcommittee 6 of ASTM. We prepared some 50 μm thick epitaxial specimens, and exchanged samples with 5 laboratories. Our first comparison indicated 6.7% difference between the 5 labs which is not bad at all. We tracked across the surface on 2 and 3 decade paper and found less than 10% variation in uniformity across the sample. Each lab used its own method of calibration and surface preparation.

P. PINCHON: I would like to stress one point about the measurement of a thin low resistivity layer deposited on high resistivity material, because in that case the correction factor acts in such a manner that you measure something controlled by sheet resistance. By experiment it is nearly always the case that the spread in measurement is very much lower in the case of low on high resistivity as compared with bulk material or high over low resistivity material. So for the case of calibration, the reported stability of the probe on ion implantation is something optimistic in my point of view.

R. ANAND: My question is concerned with the conditioning of the probes. Bob Mazur pointed out that's one of the most important tricks of the trade, and I was wondering if people who are using other than Mazur type probes, how do they condition their probes and make them stable.

P. SEVERIN: We use steel probes and they are taken from a four-point probe, in general a used one. In order to get a fine microcontact pattern the probe should be broken. Why are the microcontacts there? They are there because there appears to be in steel little, harder particles, therefore called asperities, which upon contact of a flat bottom surface of such a needle with the flat surface of a silicon slice, act as protrusions. Under a scanning electron microscope we found these protrusions to be very small, at most several

tenths of a micron. But they are absolutely much harder than the steel and therefore the steel acts as a softer matrix in which these asperities can move more or less elastically. So the system is that you break the steel probe, and in some way that metallurgists can explain you expose a number of these asperities. Thereafter like a pencil you sharpen it on a sharpening instrument so that the bottom remains untouched. You stop sharpening when the bottom has almost the size which you think will be the final size and then you start using it. At first the steel is too heavily loaded, and is plastically deformed until the contact has expanded and become so wide that it is not anymore plastically deformed. Then it remains just the same for say 10,000 or 20,000 measurements. Upon contact to the silicon these asperities make very small, something like 0.1 or 0.05 micron, impressions which remain constant over the lifetime of the probe. When you use lapped silicon then you find that a rearrangement of the asperity patterns takes place, but the microcontribution determined by (na) remains the same.

J. WHITE: We use tungsten-carbide probes and I have found that I am able to use a new probe with no conditioning and get very reproducible results.

P. SEVERIN: What is the depth of the damage and do you think the damage does really mean something to the result of the measurement? Tungsten carbide is harder than silicon and the probe I am advocating is softer than silicon with only small protrusions which are harder than silicon. In this way less damage is generated.

J. WHITE: I really do not have a number for the depth but when we compare our profiles with profiles done on a Mazur set, they are similar. One thing you have to realize about our system is that we do lower the probes on the surface unloaded. We load them only after they come to rest on the surface so we probably get quite a bit less damage than a probe that would come down on the surface completely loaded.

P. LANGER: Let me clarify that. I did work for Western Electric and helped in developing their automatic spreading resistance probe. It is possible with that particular probing arrangement to make indentations or to lower the probes to the surface and raise them under 100 grams apparent load such as to leave no visible marks. By no visible marks: you certainly cannot see them under interference contrast, and even with oblique microscope lighting you cannot find any tracks on the surface, so that this probing arrangement may yield a completely different situation than lowering the probes under full load as is done with the Mazur probe.

P. SEVERIN: In order to increase the measurement cycle one could be tempted to increase the landing velocity of the probe. It can be regulated from very small velocities up to about 6 mm/sec with the air escape valve. We have found that above 1 mm/sec the probe shows rebounding on a millisecond scale, actually losing contact and subsequently generating elastic vibrations. It is an interesting observation that the correct spreading resistance value has been reached at the first impact. We have found that a landing velocity of 1 mm/sec, where no rebounds occur, and a measurement cycle of 5 measurements/min. produce most reliable results.

A. ROBINSON: I address my question to Mike Poponiak. I noticed you showed a plot of concentration versus depth for the different probe pressures and there was difference in the concentration peaks. Is that attributed to the difference in the probe pressures or is it attributed to something else?

M. POPONIAK: Well there were two profiles I showed [P-8] and I think you are talking about the high concentration regions, right? On 40 gram versus 10 gram. What I am saying why there is an apparent difference is due to the inadequacy of the correction factor. They should both be the same C_0 . I am saying a 10 gram loading gives us a more accurate representation of the C_0 than a 40 gram loading.

A. ROBINSON: Is it determined by the monitor wafers that you use in your subcollector, or in your diffusion? When you say that that is a more accurate representation of the C_0 , is that C_0 the determined C_0 that is seen on another monitor wafer and not by spreading resistance?

M. POPONIAK: We have done independent correlation with neutron activation or differential sheet conductance measurements. Independent techniques verify that the lower loading works

better with our multilayer correction scheme. Excessive probe penetration at 40 gram loading causes the inaccuracy.

C. GERMANO: I was wondering if any of the participants have noticed the effects of taking small delta X increments where the damage left by the probe marks overlaps the previous steps.

M. POPONIAK: This morning somebody showed data indicating repetitive measurements at the same point and the spreading resistance increased dramatically. Well, in our close space probing where we have done beveling on the probes we are able to achieve a lateral movement of 2 1/2 microns without overlapping. Now a way to test this is to start with something large: 10 micron steps across the surface, reduce it to 5 microns, reduce it to 2 1/2, reduce it to 1. When you see an apparent shift to higher spreading resistance you know you cannot go below that last delta X movement. Someone else said they had a 1/2 mil resolution delta X. I am saying we can go 2 1/2 microns by preparing the probe points properly, without overlapping.

