Discussion—Steels Session

Design of High Hardness, Tough Steels for Energy-Related Applications by
V. F. Zackay

Question: Dr. Halle Abrams [1]—As to the aluminum-silicon interaction
you referred to—would you explain why you were getting the fracture
behavior you observed? Where is the aluminum or silicon going so that it
produces the observed behavior?

Answer: Dr. Zackay—The effect of the combined aluminum plus silicon
on fracture is a complicated one. About 20 years ago, people at both MIT
and various specialty steel companies studied the role of silicon in steel.
They observed that it delays the onset of softening. How does it do it?

There are certain chemical and crystal-chemical reasons that have been
postulated to explain this effect. One is that aluminum, silicon, cobalt,
and other similar noncarbide-forming elements increase the activity of
carbon. By increasing the activity, they favor the formation of epsilon
carbide over iron carbide. Another reason that has been suggested is
based on the fact that silicon does not fit in the lattice of iron carbide. For
iron carbide to form, the silicon must diffuse out into the surrounding
matrix. Silicon diffusion at ordinary tempering temperatures is very slow
and thereby delays the onset of softening. The aluminum-iron phase
diagram is very similar to that of the iron-silicon system. In general,
everything that can be said for the silicon-iron system is applicable to that
of the aluminum-iron system. With respect to fracture in these systems, I
think it was Prof. McMahon who noted, in his studies of temper
embrittlement, that silicon goes preferentially to grain boundaries. He
noted a large concentration of silicon, at certain tempering temperatures,
at the grain boundaries. From reference to the Fe-Si system, it is well
known that above about 3Si short-range order is observed and long-range
order at about 5Si. So, if the silicon segregates preferentially to, or near,
the grain boundaries either short-range or long-range order occurs and
embrittlement is likely to be observed.

Aluminum has a different size than silicon. As far as is known, it does
not segregate to the grain boundaries. Therefore, a combination of silicon
and aluminum on the one hand, enhances the solid strength, while on the

[1] Bethlehem Steel Corp., Bethlehem, Pa.

other the silicon (if less than 1.5 percent) does not segregate and the onset of softening is delayed to a maximum degree. Prof. Ritchie at MIT has shown that, in addition, there is apparently some enhanced stress corrosion behavior in these silicon plus aluminum alloys.

Question: L. Thompson[2]—Have you looked at the effects of thermal aging in service on the mechanical properties in the secondary hardening steels?

Answer: Dr. Zackay—Not as yet—this is, however, a subject of considerable interest to us. We are just beginning long tempering time experiments at relatively low temperatures. Results to date indicate that significant increases in toughness can result with little deterioration in hardness. Eventually these alloys will have to undergo long-time aging experiments to test their structural stability after such exposure.

Microstructural Control in Microalloyed Steels by Morris Cohen and S. S. Hansen

Question: I. F. Hughes[3]—You mentioned in your talk the necessity for control of niobium content, and we feel from our experience in making the 80 ksi hot rolled steel that at about 0.1Nb, the carbon level is extremely critical and that holding carbon to a range of 0.03 percent is an acceptable limitation for the BOF shop.

My first question is do you have data showing the influence of a change in carbon content from 0.03 to 0.06 percent on Cb (CN) precipitation kinetics at a columbium level of 0.1 percent? The second part of my question is that we seem to find some influence of silicon on improving toughness in these Mn-Cb grades, and I wondered whether you had seen a similar effect.

Answer: Dr. Hansen—We have no data showing the influence of carbon variations at the 0.1Cb level on the Cb (CN) precipitation kinetics in the austenite. Certainly, changes in the carbon content would affect the amount of columbium that could be dissolved in the austenite at the soaking temperature. How this might affect the response of the austenite to hot-working is debatable, however, there might be a significant effect on the subsequent ferrite strength due to changes in the amount of dissolved columbium available for subsequent precipitation hardening.

I am familiar with recent work that shows a similar influence of silicon on toughness in a titanium high-strength low-alloy steels. However, to the best of my knowledge the nature of this effect is still unclear.

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[2] General Atomic Co., San Diego, Calif.
[3] Inland Steel Research Labs., East Chicago, Ind.

Question: Dr. R. Cornwell[4]—Can present-day rolling mills handle the schedule you are suggesting where you do not get any recrystallization of the austenite? Because it seems to me you are just cold rolling the austenite.

Answer: Dr. Hansen—I think they can, provided the rolling schedules are designed to avoid large reductions per pass. While mill loads certainly will limit what can be achieved, especially when rolling unrecrystallized austenite, this paper was not aimed at designing an optimum processing schedule. Rather, it was meant to provide a framework for considering how to maximize the degree of austenitic grain refinement obtaining during rolling.

Comment: Dr. Abrams—I'd like to add a little bit to that. I like to think of the process as warm rolling. And you have to tailor your rolling schedule to your mill capability. The Japanese and Italians have much higher mill capacity than we have domestically, or certainly what Bethlehem has. But you can design a rolling practice consistent with your mill capability and still achieve the Stage III Processing that Dr. Hansen showed. In fact, this Stage III Processing ties in with another question: I'm not quite sure why there is a limiting austenitic grain size of 20 to 25 μm. Can you comment why that is? Why can't one get further grain refinement?

Answer: Dr. Hansen—I'm not completely sure either, but one way of perhaps rationalizing it might be to remember that as you refine the austenitic grain size by repeated recrystallization, you also increase the surface-to-volume ratio, and decrease the average growth dimension for recrystallized grain. Eventually, a *limiting grain size* might be approached when you have essentially one nucleation event per grain, and rapid growth of that nucleus, consuming the deformed grain prior to the next nucleation event. While speculative, this may be a reasonable way of explaining the observation. Actually, we're really not trying to explain this observation, but rather point out that there does, in practice, appear to be a *limiting recrystallized grain size*.

Comment: Dr. Abrams—What you're saying makes some sense, because I could see that if it's a thermodynamic limitation and there are surface-to-volume energies associated with it, then at some point you are just going to violate a law of thermodynamics and so that's it—that's all there is left in your system at the temperature involved. It's not a precipitate-pinning restriction or anything like that.

Still another question I had is on one of your initial slides where you showed these very very fine precipitates along the prior austenite grain

boundary. You indicated that these coarsen and that they coarsen very rapidly. It's not an Ostwald ripening mechanism. Do you have any thoughts as to how they're coarsening?

Answer: Dr. Hansen—It is likely that this rapid grain boundary coarsening is due to enhanced diffusion along such boundaries. As far as the precipitate sizes are concerned, they are about 50 Å on initial observation, but grow to >150 Å within 100 s at 950°F. This is a substantially faster growth rate than is observed for the Cb (CN) precipitates on austenitic subboundaries.

Comment: Dr. Abrams—Of course, when one is speaking of diffusion, 100 s may not really be that fast.

Question: Dr. Malcolm Gray[5]—Did I understand you correctly when you said you did not think precipitates were involved in that equilibrium between the temperature and limiting grain size? There are people probably better qualified than I am here to address that subject, but in my experience, I would have thought that precipitates were one of the most important facts. I do not know whether Dr. Pickering would agree.

Answer: Dr. Hansen—Let me comment first, if I might. Although the presence of precipitates may influence the grain size/temperature equilibrium, via a Zener-Gladman type of precipitate/boundary interaction, it might be fruitful to point out that recent data developed by Dr. Lee Cuddy of U.S. Steel seems to indicate a limiting grain size in plain carbon and vanadium steels as well.

Question: Dr. Gray—Is it the same size?

