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poświęcone są publikacjom naukowym z zakresu teorii i badań doświadczalnych w dziedzinie mechaniki i termodynamiki przepływów, ze szczególnym uwzględnieniem problematyki maszyn przepływowych

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exist for the publication of theoretical and experimental investigations of all aspects of the mechanics and thermodynamics of fluid-flow with special reference to fluid-flow machinery

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na temat

TURBINY PAROWE WIELKIEJ MOCY

Gdańsk, 24 - 27 września 1974 r.

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IIIrd SCIENTIFIC CONFERENCE

on

STEAM TURBINES OF GREAT OUTPUT

Gdańsk, September 24 - 27, 1974

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III НАУЧНАЯ КОНФЕРЕНЦИЯ

на тему

ПАРОВЫЕ ТУРБИНЫ БОЛЬШОЙ МОЩНОСТИ

Гданьск, 24 - 27 сентября 1974 г.

PRACE INSTYTUTU MASZYN PRZEPŁYWOWYCH
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Panel Discussion 1

Investigations of Two-Phase Flows and Their Influence on Turbine Stage Designing

Prof. D. J. Ryley, Chairman. The title of this first discussion panel is: "Investigations of Two-Phase Flows and Their Influence on Turbine Stage Designing". I anticipate that we may have more questions that we can handle and the way I propose to do it is this. After the three initial speeches have been made there will then be the opportunity for questions and answers from the floor. I shall circulate a number of small pieces of paper. On the top of the paper you will put your own names and then you will put the name of the author on whose paper you wish to speak. These pieces of paper will then be selected at random.

Prof. D. Barschdorff: I have been asked to give a short introduction to the main qualitative achievements in recent investigations of condensation and condensing steam flow. Talking about condensation, I want to restrict myself to the process of homogeneous condensation from the vapour phase. We have had a period of intense activities and research both experimental and theoretical in this field within the last years. The research in the field of steam condensation is based on the former investigations of Stodola, Binnie and Green and Oswatitch.

What have we been able to add to their early knowledge?

Nozzle flow of steam was regarded to be a representative substitution for the steam expansion in a real turbine. With, of course, the exception that no work is extracted from the fluid in these experiments and all condensation zones so far have been observed in the supersonic part of Laval type nozzles, while in turbines of course spontaneous condensation also occurs at subsonic velocities.

The influence of the expansion ratio on the size of droplets originating in the condensation zone was investigated. Droplets of the size of 500 to 4,000 Å were found at the end of the nozzles. Triggered by these findings a new insight into the erosion process was gained. The highly dispersed fog is assumed not to be responsible for this phenomenon but rather the impact of larger droplets on the blade surfaces.

Then gas dynamic interactions between flow parameters and condensation were investigated and adiabatic shock waves occurring in condensation zones and unstable flow patterns, as we have seen in Dr. Saltonov's lecture this morning, were investigated and can be predicted theoretically.

New insight into the theoretical connections of condensation was initiated by an article by Lothe and Pound in 1962 (also an earlier article by Kuhrt in 1952), who claimed the classical theory of homogeneous nucleation not to be applicable because of unsufficient model assumptions. In their treatments droplets are regarded as macromolecules with rotational and vibrational degrees of freedom, which adds additional terms to the nucleation formula. For steam condensation, their values are beyond the scope of experimental observations. It took about 10 years until again the classical nucleation theory gained in confidence, however with a correction for non isothermal effects in the pure vapor (steam) case. The classical theory, therefore, is not "verified", but seems to be a fairly adequate formulation, yielding results in accordance with experimental findings.

If we want to describe a flow process with homogeneous condensation, we need a set of differential equations. I have written these on the blackboard:

1) the equation of state,
2) the continuity equation,
3) the impulse equation,

4) the energy equation,
5) the droplet growth law and
6) the nucleation rate equation.

I want to mark our different confidence in these equations with the school grades 1 for the best and 5 for the worst confidence or applicability. The equation of state I would mark 2. The continuity, impulse and energy equations I would mark 1, we are quite sure how to apply these fluid mechanics equations in such a flow process. The droplet growth law, I thought, we could mark 2, but I am somewhat unsure now because of Prof. Puzyrewski's discussion. The nucleation rate equation I mark 5 because we indeed do not know what is going on in detail here. Parameters like the surface tension σ or heat and mass transfer coefficients strongly influence the results of numerical calculations and are still treated in a more or less empirical way.