P. SEVERIN: May I ask a question to Mr. Poponiak. One of the key questions that has come here today is the potential distribution around the probe. For then we know whether there is a contact resistance, either a physical contact resistance because of a barrier or a contact resistance because of microcontact. You mentioned 15 microns as the distance between the probes. Could you bring them still closer and then just find out what the key problem is?

M. POPONIAK: It is very difficult, but so far we see no influence.

F. MAYER: In answer to your question we did check on the overlap question, if you have probes that step on each other so to speak. With 2 1/2 micron steps, you see something like a 10% increase in resistivity, with 5 micron steps it is hard to say whether there is an effect: it is no more than 2 or 3%. With 10 micron steps you do not see any effect at all.

I am a little confused about your data, Fred Voltmer, on the interpretation of the contact resistance that you showed. I think you showed a considerably increasing contact resistance after you dipped in HF and as a function of boiling in water. Now on the next slide you showed an oxide growth of up to 700 angstrom. Are you implying that in an hour's boiling you grow 700 angstrom of oxide? Most people consider the maximum oxide growth in boiling water to be about 30 or 40 angstroms. What is your interpretation of that value?

F. VOLTMER: What we did was to take a slice and dipped it in HF to ensure that there was no oxide and when we did that we got a very high spreading resistance. We subsequently grew an oxide in boiling water and measured the oxide thickness and the spreading resistance and what we saw is a dramatic decrease in spreading resistance as the oxide thickness grew to about 100 angstroms. It remained relatively constant then until we grew about 700 angstroms of oxide at which point the spreading resistance got quite large indicating that we were not penetrating the oxide. We grew the oxide for the thin layers of oxide we use boiling water, for the thicker layers they were grown in a furnace tube.

F. VIEWEG-GUTBERLET: I think, Fred, I can now answer for this effect of dipping in HF. We have the same experience measuring p-type polished wafers by four point probe and if you dip the wafer into HF the resistivity increases by about a factor of 10 or more, and if you store the wafer in a temperature of approximately 150° C or you boil the wafer in water, or whatever you do, this increase of resistivity goes down by a time constant of approximately 40 minutes. You can do the same by lapping the wafer and if you lap the wafer partly and you position your probe in the lapped area you can see the decrease of resistivity readings versus time. Our explanation for that is: you have a fluoric layer on the surface, which is strongly n-type, so you bind a part of the holes to that electron layer on the surface, i.e. the negative active layer on the surface. This is related to fluoric ions on the surface.

P. LANGER: Since we have gotten into the area of surface preparation I will just throw in a few remarks and possibly Stan Schwartzman from RCA might want to comment on these. He has the one technique that does not use an aqueous solution in his beveled sample preparation.

What we have found when we went to a lighter loading probes was that you could affect changes anywhere from 100 to 400% in the measured resistance on half ohm cm *n*-type by heating for about 5 minutes at 180 - 190° C and then cooling the sample back down. These changes have been stable right now up to 3 weeks. We have also found the same effect on Bob Mazur's QTA, we were able to decrease the measured resistance by a factor of 2 by using the same thermal cycle.

S. SHWARTZMANN: All of our samples including Bob Mazur's QTA remain stable after they are swabbed with methylene chloride solution with our probes loaded to 45 grams. When specimens are prepared in the presence of water the same degree of stability can be obtained by baking them at 150° C.

P. LANGER: What I was saying was that since you are the only one that does not get tied up with water during sample preparation you may not see this effect at all. I just wondered if anybody else had seen this type of effect.

J. EHRSTEIN: I have just finished over the last month, following some very helpful suggestions from Fred Mayer, measurements using a modified surface preparation procedure. I hope to have something to say on it tomorrow in the fashion of a late newspaper. I think, in terms of improvement, we have answered at least 3 problems which turned out to be related to surface preparation for the upper ranges of *p*-type material. I thought they were separate problems, but they all seem to fall into consistent interpretation now, just because of a modified treatment, namely, the bake out procedure; 150° C for 15 minutes or more following an aqueous chem-mechanical polish. Probably this time-temperature cycle could be made hotter and shorter, but we have not tried it. Let me wait until tomorrow to discuss it further.

J. ASSOUR: Since our experience has been concentrated on deep diffusions for power devices, we are not really relating to the thin layers most of you have been talking here. We did look at polishing techniques versus lapping techniques. The lapping technique is what we use constantly, but in terms of the polishing techniques that we tried on the same calibration samples, and that means syton finish, or chemical polish, or other polishing technique, I must say that in these cases although we would reduce the noise level of our spreading resistance readings we have not been able to reproduce our calibration curve in terms of time stability; I mean that we can polish by syton or chemically, measure, and repeat the measurement two or three hours later and get different points due to drifting. In other words, the ambients, dirt in the room, or humidity, or what have you, seem to influence greatly the polished surface. Of course, since we have gone to the lap surfaces for the deep diffusion structures that we are interested in, we really do not see these effects.

D. WILLIAMS: The TI paper that you had on the calibration and performance of the spreading resistance probe, was that (111) material, syton polished?

F. VOLTMER: Yes, it was, it was all (111), but Lustrox polished.

D. WILLIAMS: In our case I used a least square linear regression. The numbers for the slope for both *n* and *p*-types are almost the same. I compared the residual sum of squares from the actual data to the fitted line and asked myself whether the slope was unity. With 95% confidence, both slopes included unity. Therefore as far as I can tell from the data the slopes cannot be said to be different from unity. Do you use a slope of unity in your work or do you use the exact value of slope as calculated from your least square regression of the calibration data? I find it interesting that my calculated slope for *n*-type is slightly larger than unity and for *p*-type it is slightly less than unity. This of course would have implications for the simple linear model of the spreading resistance measurement.