Answer: Dr. Hansen—In Dr. Cuddy's work, vanadium and plain carbon steels approached a limiting grain size of about 40 μm, while the limiting grain size for columbium steels was about 20 μm. A similar effect has also been observed by Sekine and Maruyama (see Ref *14* of Cohen and Hansen's paper).

Comment: Dr. F. B. Pickering[6]—The question of a limiting refinement of the austenite grain size and the resulting transformed ferrite grain size produced by thermomechanical processing is an important one. In the case of the recrystallized austenite grains, one must consider not only the nucleation at deformed grain boundaries, sub-boundaries, deformation bands, and other lattice defects, but also at dispersed particles. The size of such particles that can act as recrystallization nuclei is also of crucial importance. In addition, particles may also limit the grain growth of the recrystallized austenite by a grain boundary pinning effect. There is

[5] Microalloying International, New York City, N.Y.
[6] Sheffield City Polytechnic, Sheffield, England.

clearly going to be some critical particle size at which particles can change their behavior from inhibiting recrystallization to accelerating recrystallization. Thus, one needs to study what affects the size distribution of not only the undissolved particles but also those precipitated during thermomechanical processing. Such factors as the degree of supersaturation and the defect induced nucleation may be anticipated to be important. The matter is presumably one of the energy balance, and if one continually refines the prior austenite grain size, the total grain boundary energy of the system will increase to some limit at which the energy available in the deformed and recovered structure is insufficient to balance it. Do we know what this limiting grain size is?

Also, the equally important question of the ferrite grain size produced by transformation of the thermomechanically processed austenite requires to be examined, as this is the critical parameter of relevance to the mechanical properties. Again there will presumably be a limiting small grain size. The factors affecting the nucleation and growth of the ferrite formed during transformation, particularly those influencing nucleation, need to be quantified. However, because there is energy arising from the transformation, the limiting ferrite grain size is much smaller than the limiting austenite grain size. Again one would wish to know the quantitative effects of such features as austenite grain and subgrain boundaries, second phase particles, deformation bands, and lattice defects, as well as of residual solute, on the rate of nucleation. The factors governing the rate of growth of the ferrite during transformation are equally as important, and those governing the grain growth of the fully transformed ferrite structure must also be considered. The experimental difficulties in obtaining such quantitative data are, however, formidable.

Evaluation of Steels for Arctic Line Pipe by Halle Abrams and G. J. Roe

Question: F. Sczenzenie [7]—Could you perhaps comment on the microstructural rationale for the small positive effect of carbon on the 85 percent shear test results?

Answer: Dr. Roe—There are a couple of things to point out, and I only use the following as an example. First, we're working within a very tight chemical compositional range—the carbon varied only from 0.08 to 0.13. Secondly, carbon was the least important in the regression analysis work. It was the fifth or sixth term to kick out. So it's pretty far down the line.

Comment: Dr. Abrams—When doing regression analysis, one often has to take into consideration very strong interactions between the independent variables. If you looked at the independent variables we used, you

[7] Special Metals Corp., New Hartford, N. Y.

noted that there were several that strongly interacted. For example, percent total cumulative reduction is very strongly interactive with average mill load. You could get an inverse effect, where the total cumulative reduction is going in the expected direction and also takes into account the effect of average mill load; but then the coefficient of average mill load would be altered and might even become negative because of this interaction effect. And this interaction and the resulting inconsistencies are something you can't avoid, because the time-deformation-temperature interactions during processing are indeed extremely complex. And which is the lesser of two evils? Trying to represent 90 percent of what's really happening in the steel, or just representing maybe 50 percent of what's happening because one fails to take into account additional parameters that interact.

Now, in the case of carbon, as Dr. Roe pointed out, it would probably influence the drop weight, let's say, plus or minus two degrees over the temperature range we're talking about. So it could be an inverse effect, or it might be just a total ambiguity.

In view of the frequently complex higher order interactions and the consequently complex regression analysis, we deliberately did not give the regression coefficients, because we didn't want such coefficients to become, as it were, *the final word.* You know, the kind of deceptive simplication where you multiply carbon by, say, 0.437, and you think you've got it. So, rather than give coefficients that could be falsely restrictive, we chose to show the trends or directions in qualitative terms.

Question: F. Sczenzenie—Perhaps I understand you to mean there is no microstructural rationale for that particular result?

Answer: Dr. Abrams—Yes, that is the case, although in the course of our study we did take into account microstructure. You see, one of the problems is that a statistical empirical analysis can sometimes be at variance with the conclusions one might get from a strictly physical metallurgical approach. It's simply very tough to make these two approaches come together, and sometimes you get results you can't explain. I believe Dr. Roe has one more slide he didn't use in his presentation that would be helpful in demonstrating the contributions of a statistical empirical approach. (See Tables 4 and 5 on p. 94.)

Comment: Dr. Roe—We didn't make all these heats at one time. Over a period of time we made four heats and used this information to predict what we expected to get for the subsequent heats (especially for the vanadium-columbium heats). Some of our guys didn't have much faith in Dr. Abrams' predicted values, especially for yield strength; yet, as these subsequent heats showed, the predicted values were quite accurate.

Comment: Dr. Abrams—As Dr. Roe pointed out, in this study we were able to effectively use regression analysis to predict the mechanical properties so that once the chemistry was known, the processing could be adjusted to achieve the desired properties. Specifically, this is what we did: we processed four heats, and, based on these four heats and the properties obtained, we predicted what the properties would be for the next three heats. For example, for 0.630-in. plate, we predicted an 80 ksi yield strength for vanadium-columbium plate.

And some of the fellows up at our research labs just didn't believe you could get that high a yield strength with a vanadium-columbium grade. In fact, when I gave them my prediction, I granted that the yield strength value was probably high. The average measured value was 80.5 ksi. And then with CV_{15} transition temperature, and drop weight, others from our labs were at first quite skeptical on this one, because they're usually within 50 or 60 degrees when trying to do similar kinds of things—and here we were predicting the drop weight shear within 10 degrees. These numbers are extremely impressive to me.

I don't say that we're all the way there yet. After all, as Dr. Roe pointed out, we were working with only a very narrow range of alloying and a narrow range of processing. Nevertheless, the fact that we got as close an agreement between predicted and measured as we did is pretty good, considering that a property such as the drop weight shear can vary all the way from −80 to +60, depending on chemistry and processing. And so I think that this type of application is where regression analysis can be really useful.

Question: Dr. H. Solomon[8]—These pipeline steels are all welded together, but I haven't heard anybody mention anything this morning about the properties of the heat-affected zones of the welds. How do they change and how does the structure change in the heat-affected zone of the weld?

Answer: Dr. Abrams—We didn't dodge this issue. Dr. Roe was talking at about 88 rpm to get in at least the highlights of a project that represented about three and a half years of work. The internal report on that work was a pretty thick one. In that report are all the welding studies, and that's really the topic of a separate paper.

I would like to make this comment. People have had problems with the hardness tracing across the heat-affected zone. The hardness was a problem for us, too, but that's because some of these specifications were changed as rapidly as the weather. In the case of several specifications, they asked for a pre-mill qualification requirement as well as what we would do under normal production conditions.

[8] General Electric Co., Schenectady, N. Y.

I think one of the problems was that we had to use a 1-kg load for the pre-trial, but then for the actual production pipe, we were able to go to either a 30- or 10-kg load, I've forgotten which. The scatter band with the 1-kg load was much greater than with the higher load. So we found that several of the steels we tested didn't meet this particular hardness requirement. But I don't think that's a serious problem.

Now, the work done by our welding group goes into a lot more of the details of this—the effect of alloying and diffusion, and things of that nature.

Question: Dr. Solomon—Specifically, do you get any grain growth right next to the weld in that portion of the heat-affected zone right at the fusion line?