May I sum up with a brief outline: We do not know yet whether these findings from the nozzle experiments are relevant to the actual process of condensation in the steam turbine. So far the results have not been included in the process of turbine stage designing. There are other theories, one by Prof. Filippov, who observed condensation in vortices and wakes behind cascades and Dr. Gyarmathy, who calculated pressure and temperature fluctuations in cascades and it seems that the condensation zones are distributed over a larger part in the turbine. However, I would say, all these investigations have highly contributed to our knowledge of the behavior of condensing flow of steam and to our understanding of the flow process in the steam turbines.

Chairman: Now dr. Gyarmathy will speak on "The Evolution of Last-Stage Turbine Designing in the Past Decades".

Dr. Gyarmathy: I feel my contribution is already prejudged by the choice of the title for our panel discussion. If the evolution of the last stage turbine designing is to be reviewed in this context, then, I think, we imply an affirmative answer, namely, that the investigations of two-phase flows did have a great impact on the design of steam turbines. Now, let us look at this suggestion with a question mark in mind: how much truth and how much optimism is there in this statement?

If we want to examine the recent history of steam turbines operating in a wet steam region, we only have to look back to two days ago when Mr. Somm in his paper summarized the main trends and the main problems. Let me very briefly repeat these trends: the electric networks are increasing, therefore larger units with larger exhaust area sizes have to be designed, therefore increased blade length and high peripheral speed became typical to modern turbines. The problems which this evolution has brought along are multiple. I try to recall the review given by Mr. Somm: static mechanical problems, dynamic mechanical problems, metallurgical problems, the problems of manufacturing, then, down in the list, aerodynamics, and quite at the bottom of the list: erosion. As we see most of these problems have not very much to do with two-phase flow. Perhaps aerodynamics is involved here if we are very earnest about it but turbine aerodynamic design is based mainly on concepts derived from dry flow studies. In the domain of the erosion protection we are, of course, in the sanctuary of wet steam and two-phase flow studies. Let us, therefore, review the measures incorporated in modern steam turbines for protecting the blading against erosion. Have the investigations of two-phase flow phenomena really had an important effect on the evalution of these measures or not?

I tried to summarize some of the erosion protection measures in table 1. These are: traditional passive measures (like shielding), water removal, better atomization and better dispersion of water, reduction of deposited water mass (because we know, that only water which has been deposited on blades is responsible for erosions) and some special design features. Let us look at the question: how did people invent these measures? Was it engineering intuition or was it the result of intense scientific studies of two-phase flow? As indicated by crosses in the middle columns, about half of the measures were invented by intuition and another half thoroughly involved two phase flow studies.

In the next columns I tried to point out how generally these measures are used. Dots in the left column mean that they are widely used by almost all manufactures, those in the middle indicate that they are used by one or two manufactures only, and the dots in the right column show that their application is in an experimental stage. You can see that crosses on the left and dots on the left almost always

Table 1

Design measures used for erosion protection

Measure	Conceived thanks to: engineering intuition	Conceived thanks to: intensive two-phase flow investigations	Used: generally	Used: by some manufacturers	Used: only experimentally	Is it beneficial for turbine efficiency?
1. Traditional passive measures – hardening	×		●			
– shielding	×		●			
– grooves (General Electric)	×			●		
2. Water removal						
– extraction belts	×	x	●			●
– suction slots on blades		×		●		
at casing		×		●		
at trailing edges		×			●	
– internal separators	×	×		•	●	
3. Better atomization and dispersion of water						
– increased axial gap	×	x	●			
– avoiding flow separation	×		●			●
– blowing at trailing edge	×				●	
– fences on trailing edge		×			●	
4. Reduction of deposited water mass						
– production of fine fog	by-product of econom. design.		●			●
– heating of stator blades				•		
– optimization of cascade geometry	×	×			●	●
5. Special design features						
– Baumann stage	×			●		
– special cascade geometry	×					

Large cross or dot = "yes", small cross or dot = "to a limited extent only"

coincide. There is virtually no method generally used for erosion protection which could not be attributed to engineering intuition!