F. VOLTMER: We use the actual values for slope and intercept from the least squares fit to our calibration data for that particular week. For *n*-type, it is typically 1.03 and for *p*-type it is 0.97. As I showed in my paper the long term distribution of the calculated values for these slopes maintain values slightly higher than unity for *n*-type and lower than unity for *p*-type. These values are obtained from a linear least squares fit of the data in log-log coordinates, but of course a slope different from unity in these coordinates implies a non-linear relation between spreading resistance and resistivity in the model used.

D. WILLIAMS: Can you then determine resistivity values to better than 10%?

F. VOLTMER: Yes, I have the actual data for any given point in my calibration and the distance between that point and the spreading resistance value of the actual specimen of interest allows me to determine its resistivity to better than 10%.

3. APPLICATIONS SECTION

P. LANGER: Fred Voltmer, you mentioned that you have multiple peaks related in some way to the sum and difference of the rotation rates. Could you clarify that?

F. VOLTMER: Yes. For the multiple peaks, if you calculate the distance traveled for the various rotation rates, for example, just the crystal spin or just the crucible spin or the sum of crucible spin and the crystal spin, you will get peaks where the periodicity of the fluctuation corresponds to the growth for that time. So that it is not always so simple that there is just one well defined frequency for the microsegregation, but sometimes there are multiple frequencies, and they all can be related to mechanical conditions during the growth of the crystal. It is not always just a single frequency variation.

F. VIEWEG-GUTBERLET: Did you find for the distance between the peaks related to the rotational striations that the equation $d = F/W$ is satisfied where F is the relative pulling rate and W is the rotation rate?

F. VOLTMER: Yes, in fact that was the point. The peaks in the resistivity translate to a spacial frequency in the transform and that spacial frequency, i.e. the dominant frequency is exactly that derived from the pull rate and the rotation frequency.

K. BENSON: Fred, have you been able to correlate the magnitude of the microinhomogeneity in the radial direction versus the magnitude of microinhomogeneity in the longitudinal direction.

F. VOLTMER: Well, they should certainly correlate. We have not quantified it but the same variations occur in both directions.

K. BENSON: Have you been able to correlate the magnitude of one versus the other?

F. VOLTMER: We have not yet done that. We will report on it when it is done.

B. MORRIS: If I am properly interpreting one of the last profiles shown by Dr. Assour, more gold was accumulating in the region of a p diffusion than that of an n^+ , is that correct?

J. ASSOUR: No, more gold accumulates in the n region than the p^+ . The fact that the p^+ junction seems to have been raised in resistivity is the fact that you have more gold that accumulated right at the junction and in the n region.

F. VIEWEG-GUTBERLET: The explanation you derived from your results in oxygen distribution may lead to the conclusion that oxygen is distributed nonuniformly into the crystal. But I do disagree from our experiments and there may be an explanation that your experiments were done with p -type material. That means that acceptors are distributed in the silicon crystal nonuniformly in the form of striations. If you raise the level of oxygen related donors by the heat treatment at 450° C, you will get compensation at some points and you will increase the compensation rate so the residual resistivity will go up. But this is not due to non-uniform oxygen distribution. It is more due to the nonuniform distribution of acceptors that came out from our experiments when we tried to find out oxygen striations on p -type material but this explanation gave a good agreement of the other measurements on n -type material.

J. ASSOUR: According to your point of view I fail to explain the fact that infrared measurements on wafers before heat treatment do show different oxygen concentrations across the wafer.

F. VIEWEG-GUTBERLET: This may be due to swirls because in crucible pulled material the swirl complexes are distributed very nonuniformly.

K. BENSON: I did have one comment I would like to make to F. Vieweg-Gutberlet. You may have only looked at one oxygen concentration. The problem with all techniques is dependence on the concentration of oxygen in the crystal. If you look at an oxygen concentration of 10^{15} and then at one of 10^{17} you may see completely different reactions. So whether you have seen inhomogeneity or not, I think, depends on what the original oxygen concentration was in the crystal.

F. MAYER: I would suggest that Ken's explanation may be close to the truth. I think the question of time which it takes to develop oxygen donors during the heat treatment comes into it. It takes a long time at 450°C to develop donor dependent resistivity changes associated with striations. I suggest you might try to determine this experimentally.

B. SCACE: The papers were very interesting, both Fritz's paper and the paper that was just given, and we can get into an argument that would last for a week about what is going on inside the silicon but the point is that we have an extremely powerful tool here now for exploring just this kind of thing so let us not get off onto what is happening in the silicon. Let us look at what is happening with the measurement.

S. PRUSSIN: We have been referring to the spreading resistance probe tests as being non-destructive. However, there is no question in the earlier discussion that part of an effective probe measurement is that it leaves a footprint, an area full of little cracks, which act as very nice nucleation sites for dislocation generation if there is any kind of thermal stress. I know that we have tried to remove generation of slip dislocations by the means by which we prepared our wafers. We make sure that we have developed polishing techniques that leave no mechanical damage and so forth and I know that a great many of the studies that have been made in the past have shown a very direct correlation between the presence of mechanical damage and slip associated with thermal shock. I am just wondering has anyone looked into the spread of, or the generation of dislocations or slip from some of these "nondestructive" spreading resistance probe measurements that we have made.

H. MURRMANN: We have not done it until now but we just started experiments and I personally am not afraid of getting dangerous slip damage from that probe tip damaged region especially for (100) material.

F. VIEWEG-GUTBERLET: In answer to the last question, you can see from the slides I showed [A-2] we found spreading resistance traces after heat treatment, and again in the wafer when we etched the wafer after repolishing them. So we have deep damage from the spreading resistance probes and heat treatment at 1100°C .

H. MURRMANN: That is a question of the heat treatment you apply to the wafers, and what you consider to be dangerous damage.