Answer: Dr. Abrams—Of course. If you look at the microstructure, going from the weld through the fusion line into the base plate, there's no question but that you have transformation products—you have a bainite/ acicular ferrite and some martensite, and then you get a much coarser ferrite than you have in the base plate. That's why it's a heat-affected zone.

Question: Dr. Solomon—I'm surprised that you say there's very little difference in properties?

Answer: Dr. Abrams—I didn't say that there was *no* difference in properties. In this sort of thing one gets into the whole problem of just how one tests for heat-affected zone properties. For example, with respect to notch toughness, after you etch the weld area and reveal the curved fusion zone, where do you put the notch to effectively measure the HAZ notch toughness? Is it at 1 mm, 3 mm, or 5 mm from the fusion line? The effect of test procedures and the interpretation of results would require a separate paper. Naturally, when you get the kinds of microstructural changes previously mentioned, there will be some deterioration of properties in the heat-affected zone. But the point is that if the base plate has the right properties to begin with and the proper welding practice is followed, the HAZ properties will still meet the specifications for Arctic requirements, which is what we as the producer are trying to do.

Question: Dr. Solomon—One other question. These materials look like they would have very anisotropic properties, especially impact properties with this long, elongated structure. Do you find that there is a significant amount of anisotropy, and if so, what is the direction of crack growth in your impact measurements that you reported?

Answer: Dr. Abrams—All of the data is transverse data that we're reporting, which is the worst direction of testing. The longitudinal direction would, of course, be much superior.

Question: Dr. M. Korchynsky[9]—You have shown that the difference between the strength properties of plate and pipe necessary to meet X70 specifications can be traced to the strap tensile testing. You also have shown pretty good correlation between the true yield strength of the engineering product line pipe, as measured by the ring expansion test, and by the machined round tensile test. I have two questions.

First, what are the chances to introduce the machined round tensile test into the line pipe specification and eliminate the misleading test performed on flattened straps?

Second, do you have any information whether the difference between the minimum plate strength properties needed to meet specified pipe properties could not be decreased by reducing the Luders band expansion by means of accelerated cooling from the finishing temperature to, say, 1200 or 1150°F?

Answer: Dr. Roe—I don't really think accelerated cooling would probably help these steels because we're talking heavy-gage plate. Let me point out in response to the first question that right now I think the only acceptable test is the strap test. And this bothers me a little bit. The producer is going to manufacture the pipe in accordance with the specs for the pipeline. But something the users have to consider is that even for the columbium-molybdenum steels that work harden, when measured by the ring test, they have a yield strength of 80 000 psi just like the vanadium-columbium. If you're going to measure the pipe by a ring test, I wonder if anybody has considered that they're measuring only 70 000 psi on the strap test but really they have an 80 000 or 90 000 psi yield strength piece of pipe. How does this affect their fracture toughness requirements?

What I'm saying is, if I was going to do it, I'd pick a more representative test, which I believe to be the ring test, or, a machined tensile would be another possibility.

Comment: Dr. Abrams—I'd like to add something. What we hoped to show here was that the user is deceiving himself. He's getting this extra strength that he's requiring in the strap test at a sacrifice in toughness. And all of his models for ductile running fractures, which are based on strength, will then be screwed up.

To answer your question specifically, E. Jonas from our central metallurgical group has a proposal before API for the use of a machined round tensile in evaluating the pipe yield strength. I think that if we can present enough data to support this proposal, it could become a reality.

As to the second question about the Luders extension, I agree with Dr. Roe only from the point of view that it's kind of impractical to do accelerated cooling on the plate mill. That is the main limitation. But you could probably get some benefit from it via continuous yielding.

[9] Union Carbide Corp., Pittsburgh, Pa.

But one of the things that I have found that is very strange—and I have data to support this—is that, if you test enough specimens, you find that some of the acicular ferrite grades that are supposed to have continuous yielding behavior actually don't all have continuous yielding behavior. Likewise, some of the vanadium-columbium grades that have discontinuous yielding behavior and Luders extensions sometimes have continuous yielding behavior.

Comment: Dr. Korshynsky—I would like to point out that the practicality of accelerated cooling of plates at least up to ¾ in., has been demonstrated, as reported in a paper by J. D. Grozier, published in *Proceedings Microalloying '75* pp. 241–250. The effect of accelerated controlled cooling on improvement of properties of plates has been reported also by researchers, K. Tsukada et al of Nippon Kokan.

Answer: Dr. Abrams—I'm not saying it's not feasible; it's very feasible. It's just a question of paying for the facilities and so forth.

Question: G. Delvecchio[10]—Is it practical to use a plate yield strength of 89 ksi if the required flattened strap yield strength is 70 ksi?

Answer: Dr. Abrams—Definitely not! Again, there's the question of the economics. You could develop a rolling practice, consistent with your mills, to achieve the specified properties. Because you want to have sufficient product yields, you just don't want to be rejecting plates like crazy after you put this much money into it, and you know it's not going to go into pipe.

The other thing that you have to be very careful of, and I don't think this point came across, is that these two sigma 95 percent confidence intervals can be very misleading. Because it doesn't mean, if you have a plate say, 77 ksi, that it won't be a 70 ksi pipe. In other words, we're saying that at a 95 percent confidence level you want minimum 89 ksi plate and then you will be assured of making X70 pipe. And in the paper, I think we have some examples. With regard to the Battelle drop weight requirement of +23°F in the pipe, it has to be −10°F in the plate to assure this requirement at a 95 percent confidence level. And there were four plates, one might have been +7, one might have been −5, and they were not −10 but they still made pipe that were within +23. And all that meant was that the plate-to-pipe shift for these particular pipes plus the two sigma variation that you're trying to predict from the data was less than expected; so you were able to still meet the pipe requirement.

Question: G. Delvecchio—The present specification for the Alaska Highway Pipeline requires a CV_{100} energy of 80 ft·lb. Have you done work examining the CV_{100} energy?

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[10] The Steel Company of Canada, Ltd., Hamilton, Ontario, Canada.

Answer: Dr. Abrams—Yes, we have. When we get into the data, we can also see what the CV_{100} is, because we rate the fracture appearance of each Charpy test specimen and plot full Charpy fracture appearance and energy curves. The way you would do this is to go to very low carbon and to very low sulfur. With these conditions, we have seen CV_{100}'s in excess of 100 ft·lb. But I would hate to have to agree to that kind of a number. It should realistically depend on the design temperature.

Once again, I think this is where the specification writers or the users of this pipe are not necessarily being practical, because if you combine your chemistry and processing, you can reach 100 percent shear at, say, −60 or −100. And your CV_{100} at that point might be 40 ft·lb. But at the design temperature, let's say −10, since your curve is constantly rising, it might be 80 or 90 ft · lb. And again they're asking for a requirement that has no relation to what the pipe has to do in service at the design temperature. And I hope there are some pipe users out there. This is where some education has to come in. Because you just don't get CV_{100}'s of 80 ft · lb for free; and if it turns out to be at −60 or −80°F, they don't need it.

Question: G. Delvecchio—Can you comment on splitting in these steels?

Answer: Dr. Abrams—Actually, that's another paper, but here are some brief comments.

Obviously, if you go into the two-phase region or if you do very severe controlled rolling, you do get splitting. And when you get splitting, you get lower shelf energies. Surprisingly enough, the conditions at which we rolled these pipe had only very minimal splitting. But this whole presentation—and what we're showing—has gone through a learning experience.