Therefore, summing up, I come to two conclusions which I would like to express in a pointed manner, hoping to provoke a heated discussion: The general evolution of turbine stage design has been relatively slightly influenced by scientific investigations of two-phase flow. This is my first statement. The second statement is that erosion protection relies mainly on engineering intuition, on material technology and on operating experience and only to a relatively insignificant extent, at least at present, on scientific insight into two-phase phenomena. Thank you.

Chairman: Mr. A. Smith of Parsons will speak on: "The Possibilities of Assessing Water Damage in LP Steam Turbines".

... to speak on this particular topic, I would like ... all large steam turbine builders. This interest results from the increasing proportion of large turbines now being ordered and the fact that many of these are supplied from water moderated reactors where the steam condition in the HP turbine is wet. It is of prime importance, both to the customer as well as the turbine builder, that these machines should be extremely reliable and a limit to this reliability could be blade or joint erosion, which explains the attention being paid at this moment to this problem.

Site experience would indicate that erosion in wet HP steam turbines is perhaps not so severe as we might have once thought, however, we must remember that there are several types of water moderated reactor, and turbine operating experience from these can differ. This may, to some extent, be observed by some manufacturers providing more cylinder water extraction stages than others. It may also be significant that fewer water extraction points are evident on those turbines supplied from pressurised water reactors where the steam is not radioactive than those supplied from boiling water reactors. This could either be explained by differences in the blading design or by ionisation influencing nucleation rates, and subsequently droplet sizes and coarse water distribution.

On the question of erosion protection in low pressure turbines, I consider that the problem has been exaggerated, and certainly it would seem that conventional methods of shielding or hardening the moving blade leading edges have succeeded in protecting these turbines. This point was illustrated earlier when I spoke of predictions of erosion shield lives of only 3 months on the Ferrybridge 500 MW machines, when in fact they are still operating satisfactorily after some 10 years. The reason for this low erosion rate in large high speed LP turbines may be partially explained by the decrease in size of potentially damaging droplets resulting from higher expansion ratios, but better atomization during the film stripping process from nozzle blades is also thought to be important, since the higher acceleration of these smaller drops reduces both their impact velocity with the following moving blade, as well as the duration of stress levels in the material.

I would like to mention another point which arose today, which could form an important topic for future investigation.

One of my Polish hosts gave a paper this morning on 'Nozzle Heating in LP turbines', which I understand has been incorporated in the Biblis LP turbines in Western Germany. It would appear that steam heating has been employed through hollow last stage nozzle blades so that potentially damaging surface water formations can be evaporated, should this prove necessary. One must also consider, however, the effect this may have on the aerodynamic performance of the nozzle. Although heating rates are possibly smaller than at first thought, because of the small proportion of the total moisture being deposited on the nozzles, I suspect that the evaporation process will result in a significant thickening of the boundary layers on the blade surfaces, therefore making them more susceptible to separation in zones of adverse pressure gradient. Thank you.

Discussion

Prof. J. Krzyżanowski to dr. G. Gyarmathy

We are witnesses of two quite oposite opinions. Two days ago Prof. Filippov expressed an opinion that we are facing a great progress in the field of two-phase flow investigations applied to steam turbine designing. Today dr. Gyarmathy showed us that it is not the case and that we did not contribute anything at all to the progress of designing. I would like to ask dr. Gyarmathy to be more precise on this point. I think this question is similar to the question which is very often posed in connection with the role of aerodynamics when applied to turbine designing. Some people express very positive opinions on the expected impact of applied aerodynamics on the design of turbines and on new prospects of aerodynamics due to computer applications. Some neglect it and claim that the potential improvements of the turbine blading are already exhausted and further refining of turbine aerodynamics is economically not justified. I think this question may be extended also to the second part of our discussion today. Dr. Gyarmathy, could you try to be more specific on this point.

Dr. G. Gyarmathy: Unfortunately due to language difficulties I did not realize that Prof. Fillipov was really so very optimistic. But I think there is no real discrepancy between these two statements, at least in the formulation cited by Prof. Krzyżanowski, because the optimism refers to progress in studying two-phase flows. My comments refer to the impact of these results on the design of turbines. I think we are at the stage where the results are accumulating but they have not produced yet that kind of influence on turbine designing which possibly they could.