F. VIEWEG-GUTBERLET: May I ask a question of Dr. Murrmann? How does the accuracy of your epi thickness measurement depend on the dopant distribution in your epi layer? Is it true that the assumption is made that the epi layer is uniform in resistivity?

H. MURRMANN: You are right, the method that we have proposed has to be calibrated for *each specific* epitaxial process that you apply. For instance, you would get slight differences between silicon tetrochloride epi and silane epi. In addition, the buried layer diffusion process has some influence on this. But whenever you take a certain buried layer and epitaxial process you can well determine the *effective* epi layer thickness by this method. We have checked it in addition by comparison with breakdown voltage measurements and these values agreed very well too.

N. GOLDSMITH: The only region we have checked for thickness correlation between IR and spreading resistance was a series of samples at around 100 micron epi thickness and here we found that the error was really in point count: you have to choose where you place the interface. For the samples I looked at we agreed to ± 1 point count. For the step size and angle we used this was ± 1 micron out of 100. The accuracy is also limited by knowing where

you start from which adds to the error of where you place the interface. Our interface position is selected by our computer program which calculates the substrate resistivity and then looks for the point where the substrate resistivity breaks and assigns that as the thickness.

F. MAYER: I would like to add here that the subcommittee 6 of F-1 has just undertaken the evaluation of this method as a standard method and I think Fred Voltmer has volunteered to draft it. Do you want to comment on this, Fred?

F. VOLTMER: Yes, I think that the thickness depends very much on the angle which you are probing and the precision or the range of thicknesses you are looking at. For example, for very shallow angles or very thin epitaxial layers the critical factors are quite often the alignment of the probe and the precision with which you can form a bevel. We feel at TI at least that when we are careful and when we are talking about lapping on 17 minute blocks we can determine the thickness of an epitaxial layer, and that is a defined thickness, let me point out, to about 400 angstroms. We define the thickness as the intercept of the slope in the change in epitaxial resistivity with the extension of the substrate. When one has p/n junctions it becomes quite a bit more difficult because there is quite often a depletion region which makes it difficult to define exactly the point where the junction occurs. For thicker epitaxial layers, it is a matter of how many steps you want to take and how shallow a bevel you want to use for probing. In other words, if you have a thick epitaxial layer and are willing to probe almost forever you can get these same kind of precisions. If you take larger steps the step size becomes a limitation on the precision.

G. GRUBER: Fred and I had a bit of discussion about this same thing at the committee meeting. As he says the thickness measurement is dependent certainly on the ability to measure the angle and also on the ability to determine the position of a p/n junction or what you would call the interface between an n and n^+ region. In most cases, I would say in the majority of cases, the picking of the p/n junction is a matter of extrapolating the two sides of the junction to the point of maximum inflection on the curve and calling this point the p/n junction. If there is a broad high resistance region where the spreading resistance is off scale then you do have a problem in choosing the junction. This however, is not the normal case. For determining the thickness of a layer, such as an n/n^+ or p/p^+ , most people do exactly as Fred has said and pick the point where you get maximum inflection in the transition region from n to n^+ . In the normal situation we find that you can determine this thickness to within several percent, or better.

W. SCHROEN: I would like to make a comment on epitaxial layer thickness and concentration. We have seen data this morning which showed that there can be a distinct out diffusion from the substrate at the interface of the substrate and the epi layer. The same sort of out diffusion or autodoping can happen at the interface of the epi layer and the buried layer. From a modeling standpoint this effect has been well explained by researchers at Fairchild such as Grove and Deal and others. Now the question is how do we distinguish between this experimental fact and the multilayer analysis which can pretend such an effect as we have seen yesterday. The answer is that when there is a real out diffusion the original spreading resistance data will show it. The original spreading resistance data can be corroborated by the results of the C-V technique and thus distinguish a true out diffusion effect from the artifact of the multilayer analysis.

S. PRUSSIN: The point was made that by using a very shallow bevels we can increase our precision in measuring shallow diffusions or thin epi layers. By doing that we raise another problem. That is the greater difficulty in determining where to start the probes because the bevel's junction with top surface then becomes less sharp. I think that the end of the layer is one of definition and is not really a problem, but I think there is a major problem with exactly getting your probes to start at the surface-bevel intercept. I was wondering whether there were some suggestions in terms of practical techniques which would enable the operator to start more precisely at this junction.

F. VOLTMER: I would like to address that point. We have established a technique whereby we grow a reactor deposited nitride on the surface of the slice as Walter Schroen pointed out in one of the earlier talks. That nitride is hardness-adjusted to equal that of silicon so that we can get a very distinct intercept between the lapped part and the nitride, so distinct that it is quite apparent in the microscope. The subsequent probing across that

shows then a very drastic change in the spreading resistance when you go off the nitride onto the silicon layer and that then allows you to identify very precisely the position of the beginning of the bevel. The problem is that if you are using, for example, the 17 minute angle beveling block; a 5 micron error in the position of where you put the bevel will give you a 500 angstroms error in depth and I feel that doing much better than that with the 5 micron probe point is quite difficult, but we use the nitride to define very precisely the position at which the bevel occurs. This allows you to see if the bevel is straight, which can be another source of error.

G. GRUBER: We have seen the same thing with an oxide layer on the surface. You can use the same technique.

W. SCHROEN: In answer to your question, I feel as Fred Voltmer stated, that the intercept of the bevel and the surface is no longer much of a problem. It is much more of a problem to determine where the junction is with the substrate or with the buried layer, since we may have to deal with the effects of out diffusion or the artifacts of the multilayer analysis. However, anyone skilled in the art can extrapolate where the intersection of the epitaxial layer and the substrate is. This extrapolation is usually precise enough, even for very thin epi layers. I feel confident that we can determine the thickness of the epi layer at the interface with the substrate.