If a few years ago somebody had said, "I want 85 percent shear at −50 or −60°F in the plate for ¾-in. plate," you would have laughed at him and wanted to add 3 percent nickel or something like that. But users want to get specified properties without going to expensive alloying. Through this learning curve, we did try novel and relatively extreme processing conditions, and you might be familiar with some of the continuum rolling work that was reported out of Bethlehem. In this work, we did see extensive splitting. And I think Dr. Bruce Bramfitt just published one of the papers on splitting observed in our laboratory.

But now we're trying to avoid splitting, because it's a fracture result we do not want. And so you could compromise and raise your finishing temperature sufficiently to try to avoid splitting.

Question: L. Luyckx[11]—You mentioned sulfurs between 0.002 and 0.010 as one of the chemical variables, and you mentioned also that all

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[11] Reactive Metals and Alloys Corp., West Pittsburgh, Pa.

your plate was tested in the transverse direction. Other studies have shown some major property differences between these two values, suggesting that you could have variations between 0.002 and 0.010 sulfur and you never mentioned that in your results. Could you elaborate, please?

Answer: Dr. Roe—The earliest heat was the 0.013 sulfur, the last heats we made were 0.002 to 0.003, the highest was 0.006. They were desulfurized hot metal plus a misch metal plunge in the ladle to get the sulfur way down and get the shelf energies up.

Control of Microstructure by the Processing Parameters and Chemistry in Arctic Line Pipe Steels by C. Ouchi, J. Tanaka, I. Kozasu, and K. Tsukada

Question: Dr. M. Korchynsky—First, what was the maximum gage of plate to which accelerated cooling was applied?

Answer: K. Tsukada—The accelerated cooling is basically applicable to any thickness plate.

Question: Dr. Korchynsky—What was the gage in your experiments?

Answer: K. Tsukada—The plate thickness investigated here was 20 mm.

Question: Dr. Korchynsky—Was the shape of the stress-strain curve of plate subject to accelerated cooling different from that of hot rolled and air cooled plate? Particularly, was the yield point or elongation any different?

Answer: K. Tsukada—Most of the steels showed the yield point if the suitable controlled rolling is taken before the accelerated cooling. In a higher cooling rate above 20°C/s or in the relatively higher carbon equivalent steels, the yield point tends to disappear. However, ductility such as elongation is not changed by the accelerated cooling.

Question: G. Delvecchio—You presented some data showing the effect of titanium for improving toughness of the plate. Is this effect applicable to the hot strip rolling of skelp?

Answer: K. Tsukada—Yes, titanium addition is useful in a strip mill rolling. Titanium nitride prevents the growth of austenite grain size in addition to the refinement of the austenite grain size attained at the slab reheating temperature. We confirmed the beneficial effect of the small amount of titanium on toughness in hot strip mill process.

Question: G. Delvecchio—What are the limitations of using a titanium nitride mechanism?

Answer: K. Tsukada—The amount of titanium addition is very impor-

tant, because it depends on that of nitrogen. The cooling rate of solidification process is also important to obtain finely dispersed TiN.

Question: Dr. M. Gray—Do you find that the amount of titanium or nitrogen or the titanium to nitrogen ratio that you use for control of grain coarsening is affected by the vanadium and niobium levels? That is, do you have to adjust the amounts or the ratio when you have other microalloying elements present?

Answer: K. Tsukada—Titanium addition is 3.5 times nitrogen content. The formation of TiN might be affected by vanadium or columbium, but the effect is not clear.

Question: Dr. Gray—You don't change it—you always use the same 3.5 to 1 ratio?

Answer: K. Tsukada—All the same.

Question: L. Luyckx—The effect of quench and temper on the properties was tremendous, but you showed the first improvement after accelerated rolling.
My question is, do you still need accelerated rolling when you're going to ultimately use the quench and temper practice in the pipe? In other words, does it make any difference in the quench and temper properties whether you've applied it to as-rolled steel or after accelerated cooling practice?

Answer: K. Tsukada—Accelerated cooling and quench and temper treatment after pipe forming is a completely independent process. Accelerated cooling in plate is not necessary for heat treatment after pipe forming. Although both processes give rise to different effects to the properties, production costs in each process are also different.

Question: L. Luyckx—Regarding your quench and temper results, were they after accelerated cooling or as rolled?

Answer: K. Tsukada—As rolled.

Influence of Microstructure on the Temper Embrittlement of Some Low-Alloy Steels by R. Viswanathan

Question: L. Luyckx—In one of your very first slides, you showed *very pure steel* with that curve going down from the top; what do you mean by *very pure steel?*

Answer: Dr. Viswanathan—The *pure* steel contained phosphorus, sulfur, antimony, and tin below about 15 ppm. Details regarding the composition of the steel are provided in Ref. *1* of my paper.

Question: Dr. S. Banerji[12]—Molybdenum is fairly well known to be an inhibitor of temper embrittlement; why would you expect any kind of temper embrittlement in a chemistry loaded with molybdenum?

Answer: Dr. Viswanathan—The effect of molybdenum on temper embrittlement is rather complex. It may be beneficial or detrimental depending on the level of molybdenum. Interaction effects between chromium and molybdenum are also important in this connection. Furthermore, for a given nominal bulk composition of molybdenum, the molybdenum content of the ferrite matrix decreases with prolonged tempering. This could lead to attendent changes in segregation of impurity elements to grain boundaries.

Question: Dr. Banerji—In the tempering kinetics, when you said the alloy carbide begins precipitating, you lose molybdenum from the ferrite, so, you are losing the effect. As long as you are in a range where you can hold molybdenum in solution, would you not expect a suppression of temper embrittlement?

Answer: Dr. Viswanathan—It is not clear to me whether the low temper embrittlement susceptibilities reported for Cr-Mo steels reflect the true embrittlement behavior of these steels or whether it is due to the fact that the steels have not been exposed under the conditions producing maximum embrittlement. Presently available data are inadequate to resolve this question.

Question: Dr. Banerji—What I really think I am asking is, is temper embrittlement as critical in Cr-Mo-base steel as it is in Ni-Cr-base steel, for example?

Answer: Dr. Viswanathan—In general, available data suggest that the Cr-Mo-type steels are less susceptible to embrittlement than steels that contain nickel and chromium in combinations.

Comment: Dr. R. Swift[13]—I do not want to defend the data, I just want to explain something. When we started to look at commercial Cr-Mo steels, we were trying to determine the susceptibility of a plate once it got out into the user's shop. In looking back, we could have done things a lot differently. Had we been smarter in the beginning, we would have taken smaller sections and water quenched them after tempering. Any embrittlement would then be a measure of the true susceptibility of the material. That would have helped to eliminate a lot of the confusion that has resulted from trying to interpret early data. You are looking at relative effects of different heat treatments on susceptibility to embrittlement. I

[12] Foote Mineral Co., Exton, Pa.
[13] Lukens Steel Co., Coatsville, Pa.

think that the work you and Prof. McMahon of the University of Pennsylvania have done shows that Cr-Mo steels have a low susceptibility to embrittlement. I agree with you, the data is often very confusing.

Question: R. Gaitonde[14]—About the level of impurity elements in commercial heats—what do you think is the minimum amount of antimony or phosphorous you could have before you can eliminate temper embrittlement from rotor forgings? Can you really attain that?

Answer: Dr. Viswanathan—I really don't have a simple answer to the question. All we know is that in terms of embrittlement potential, antimony is the worst, then tin and phosphorous in that order. Tolerable levels for each element would, of course, depend on the alloy chemistry, strength level, grain size, and numerous variables. Because of this, it is difficult to write specifications for impurity elements without regard to the other parameters.

Question: R. Gaitonde—My actual question is that Westinghouse is a turbine manufacturer, and I represent a utility who buys turbines from Westinghouse. Could Westinghouse supply us a rotor now with a more controlled chemistry or do you buy one with a more controlled chemistry that is less susceptible to temper embrittlement than the early '50s or do you still buy to the same specification you had then?