S. PRUSSIN: One of the techniques that we have tried was to examine the bevel surface after we completed the spreading resistance analysis and tried to find out where the first footprint appeared using interference microscopy. Possibly in this way one can determine very accurately the relationship of the first footprint with the theoretical bevel-surface intercept. What I want to know is whether techniques like this have been tried by anybody.

G. GRUBER: I would like to make the comment that we have been talking about very shallow low angles for measuring very thin layers. We have found in some of the work that we have done that you do not really have to go to as shallow an angle as you might expect to use for a very thin layer. If you go to smaller increments, for example, across a less shallow bevel, say 1 degree, many people worry about the overlap of the probe imprints and so forth. We have been able to determine experimentally that even down to a 1 micron step increment we have been able to repeat and reproduce the results without any problem due to overlap.

B. MORRIS: As to that, I would like to ask what surface preparation technique you use.

G. GRUBER: This is a Syton polished surface using a Plexiglas plate.

B. MORRIS: I have never found that to be true. I wish it were.

G. GRUBER: You have to be sure that when you calibrate, you use the same step increment during the calibration as is used when running a specimen.

B. MORRIS: I still find that with less than 2 1/2 micron steps we have a very large signal to noise degradation and the whole curve shifts. I have one other comment to make as to the surface definition. We do an anodic oxidation, growing about 400 or 500 angstroms which gives you a definite darkening of the surface. This involves no heat so you are not going to change any of the profiles.

B. MAZUR: I have a comment on that, I have made this comment to a number of individuals and perhaps should make it publicly to all, that the business of sample preparation and particularly beveling with small angles is in part a matter of skill. At lunch, I pointed out the similarity to the situation involved if someone buys a transmission electron microscope and just goes in that afternoon and starts to prepare samples. There is a certain amount of skill involved in the preparation of samples! The other point to this is that basically what Gil Gruber has said has merits. What we are suggesting to you is that you consider the possibility of actually being able to use one micron steps despite what Bernie Morris said and thereby achieve greater precision in the measurement of thicknesses on such layers. Along with that I will make a suggestion that I have made to a number of people that I have talked to in the past and that is that the contacts we use are 5 - 6 microns diameter and as someone pointed out when you are taking one micron steps and you go back to look at it afterwards you of course find it hard to see where that first one was because it

is covered over by subsequent probe marks. I would like to suggest here that at the time you begin the run you set up to center the first contact on the bevel edge and initially set the probe to take a single measurement point. Then when it stops you shift the stage laterally by 5 - 10 microns and continue running. This causes a displacement of the rest of the points in the run from the first point and allows you to go back later with an interference microscope or a scanning electron microscope if you like and locate exactly where that first point was relative to the bevel edge. Perhaps parenthetically, I guess, the point would have to be made that the interference microscope is not going to do you much good beyond about a quarter micron as far as determining where you are in depth.

M. POPONIAK: Just a comment on this overlapping. We have done a considerable amount of study and it is a function of the actual resistivity. What I am saying is that there are ranges of resistivity that you can apparently overlap and that is predominantly the high concentration regions. As you go to lower concentration regions it is very sensitive to this and you cannot overlap.

G. GRUBER: What do you mean by "you cannot overlap"?

M. POPONIAK: You get an increase in apparent spreading resistance, an error due to the overlapping.

G. GRUBER: As I said, it is required that you calibrate using this same step increment as when running the specimen. If you do this you can eliminate any effect of overlap.

M. POPONIAK: All right, that is your comment, but that has to be proven because now you are trying to probe into a mechanical damage area, yet everybody is trying to get a good polish to start with.

G. GRUBER: All I know is that I can use a step increment that results in overlap of successive contacts and if I calibrate with that overlap I can get the same resistivity as for the case where I have used a larger, nonoverlapping step increment for both calibration and sampling.

D. DICKEY: I would like to comment on Goldsmith's paper. The errors that you found in your diffused layers no doubt result from the fact that you do not have uniform resistivity in those layers. The so called Dickey correction assumes that you have uniform resistivity throughout the layer. That assumption is certainly not valid for shallow diffusions. This points up the need for an improved correction scheme and that is the subject of my second comment. The problem of correcting spreading resistance measurements in a layer having depth dependent resistivity is something that needs working on. I indicated on my last slide yesterday that the imaginary superposition of a number of layers of uniform resistivity could be a useful approach. It occurred to be last night that that approach is really just the spreading resistance equivalent of the incremental sheet resistivity experiment that people have used for years with a four point probe. I suggest that you could derive corrected spreading resistance measurements from an incremental approach using the same mathematics that are used with incremental sheet resistivity. The calculations that would be required would be rather trivial and I think it is a very logical thing to try.

N. GOLDSMITH: I will comment on your first point. In the interest of time I did not choose to read our introduction which clearly said that your formula was derived from uniform samples and was hardly expected to provide adequate corrections for a diffused layer.

W. SCHROEN: Let me question you with regard to your suggestion of another approach to correction factors. I did not fully understand your suggestion of using a procedure with overlapping steps in order to get to an analogous situation of the four point probe. Would you please repeat your suggestion?

D. DICKEY: I am just suggesting that you should go through the mathematics of using the superposition principle and find out what the difference between successive spreading resistance measurement should be at slightly different depths. The difference in the measured resistance that you see at two microns down and at 2.1 microns down, for instance, should be characteristic of the resistivity in that (1/10) micron increment just as it is in an incremental sheet resistivity.

F. MAYER: Perhaps I could throw another question at you as it seems like an opportune moment. Listening to what we had this morning there has been an excellent correlation between patterns of resistivity established by other methods and by spreading resistance measurements, which says that the spreading resistance probe has arrived as a tool that is accepted. Not only is it accepted, by and large the data seem to fit the models we have of production processes. Having gotten to this point I think it is pertinent to ask where we go from here. Obviously we are moving into an era where we can begin to standardize spreading resistance probe measurements and I think that is underway now. My opinion is that we have not got the speed yet or the facility to use it as an on-line production control instrument or even as a routine instrument in a development laboratory situation. It would be interesting to hear what the panel and perhaps what Bob Mazur would have to say on this subject.

W. SCHROEN: Your comment is well taken with regard to the actual probes. In addition, I would like to suggest that maybe the National Bureau of Standards should start to standardize the multilayer analysis. This standardization should consider what needs to be done analytically, what are reasonable mathematical assumptions, what are reasonable calculation procedures, what are the best computer programs and so on to make sure that all users can uniformly use spreading resistance data.

G. GRUBER: I think it is obvious from this last day and a half that additional work is necessary in the area of layer corrections, i.e. junction corrections and in technique. I think also it is becoming obvious that the technique is becoming more accepted in the industry and that applications are being found almost every day, I think in a few years, when we have the next seminar some of those seats that are empty will be filled.

K. BENSON: That was a nice leading question to ASTM Subcommittee 6 (of Committee F-1); I made introductory remarks on it the other day. At present we have 3 documents in Subcommittee 6 on spreading resistance. One is how to make a surface measurement. We have another document looking at how to make profile measurements and we have another document on using it as a tool to determine thickness. The documents are in various states of preparation and we have only been on this three or four years and every year we do make progress. If anybody wants to speed this progress up I suggest that you be with us in Scottsdale, Arizona in September and by that time we hope to have the first document (on surface measurement) well underway so that the other two documents can be completed.

J. KORVEMAKER: I would like to add a question and remark to what Walter Schroen was saying that NBS should get us some more idea about the method to follow; so I think what Jim Ehrstein was doing in the last half hour of the lecture, in stating that there are certain conditions you have to do to prepare to make the measurements more repeatable could be applicable particularly when you go to a high resistivity area. I wonder if there is not anything there but surface layers of moisture that will give you trouble. I personally always had great trouble to measure pico-amps without having some trouble with moisture. Are the methods that we use for measuring really repeatable and does everyone use the same methods? I do not think so.

J. EHRSTEIN: I do not know exactly how to respond. I can say simply that the work I reported on this morning was only a beginning of what we intend to do. The difficulty is that in terms of any of us with the Bureau of Standards or probably with any government agency trying to assess what problem areas need and deserve attention, none of us are really trying to make work. We do need an accurate assessment of what problem areas still remain.

J. ASSOUR: Now listening here, I really cannot help to feel my frustrations a few years ago when we first were introduced to the spreading resistance probe and the first question that came to us is how do you calibrate the probe, and everybody said you use the four probe sheet resistance method according to the NBS or the ASTM standards; and naively I said, after working 10 years with silicon, what calibration sample should I use? I have a wafer here and I measure $10\ \Omega\cdot\text{cm}$ on my instrument. How do I know it is $10\ \Omega\cdot\text{cm}$ material? Well that is when I think I called Jim and other people and I asked how do I get my hands on something that I can call $10\ \Omega\cdot\text{cm}$ material. But then I thought that for the many years I have been dealing with silicon we have really never concerned ourselves with this problem and everybody had his own technique for determining his calibration samples. Accordingly, the semiconductor industry has progressed very nicely. If we have 10, 10.2, or $10.5\ \Omega\cdot\text{cm}$ does it make a

difference? For the past two years a lot of papers have shown that the spreading resistance technique at least can answer many, many questions that frustrated all of us and I hate to see here that we should get hung up on details about the damage or the microcontact or what have you. At the National Bureau of Standards here I would like to see very much not only two samples of known resistivities (SRM 1520) that one can purchase for calibration but a larger number of samples that cover the whole range of resistivity that the semiconductor industry is actively pursuing.

H. JANUS: I would like to ask if anyone has measured bulk resistivity striations letting the probe steps going down towards 0, do the striations peaks head toward infinity or is there an upper limit?

F. VIEWEG-GUTBERLET: We did a lot of measurements especially to build up a spreading resistance topograph of striations and we found that for the rotationals as well as what we call growth striations, which are autofluctuations, we do not see an advantage to lower the step width below 10 micrometers. But for very sharp profiles sometimes it seems to be better to switch from the two point probe mode to the one probe mode.

J. EDWARDS: What we found when we were trying to get very good precision was that as we changed from the 25 micron to the 10 micron step increment, the noise level increased and as we went to the 5 micron step increment the noise level was greater than the resistivity fluctuations which we were trying to measure.

D. YODER: We have on occasion found we do not know what the conductivity type of the top layer is when probing devices or multilayer structures. I wonder if anyone has any idea of how to determine this? Has anyone tried putting a thermal probe in the spreading resistance probe or something of this order?

B. MAZUR: This is in answer to the previous question about conductivity type. Some years ago I did make a thermo-electric probe that went on one of the early prototype spreading resistance probes. You can do that; it works. We do not have anything like that commercially available. Obviously with the ingenuity that has been displayed here in the last day and a half, it really is not going to take a whole heck of a lot for people to wrap little pieces of nichrome wire around one of those probes, and you can do this if you like. I assume that the question was asked in reference to small areas because obviously if you have a large area exposed on the top surface you could check it with a standard thermo-electric probe. I would suggest the use of chemical stains in conjunction with spreading resistance. I would definitely not go home and pitch out your staining stuff. We have been using the stains consistently from the beginning of our measurements as a qualitative indicator of what is going on overall. Remember that the spreading resistance measurement is a very high spatial resolution measurement and if you evaluate a epitaxial layer, you may choose to do so at one point rather than all over the wafer or all across the diameter and so you may get a very detailed picture but it is a very detailed picture of a localized region. It takes only a few seconds after having the sample profiled on the spreading resistance probe to stain it and get an overall view of whether the junction is uniform and so on. You are not then depending on the stain to indicate a p/n junction or anything. You can simply compare it back to the spreading resistance profile and use it as an indicator of that point on the spreading resistance profile where it falls and see whether the layer is uniform and so on. This was found to be quite useful in the power device area for looking at the effect of rough surfaces on diffusions and the like.