Answer: Dr. Viswanathan—By and large, turbine steels today are of much better quality than those produced 30 years ago and impurity levels have steadily gone down; but we are not at a point where we can lay down stringent requirements in terms of impurity contents, since we don't have quantitative data relating steel purity and the component performance. We had a simple solution to the temper embrittlement problem 15 years ago; we had found that if you eliminate carbon from steels, you will have embrittlement-free steels. The steel manufacturers won't agree to this proposal for some strange reason!

Question: L. Luyckx—The rare earths are known to be making intermetallic compounds with all those tramp elements you were mentioning. I'm sure you've been looking at that before; cerium, lanthanum, and praseodymium produce very high melting point intermetallics where they meet at the grain boundaries with lead, tin, antimony, etc. All those elements are insoluble and they tend to eliminate some of that temper embrittlement. Is that right or wrong?

Answer: Dr. Viswanathan—I am not aware of much published work concerning the addition of rare earth elements to eliminate temper embrittlement. One should be careful to ensure that low melting rare earth

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[14] Commonwealth Edison, Maywood, Ill.

intermetallic compounds (for example lanthanum-nickel compounds) are not formed at grain boundaries, since this would impair the forgeability of the product.

Comment: L. Luyckx—That's right. The rare earths are insoluble as well as some of the other elements such as lead, bismuth, etc., so the only useful way to use them is as getters. In other words, the rare earth metals go after oxygen, sulfur, and those tramps to form intermetallics at grain boundaries. If that's exactly the quantity that is used, then you eliminate embrittlement. If you go beyond that, then of course you create a problem with the lanthanum-nickel lanthanum-chromium intermetallics or eutectics at the grain boundary.

Answer: Dr. Viswanathan—A range of concentrations of the rare earth element may have to be explored to identify the optimum level of the additions.

Comment: R. Anderson[15]—We did some work on a high-temperature alloy indicating rare earths may be used to control *tramp elements*. We added lanthanum to an experimental heat of 901 to remove sulfur. By monitoring the sulfur level of the bath and making a controlled addition of lanthanum, we were able to remove sulfur to a very low level (<5 ppm) and also prevent the occurrence of any lanthanum-nickel intermetallics. We concluded one can control the intermetallic if one is very careful with the addition.

Comment: Dr. M. K. Koul[16]—All your data showed that all these impurities, phosphorus particularly, go to the grain boundaries. And in my boron steel work, I know that the hardenability effect you get is from boron going to the grain boundary. So, I ran an experiment with steel at various levels of phosphorus—0.01, 0.02, 0.04, and at a fixed boron level. And what I found there was that after 0.02P, the boron hardenability effect was not present. The boron hardenability effect, which is due to the free boron at the grain boundaries, was absent due to phosphorus-boron interaction, and so I thought that this would be a very good way of working against the phosphorus segregation, that is, the effect of phosphorus on temper embrittlement, with the boron addition.

And since then, we have run some experiments. Not extensive, but some experiments, where we have added controlled quantities of boron with titanium, zirconium, and vanadium to varying degrees, that is adding vanadium as you have—0.1, 0.2, or so—and together with that vanadium, have some boron, titanium, and zirconium, and we do see some improvement in temper embrittlement.

[15] Universal Cyclops Specialty Steel, Bridgeville, Pa.
[16] Foote Mineral Co., Exton, Pa.

Comment: Dr. Viswanathan—I find the results regarding the beneficial effects of boron on temper embrittlement to be quite interesting.

Structure-Property Relationships for Pearlite-Reduced Mo-Nb Steels Finish-Rolled Moderately Below Ar₃ by A. P. Coldren, G. T. Eldis, and G. Tither

Question: Dr. Abrams—I'm just curious as to how you could put a certain slope to your strength results and attribute so much of your strengthening due to substructure if you didn't actually measure the substructure; for example, how well it developed, what the subcell size was, or something along those lines.

Answer: Dr. Coldren—Just by reasoning. In a zero niobium steel, there would be no precipitation strengthening, so after correcting for grain size differences any remaining effects can be ascribed to substructure.

Question: Dr. Abrams—All right, but how did you come up with the slope of the substructure line? Did you just use a standard Hall-Petch slope and attribute it to that?

Answer: Dr. Coldren—I'm not sure which diagram you're referring to.

Question: Dr. Abrams—You've normalized everything to ASTM grain size 11, you start out with some base strength, and you have a contribution due to substructure and everything left over is due to total precipitation. How do you know what was due to your substructure if you didn't measure how much substructure you had in your final microstructure?

Answer: Dr. Coldren—This is on the assumption that only substructure plus precipitation could cause strengthening after you take out the grain size effect. I can't think of anything else that might have caused the increase. In a zero niobium steel, there can be precipitation.

Question: Dr. Abrams—That gives you your intercept, but how do you know what the slope of your substructure component is going to be, based on the amount of deformation you introduce?

Answer: Dr. Coldren—We just drew a line through the three points. The line is purely empirical.

Question: Dr. Abrams—But you still don't know what the substructure associated with those points is?

Answer: Dr. Coldren—No, I don't. We didn't quantify it. All I know is that's what you get when you have 30 percent ferrite deformed 20 percent. And what else can it be besides substructure if there can be no precipitation effect?

Question: Dr. Abrams—There's something bothering me about how you normalize all these interactions out. Maybe somebody has some comments?

Author's written comment—The only normalization was to correct the yield strength values to a common grain size, ASTM No. 11, using the Hall-Petch relationship with a K_y value of 20 N/mm^2/mm$^{-\frac{1}{2}}$ (0.58 ksi/in.$^{-\frac{1}{2}}$).

Question: J. M. Leitnaker[17]—On your last rolling, last pass, what was the reheating cycle?

Answer: Dr. Coldren—There was no reheating on rolling; it was carried out on a falling temperature.

Question: J. M. Leitnaker—You don't go back up in temperature?

Answer: Dr. Coldren—Once we started rolling, the temperature was falling continuously.

Question: G. Delvecchio—How did you measure your temperature? With a pyrometer or with a thermocouple?

Answer: Dr. Coldren—Embedded thermocouple at the midthickness, midwidth position.

Question: Dr. Koul—If you could find out the amount of columbium in solution, could you find out how much columbium has precipitated and how much has not?

Answer: Dr. Coldren—By extracting and analyzing? Yes, I suppose.

Question: Dr. Koul—The other question I had was, how does this differ from the U.S. Steel work?

Answer: Dr. Coldren—It's quite similar, I believe. Their patented process plus composition—in the processing, as I recall, they recommend rolling to obtain 10 to 40 percent ferrite—deformed ferrite—and their composition is quite similar to our 0.2Mo-0.06Nb steel, except that they had 1.2Mn instead of 1.4Mn. I understand that more recently they have changed their composition, but originally that's what they were using.

Question: Dr. Koul—And I think the amount of deformation that you use below the transformation temperature is to control the amount of splitting rather than anything else; and that the amount of splitting increases as the amount of deformation increases.

Answer: Dr. Coldren—Our results seem to confirm that idea.

[17] Oak Ridge National Labs., Oak Ridge, Tenn.

Question: Dr. Abrams—I'd like to go back to the substructure question. Is it possible that the amount of recovery with the alloyed material is such that at 10 or 15 percent deformation you don't have this dislocation substructure?

Answer: Dr. Coldren—This is not 15 percent deformation. This is volume percent of deformed ferrite. It's all at 20 percent deformation.

Comment: Dr. Abrams—This still doesn't explain what's happening.