F. VIEWEG-GUTBERLET: May I comment to that problem? As you remember the paper presented this morning from RCA with respect to oxygen effects in silicon and silicon striations. There is a great interest to get information if compensation occurs or if the conductivity type changes within the limits of striations so the question is does the system you are suggesting have the resolution to find out these changes in conductivity type or compensation?

M. POPONIAK: Years ago when we had a three point probe which in reality was a one point probe, we reversed the current polarity. If you increase the current sufficiently to get into a non ohmic region you can determine the type down a profile or across the surface by observing the forward and reverse voltages. It is especially sensitive on high resistivity material.

R. MAZUR: I have comments on both those courses. As far as Fritz's problem goes I do not have a quick answer for you on that. You would have to look at the specific situation to see whether anything could be done. We have generally been able to tell a lot about conductivity type from the appearance of the profile in conjunction with other knowledge that is available such as the thermo-electric probe conductivity type of the top surface and perhaps the substrate region (the regions that are accessible) and we have used that in conjunction with the appearance of the profile and with staining techniques to keep track of conductivity type. Your situation may be a way-out case that is hard to do much with. The other suggestion that Mike was talking about I would caution you about doing that with the existing spreading resistance probes that you are using for making measurements. When you start passing large currents through them, you will be heating the material in the region of the probe and I do not know what microcontacts there are there but things could change afterwards. I am not speaking from experience, I have not tried to do that, but just be aware.

M. POPONIAK: Just on a comment to Bob's. In essence, you do not have to go up every high current to look at a very small differential voltage difference between forward and reverse current so we did not burn our probes out and we did not see deterioration in our older type probes. As far as looking at the general profile from a spreading resistance and relating it to a conductivity type if you recall that one slide I showed yesterday where within 10 mils I had three complete different profiles and by no means did the spreading resistance indicate I had a p -type layer all the way up to the top of the epi over my p^+ subcollector. Only the stain showed that up, we did not type it conductivity wise. Right adjacent to that was an epi over p^- , adjacent to that was a resistor of the same epi but over an n^+ subcollector. Spreading resistance profiles can be very difficult to interpret as far as conductivity type is concerned.

R. MAZUR: I think Mike has very well made my earlier point about the valuable usage of the stain in conjunction with spreading resistance profiles.

F. MAYER: I have one question that arose out of what Bob said. I would just like to poll the panel quickly. There are two ways of doing this, one is constant current and one is constant voltage. I think the majority of the people here are using constant voltage method. How many of you are using the one millivolt or the 10 millivolt respectively?

J. EHRSTEIN: NBS tends to run the 10 millivolt constant voltage mode;

F. VIEWEG-GUTBERLET: We use the 10 millivolt mode;

G. GRUBER: For higher conductivity material, the opamps in the instrumentation tend to saturate at the 10 millivolt level so we tend to use the one millivolt range;

J. EDWARDS: For the work that I did we used the 10 millivolt range;

F. VOLTMER: We use principally 10 millivolts;

N. GOLDSMITH: All our work was at 10 millivolts;

H. MURRMANN: We too work with 10 millivolts;

J. ASSOUR: 10 millivolts.

B. MAZUR: We are just in the process of modifying our standard equipment to use 5 millivolts. There is a reason for that. The operational amplifiers that are readily available and most readily used have an output current capability of 5 milliamps. When you bias one ohm with a 10 millivolt signal you have to draw 10 milliamps. The older discrete circuit operational amplifiers that we used were very nice in going ahead and doing their bit even far beyond the call of duty. Now those are no longer available and we are using integrated circuit modules that are more tightly designed and will not perform anywhere beyond the specified output current so we are in the process of shifting to a 5 mV bias. The one millivolt arrangement is tricky because drift in the operational amplifiers is such that one or the other of the operational amps may drift in such a direction as to have the offset voltage slightly exceed one millivolt in the wrong direction and if so your log Resistance-ratio unit will malfunction and the output will go off scale up or down. It would then need to have the offset voltages on the operational amplifiers reset which is a relatively complicated procedure. Anyway, we are switching to 5 millivolts. If you need a standard for the future, I would suggest starting there.

H. JANUS: I have one thing that has been worrying me. That is when you talk about the accuracy of the measurements at high resistivities, at $5,000 \Omega \cdot \text{cm}$ you have one phosphorous atom in one cubic micron. How can you talk about an accuracy of even 10%?

J. ASSOUR: I guess I should answer this. At least with oxygen and gold you do upgrade the resistivities. I will not even guess what the accuracy of this resistivity is. Usually when we reach these kind of resistivity levels we try very hard to get down as fast as possible or be out of business.

D. BARTH: I want to comment on the closely spaced probes and a question. We have done profiling of thin, one micron and less, epi at one micron stepping distances with a shallow bevel and we have been able to get, with careful surface preparation (a good plate and good polish), good flat curves without a lot of noise. Going down to a half micron and quarter micron steps is very bad. From a noise standpoint, I would consider one micron a lower limit for the time being. Having been involved in ECL and high power microwave transistor work where we have a thousand angstrom emitters and 1,000 to 2,000 angstrom base widths, device performance tells us we are close to that, and we measured that with a spreading resistance probe. I have a question. Have any of you gentlemen done any work on very shallow layers, and I am talking on less than 4,000 angstroms where all the correction factor curves stop, and have you had any success at making those measurements accurately and do you think the spreading resistance technique will be applicable to layers this thin?