Question: Dr. F. B. Pickering—When we introduce a given deformation into a steel comprising a duplex structure, do we know the distribution of strain in the two phases? Presumably, because the material is being plastically deformed, this will depend on the respective flow stress values and work hardening rates of the two phases, and, of course, on their volume fractions. Don't you think that this strain distribution would have a marked effect on the subsequent behavior of the steel?

Answer: Dr. Coldren—At this temperature, you may have a point there.

Comment: Dr. Pickering—The analysis of the effect of microstructural variables on the properties of acicular ferrite structures is complex, particularly when there are recovery substructures, such as subgrains, present. The total strengthening over and above grain size and solid solution strengthening is by no means solely the result of precipitation. In fact, it comprises at least three terms, namely, precipitation strengthening, subgrain boundary strengthening, and *forest* dislocation strengthening. Whiteman[18] has analyzed such strengthening in these terms, but it is also possible that there are further complications due to solute-defect interactions. Moreover, when we have such elements as niobium and molybdenum present, they can markedly retard recovery and thus lead to it being necessary to apply this rather difficult type of analysis. However, the results of Whiteman do indicate that the analysis has much to recommend it.

Comment: Dr. Abrams—That's the thing that bothered me. In our continuum rolling studies, we observed very complete substructure by means of electron microscopy and saw very well-defined subcells, and, with this microstructure, we were getting strength increases like 40 or 50 ksi. Whereas if you just had dislocation tangles and things of that nature, then I think you'd see the types of strength increases you're seeing in your study.

[18] Whiteman, J. A., "Low Carbon Steels for the Eighties," Institution of Metallurgists Spring Residential Conference, Manadon, April 1977: published by the Institution of Metallurgists, London, England.

Controlled Processing of Molybdenum Bearing Line Pipe Steels by G. W. Delvecchio, J. E. Hood, and D. B. McCutcheon

Comment: Dr. Abrams—You could go back to our paper where Dr. Roe presented a plot of pipe yield strength at 0.5 percent offset versus plate yield strength at 0.2 percent offset. This plot showed that at the highest strengths you lose more strength, and Tanaka showed the same thing at the Microalloying '75 Conference.

We tried to correlate the delta strength with chemistry and microstructural variables; and the only thing it correlated with was the initial yield strength. So the point I want to make is that although you get significant work hardening with a high manganese, columbium-molybdenum grade, which we used in our mill trial, you might be at a 90 ksi yield strength level in the plate. But when you make your strap test from the pipe, you might lose as much as 18 or 20 thousand and still come back to a pipe yield strength of 70 ksi.

The other point is that you had said it is beyond the scope of your paper to look at circumferential properties.

Comment: G. Delvecchio—The Japanese have done that to some extent.

Comment: Dr. Abrams—Well, we at Bethlehem also looked at these properties. What we did is this: with reference to the 89 pipe we produced, we chose a continuous yielding material—a high manganese, molybdenum-columbium grade and then a vanadium-columbium grade, which is discontinuously yielding, and we selected several pipe. We took round, machined tensiles at various positions from 12 o'clock, 1 o'clock, 2 o'clock, 3 o'clock, all around the circumference; and then the other thing we did, which is really interesting, was we actually made sheet tensile specimens to represent the outer fibre, which would be in tension, a center section, and then inside diameter, which is in compression. And we got some interesting results.

Although these were preliminary results and, of course, require verification, they do explain a lot about what is happening with these two grades. That is, when you talk about the pipe, the U and O processing, the strain rates, and the amount of strain across the width of the plate, what you see at 6 o'clock, you don't see at 4 o'clock, or at 1 o'clock. And so a grade that appreciably work-hardens is going to be very susceptible to the amount of strain and strain rate that you're putting in, whereas let's say a grade that doesn't, might not. And you can see some unusual results. For example, when your neutral residual-stress axis through the thickness is not at the midpoint.

It's interesting that a lot of people are aware of a lot of the same things and are working on them, but since these things cost a lot of money to do,

it would be good if we could join together and say, "you do this, I'll do that, and share the workload." I just wanted to make these comments.

Comment: G. Delvecchio—Can I refer you to a presentation that was made by F. Christensen at the CIM conference, where we presented some of our results. We've done the same thing. We used round specimens. And the differences that we found—probably the same results—you get a very high yield strength on the outer fiber, and you go down to a very low level on the other side, and it's amazing how much strength difference there is.

Question: F. Logan[19]—Dr. Gray in the Italsider paper indicated that the martensitic-austenitic islands in the manganese-molybdenum steel resulted in inferior sulfide stress cracking resistance. Have you looked at this in your high manganese steels at all?

Answer: G. Delvecchio—We are looking at it right now. I don't think I can divulge any data or tell you what the results are, but it's being done. This may be presented at a later date.

Written answer: G. Delvecchio, J. E. Hood, and D. B. McCutcheon—We have conducted studies examining the hydrogen induced cracking behavior of several line pipe grades using both the Shell[20] and BP tests.

With the Shell test, we examined the sulfide stress cracking behavior of the following types of steel: (*a*) conventional ferrite-pearlite steels, (*b*) partially acicular C-Mn-Mo-Cb steels, (*c*) fully acicular C-Mn-Mo-Cb steels, (*d*) SAW weld metal from the preceding steels (welds were made with Linde 585X flux in combination with both Linde 44 wire and experimental Stelco wire), and (*e*) HAZ from the previous steels.

The Shell test results were compared to those of API grades obtained from literature. These included: (*a*) N80—normalized, normalized and tempered, and quenched and tempered conditions; (*b*) J55—normalized condition; (*c*) P105—normalized condition; and (*d*) P110—quenched and tempered condition.

The results indicated that the resistance to sulfide stress cracking of the steels and SAW weld metals was equivalent to or better than that of the API grades at a given strength level. The sulfide stress corrosion cracking resistance of the heat affected zones was comparable to that of normalized J55 or N80 at approximately the same strength level. As yet, we have not examined the microstructural aspects of the sulfide stress cracking failures in these steels.

For the BP tests conducted on partially acicular C-Mn-Mo-Cb steels,

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[19] Cameron Iron Works, Houston, Tex.
[20] Fraser, J. P., Eldredge, G. G., and Treseder, R. S., *Corrosion*, Vol. 14, Nov. 1958, p. 517t.

we have found that the cracking was associated with inclusions (particularly Type II manganese sulfides). We have not observed cracking related to islands of austenite-martensite constituent. In addition, Climax Molybdenum Company has conducted an excellent metallographic study[21] on samples of C-Mn-Cb-V and C-Mn-Mo-Cb steels cracked using the BP test. They also found that the hydrogen-induced cracks were associated with nonmetallic inclusions and that the main crack paths were not influenced by pearlite or by the presence or absence of martensite islands or bainite.

Question: L. Lucykx—In view of the growing steelmaking challenges with high manganese, low carbon, and low silicon content, I have a question to ask you. What is your *gut* feeling right now on the optimum silicon content and on how silicon affects the properties of your steels. In other words, do you have a silicon specification and what is it?

Comment: G. Delvecchio—For what steel?

Question: L. Lucykx—For these Arctic line pipe steels.

Answer: G. Delvecchio—Our philosophy with respect to silicon is that it is beneficial for developing the partially acicular ferrite structure that we intend to use for Arctic grade pipe. We have produced steels with silicon contents ranging from 0.08 to 0.35 percent, and have shown through regression analysis that silicon significantly raises the strength of the final product without adversely affecting toughness. Thus, we intend to produce steels with a nominal silicon content of 0.25 percent.

Question: L. Luyckx—Have you explained various carbon levels and their effect on properties? Do you feel that extra-low-carbon steels will be required for these large Arctic line pipe orders?