G. GRUBER: I have looked at some 0.2 and 0.3 micron layers but they were usually n on n^+ type with no junction involved. We have also looked at thin ion implants but I do not have anything more to say about them.

M. POPONIAK: A comment about Westinghouse's question. We have done very shallow implants and they will be reported on and it looks very feasible.

B. MAZUR: In partial answer to that question and also in line with something that Fred talked about before, obviously, the development of the spreading resistance technique is not finished. I think it is clear always that anything can be improved. We certainly expect to do some additional development work in the future. We also expect to be able to take advantage of some of the genius at IBM and at various other places in order to obtain technique improvements similar to the "bent" probes that we now supply to a lot of people. These are a practical approximation to the IBM closely spaced probes as produced by Ed Gorey. Perhaps in the future we can get closer to the spacing now used at IBM. Certainly I think that you would all agree that there is no fundamental limitation in the use of 25 micron radius probes and certainly no fundamental limitation in the lighter load that we use in normal measurements of 20 grams. Improvements in the mechanical parts, improvements in the probe tips and vibration isolation and so forth may well allow us to get down to perhaps 1 micron diameter contacts. It should be interesting to see whether something like that can be done and perhaps we could then get to a one-tenth micron capability of doing profiles or something similar for thin layers. One last comment on this and then I will shut up, Fred. This applies to the Westinghouse question; it also applies to the comments that TI has made about the junction position, the lack of information about it and so on. You can, of course, use the spreading resistance probe, for instance in the case of samples with base widths of 2 tenths micron or something. Now, if the spreading resistance profiles in those structures are not perfect, what else have you got? Or to put it another way, you can control the production of those base widths and the like with spreading resistance profiles even if you cannot interpret the data to give you absolute impurity concentration profiles. The same thing goes with respect to junction position location. I would be happier to see raw data used more.

D. WILLIAMS: This afternoon I have heard at least two suggestions on where we go from here. The first was that we take a simple empirical approach to the measurement. The other, proposed by Dickey, was that we do some more studying of correction factors. It seems to me first of all that spreading resistance is supposed to do something very rapidly, but cheap and dirty to verify something else you have done. NBS, RCA and TI, at least during this meeting, have claimed that there is a linear relationship between resistivity and spreading resistance. I hope this is true because this implies that the measurement data checks with the simple model. Now, if we were not interested in going to doping profiles then all you would have to do is have N samples which you keep the same and as long as you get the same spreading resistance reading then you do not have to worry about the resistivity of the material, you just say that your spreading resistance probe at least on these samples are measuring the same thing. Maybe these are some of the areas that could be investigated. I think that the study of correction factors is very interesting and make a nice mathematical study. What

does it have to do with actually studying your manufacturing process, if I get a certain spreading resistance characteristic and I know I can get good product on that material and I do not if it is some other material, is this the original intention of spreading resistance measurement?

N. GOLDSMITH: I would like to add comments to that. There are two aspects to using spreading resistance for production control. In one case you are interested in reproducing something and in our case, as Jacques Assour showed, we have a high resistivity n layer covered over by a high resistivity p layer and we want to control that high resistivity n layer. Well then we simply control on the height of the recorder pen. The higher it goes, the higher the resistivity and you can sort by the height of the recorder pen without a calibration graph. That is a practical application; it works and it is in production. However, as Walter Schroen pointed out, there are times when you want to go beyond that. You want to predict in advance from the starting material what devices you are going to make. At that point you have to have quantification of the data and that is when you have to include correction factors. You also have to know what your calibration curve is and it has to be transferable from laboratory to laboratory.

J. ASSOIR: It is true that with production processes you like control. Good or bad. Of course, good control makes good devices, and when you have bad control you try to make it better. On the other hand, there are still a lot of device characteristics that we do not understand in solid state devices and there comes a time when you have the structure of the device you would like to sit down and think about it and try to correlate it with the physics and in this instance you must have some data that you can rely on in order to develop design modeling.

S. MYLROIE: I think it was alluded to in some of the other answers that there are really two areas we are talking about. One is the production control area where the raw data is good enough for process control and maintaining a process. The other area where we need the calibration and correction factors is that of closing the loop back to the device characteristics, in order to work with the circuit designer and the device designer. Then when they know the devices they want, through models we can develop and characterize the processes needed to generate these devices. For this type of work we need to be able to relate to doping concentrations and the device physics and this area is where we need the accurate correction factors.

D. WILLIAMS: I perfectly agree, I think that the correction factors are an interesting study and should be done. That is my point. However, there appears to be many chances for error in going from spreading resistance to doping profiles. If I have spreading resistance values I hope by calibration curves to translate to resistivity values. From there I can hopefully go to dopant values by using Irvin's curves. Now, many factors, as pointed out by NBS and RCA, may cause problems such as surface preparation that gives a high resistivity, or maybe you have a damaged surface, but you are also going through at least two translations and a correction factor from a spreading resistance to a doping profile before you come up with any answers. How many other factors there are, I do not know.

G. GRUBER: I would like to comment, I think the fact that this is an NBS and ASTM jointly sponsored symposium goes a long way in saying that the real need in the industry is for standardization of the spreading resistance technique, in fact for standardization of all resistivity measurements. This whole week was interesting for me, it was the first time I had been to an ASTM meeting and I would recommend it to everyone in the industry. It pointed up to me the need for standards in things that I considered pretty well standardized already, such as wafer thickness. I also sat in on a discussion of the inaccuracies of mobility measurements and the effects on Irvin's curves. Coupled with this I think what we have learned at this Symposium is the fact that the Spreading Resistance technique can be no better than the standards on which it is based.