Answer: G. Delvecchio—We have analyzed data from both experimental and production heats of C-Mn-Mo-Cb steel, and have developed regression equations for the effects of carbon and the other alloying elements on the plate strength. For the high manganese heats (1.80 to 2.10 percent), the carbon range was 0.035 to 0.090 percent. Carbon has a very pronounced effect on microstructure, particularly the volume fraction of retained austenite-martensite constituent, which in turn, controls the plate strength (and, consequently, the pipe strength). Although carbon is economical and has a significant effect on strength, it can have an adverse effect on both toughness and weldability. Accordingly, we control the carbon content in balance with the total alloy content. For most of the anticipated Arctic Grade 70 pipe that will be produced, the carbon content will be typically 0.06 percent.

[21] Coldren, A. P. and Tither, G., *Journal of Metals,* May 1976, pp. 5–10.

Comment: Dr. M. Gray—I am interested in the work concerning the strain distribution around the perimeter of mechanically expanded U and O pipe and all the unpublished results that exist. I am familiar with other results that are also in the *banned and burned* category. If one examines the same position on the outer perimeter of the pipe, it is found that for some methods of pipe forming, the local strain (measured by X-ray) varies all the way from about 0.5 to about 4.5 percent for a nominal expansion of 1.5 percent. This could give rise to substantial variation in properties, especially for the acicular ferrite type steels that have rapid work hardening rates.

These observations are rather worrying for those people involved in specifying pipe properties and pipe buyers. I guess you're just as concerned as a supplier with a liability for the final product. I think it would be nice if all the people having results that haven't been published, could get together and see if there is any kind of unanimity and what they really mean.

Comment: Dr. Abrams—Since this is really the last paper on HSLA steels dealing with pipe and the other two don't, I might summarize some of the comments that were made about these steels.

Basically, what we're saying is that there are a variety of alloying systems and processing options that can meet the proposed specifications for Arctic applications. And, depending on your melting facilities or whether you want to restrict yourself to a very low-carbon high-manganese grade with the attendant problems in the BOF shop, I think the whole thing is going to boil down to cost effectiveness. None of us have mentioned the costs here, but that factor is certainly present in all of our internal reports. Of course, cost effectiveness, as a function of properties and microstructure, is bound to play a very important role.

Another point: I don't know if we're all going to participate in whatever lines are going in, but we wouldn't in any event all be using precisely the same grade and processing. As producers, we all have to meet the property requirements, regardless of what particular method we each select.

Question: G. Delvecchio—What is the power of your plate mill? And what is the maximum size pipe you can make?

Answer: Dr. Abrams—Well, we have two 160-in. plate mills. Right now our capacity in our pipe mill is 42 in. diameter, and we probably could make a 48-in.-diameter pipe on our pipe mill if we changed our dies; but then on a 160-in. plate mill the width of the plate does not allow much room between the plate and the side guards. Additionally, there will be shape problems—crown and so on. Consequently, I do not think we would be prepared to participate in pipe requirements beyond the 42-in. diameter. As far as our mill capacities are concerned, the Burns Harbor

Mill is rated at 12 million pounds and the Sparrows Point Mill at 9 million pounds. Although we have at times exceeded these capacities in some of our rolling trials, I do not think we'd want to do this sort of thing on a routine basis. What it amounts to is that one has different restrictions, depending on the mill. We as well as other domestic producers do not have anywhere near the capacities that Italsider or the Japanese have.

However, in designing our processing, we put a great deal of study into mill loads and the time-temperature-deformation on our experimental mill at the Homer Research Labs. We then related our findings to what we could get in our big mills, with the result that we could predict the mill loads so the superintendent could go to sleep at night knowing that we're not destroying his mill. Most of the time we were very successful and got to the point where they wouldn't even be around when we did our rolling—the mill people were quite confident that we wouldn't exceed the mill capacity. With maximum productivity as the goal, we tailored our processing to our mill, just as everybody else does. After all, maximum productivity is a key factor in achieving an economic cost.

Author's written comments: G. W. Delvecchio, J. E. Hood, and D. B. McCutcheon—We have conducted a study to develop an understanding of the stress-strain behavior of both low and high manganese C-Mn-Mo-Cb steels during UOE pipe making. In this study, we machined small tensile specimens (mini-tensiles) from 914 mm (36 in.) outside diameter by 12 mm (0.475 in.) wall pipe at five locations through the pipe wall thickness (Fig. 1). The specimens were only taken at a position 180 deg to the longitudinal seam weld and were oriented transverse to the pipe axis. Mini-tensile specimens were taken both before and after the expansion/hydrotest cycle.

In the as-formed pipe condition, both the low and high manganese UOE pipes showed similar trends, with the inside diameter of the pipe having lower yield strengths than the outside diameter of the pipe. On the pipe inside diameter (compression side of neutral axis), there is a loss in yield strength relative to that of the as-rolled plate due to the Bauschinger effect, while on the pipe outside diameter (tension side of the neutral axis), there is an increase in yield strength due to work hardening. For UOE pipe, the 1.5 percent expansion (followed by a 93 percent SMYS of Grade 70 hydrotest) increased the pipe yield strength and tended to reduce the strength variation through the pipe wall for both the low and high manganese steels (Fig. 2).

The stress-strain curves of the individual mini-tensile specimens were combined mathematically to develop a *composite* stress-strain curve that represents the true stress-strain behavior of the pipe. For the limited data, the composite stress-strain curve yield strength correlated with both the ring expansion (true pipe strength) and the flattened strap yield strengths.
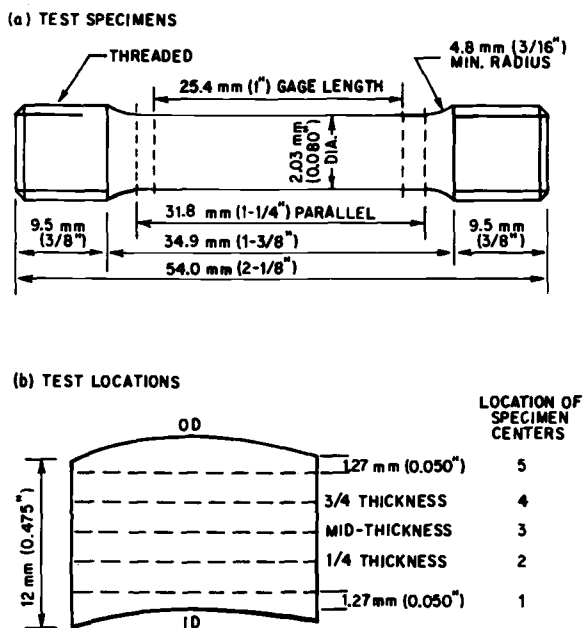
(a) TEST SPECIMENS

(b) TEST LOCATIONS

FIG. 1—*Mini-tensile test specimen.*

Previous studies at STELCO have shown no statistically significant difference between the pipe ring expansion and the composite mini-tensile yield strengths. The limited expanded pipe data from this investigation suggest that the ring expansion and mini-tensile yield strengths are equivalent (Fig. 3). The flattened strap yield strength of as-formed pipe agreed with the mini-tensile yield strength (Fig. 4). For expanded pipe, the flattened strap yield strength was significantly (statistically at the 90 percent confidence level) lower than the minitensile yield strength, indicating that the flattened strap test is a conservative measure of the true expanded pipe strength.

Effects of Composition and Gage on the Microstructure of A533-B Steels by R. P. Smith and R. A. Swift

Question: Dr. Korchynsky—In the range of plate thicknesses investigated, is the austenitic grain size a factor that might influence the hardenability? Could this not explain that in thinner thickness, for example, 120 mm, you have more ferrite than predicted by calculation?
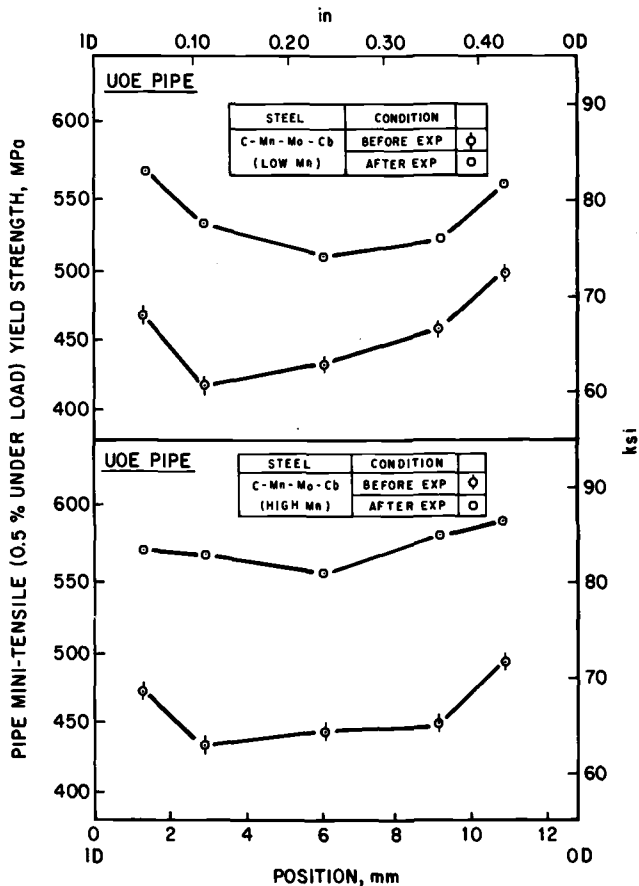
FIG. 2—*Variation in yield strength through the pipe wall thickness.*

Answer: Dr. Swift—Generally, when you take a particular grade of steel, that is not a factor.

At Lukens, we find that grain size of a particular specification is relatively constant. For example, with the Cr-Mo steels, since it is an extremely high toughness material, we use a coarse grain practice to get the higher strengths required of the material while still having acceptable toughness. The opposite is true of Mn-Mo-Ni steels. These steels are produced by a fine grain practice so as to capitalize on toughness since strength is not as critical a factor. I guess that what I am saying is that producers use melting practices that restrict grain sizes over a fairly narrow range so that it is not a critical factor in heat-to-heat hardenability.
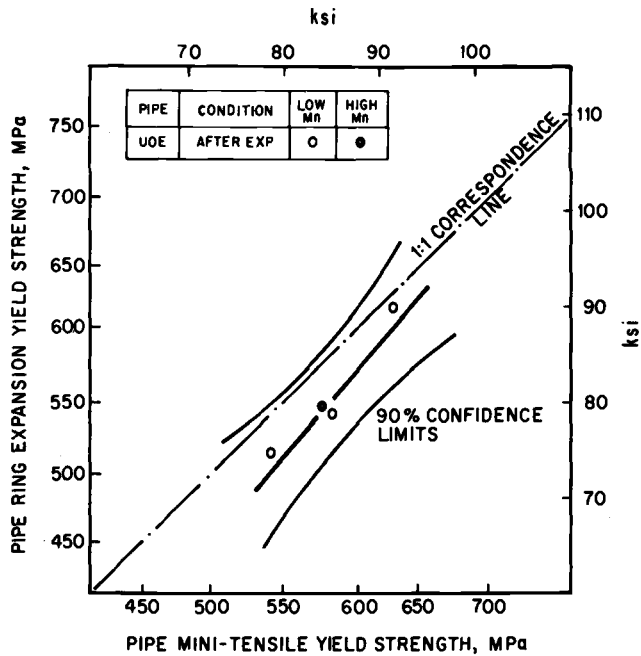
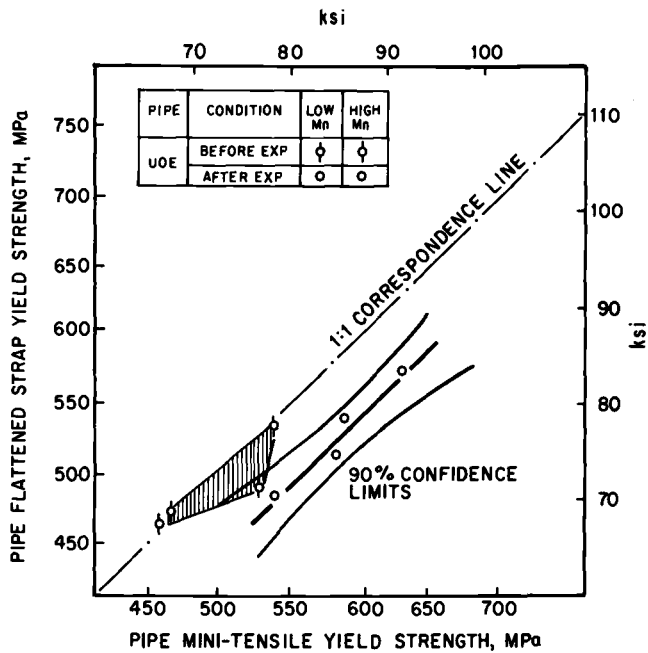FIG. 3—*The relationship between pipe ring expansion and mini-tensile yield strengths.*

FIG. 4—*The relationship between pipe flattened strap and mini-tensile yield strengths.*

High-Hardenability Carburizing Steels for Rock Bits by D. E. Diesburg

Question: W. Mankins[22]—The slide showing your second specimen was for the fatigue impact test. Superimposed on the test specimen was the outline of what appeared to me to be an involute gear tooth. What is the significance of that shape in your study?

Answer: Dr. Diesburg—Originally, the test specimen was designed to simulate a carburized gear tooth and the fillet radius was put into the specimen to represent a radius that might be expected at the root of a tooth.

Question: W. Mankins—The second question I have concerns the performing of the test. I didn't quite understand from your apparatus whether you had the arm strike the test specimen, manually caught it on the rebound, and returned the hammer to its rest position, or if another method was used?

Answer: Dr. Diesburg—The drop height was adjusted to a few inches. The technician had to catch the hammer as it bounced off the specimen after each impact.

Question: W. Dwyer[23]—How did you measure your residual stresses inside the case? I noticed your depths are on the order of millimeters.

Answer: Dr. Diesburg—The residual stresses were measured by X-ray diffraction on surfaces exposed by electrochemically polishing away successive layers of the case.

Question: Dr. Banerji—Would you explain a little bit more, I don't know how you got the fracture toughness, K_{Id} values from the precracked Charpy specimens. Do you have a carburized case there?

Answer: Dr. Diesburg—Several unnotched Charpy-size specimens of each steel were carburized to have the desired case depth. The specimens were notched by electrodischarge machining, each to a different depth. Fatigue cracks were grown at the base of the EDM notches. This procedure produced a set of specimens for each steel having crack tips at various locations in the case. Fracture toughness is a measure of the resistance to crack extension of the material surrounding the crack tip. Therefore, by measuring the loads required to break each set of specimens, it was possible to calculate the fracture toughness gradient through the case. The K_{Ic} values were calculated using slowly applied fracture loads, whereas K_{Id} values were calculated from loads applied dynamically and measured with an instrumented striker.

[22] Huntington Alloys, Inc., Huntington, W. Va.
[23] AC Spark Plug Div., GMC, Flint, Mich.