Responding to the invitation by Prof. Krzyżanowski let me try to examine this question in a more detailed way, by using my table 1.

I do not think I have to spend very much time on talking about the first category of passive measures. In the second category – water removal devices – there are rather many domains where two-phase flows investigations have played an important role, however, these investigations refer more directly to the cases of suction slots and not to the simpler case of extraction belts which are today widely used. (There may be though some difference in this respect between steam turbine designing and manufacturing in the Soviet Union and in the Western countries.)

The third category, better atomization and dispersion of water, achieved by creased axial gap width, is very beneficial. This very little red sign here would indicate that we owe this measure to the investigations of two-phase flow phenomena. However, it is a geometric necessity to provide a large gap in turbine blading and therefore, I think, this measure cannot be attributed to two-phase flows investigations. Fences on trailing edges are, however, a result of such investigations, but they are merely a proposition rather than something widely used, so we see them in the column "exceptional". (The idea was due to detailed visual examinations of water flow movement on blades performed in England.)

In the next category, which deals with measures aimed at reducing the mass of erosive water, we see first the production of a fog consisting of fine droplets. Theoretical investigations of turbine two-phase flow show that finery of fog is beneficial for both erosions and efficiency. In practice, however, particulary high values of expansion rate, which are necessary for the formation of fine fog droplets, are realized only unintentionally, if at all. I mean here the use of blading of small chord in the first LP stages, which is selected due to economic considerations and which probably reduces erosion when fog formation occurs in this part of the turbine. Further measures, like special cascade geometries or Baumann stages, are not generally used and have for the most part been intuitively invented.

So, I think, we must conclude that detailed two-phase flow investigations, though being helpful in assessing and explaining phenomena, did not directly initiate widespread use of special design measures. Rather, such measures have mostly been introduced as a result of engineering intuition.

Chairman: A question to be asked by dr. Bakthar on the paper by dr. Soltanov.

Dr. F. Bakhtar: I would like to apologize first because, due to the language difficulty I must have missed important parts of Dr. Saltanov's paper. So please will you forgive me if I am asking you about a point that has already been explained.

You explained in great mathematical detail the observation that instationary wave patterns can exist in the sub-sonic parts of convergent-divergent nozzles. I am very puzzled by this. I would be grateful if you would kindly enlarge on this by explaining in simple language the basic physical processes that lead to such instationary flows.

Dr. G. Soltanov: I think there is a misunderstanding. The experimental results that have been presented in my paper indicate the existence of nonstationary shock waves in transonic region where the Mach number was about 1.05 to 1.15. In our experiments we have found, like Dr. Barschdorff, that supercritical heat release causes nonstationary shock wave. In such a case three types of crises might happen, I would say.

The first one is the appearance of stationary shock wave, so called thermal choking.

The second crisis is that one when nonstationary shock wave appears and moves upstream. However at that time the rarifying waves propagation influences the shock and the last one disappears before the throat is reached. This phenomenon has been confirmed by the experiments and calculation as well. In such a case the mass flow rate can be calculated with the assumption that vapor is fully subcooled.

The third crisis, we have been observing, as well as Dr. Barschdorff, when the shock wave passes through the throat of the nozzle and disappears in the subsonic part. It is obviously connected with the fact that the time of the shock motion in this case is higher than the time of shock disappearing due to the influence of rarifying waves.

In the subsonic nozzle the shock waves moving against the flow have been observed when the rotor passed the outlet of the nozzle at the Mach number $0.6 < M_0 < 1.0$, relatively small axial gap and $w/c_0 > 0.1$.

The reason for that was the shock type entry of the cylinderlike rotor leading edge into the transonic jet from the subsonic nozzle.

Chairman: The next question is one from Mr. Keller to Mr. Smith.

Mr. H. Keller: I want to make some remarks on the contribution by dr. Smith. The first is that my company the Kraftwerk Union in Germany does not use heated vanes in nuclear power stations. But for test purposes we used heated vanes in a two outlet flow turbine at Vienna. This turbine has been in operation for approximately one year and a half and we had already an opportunity to look for the success of this measure because one flow has a heated vane and the other one has none. We saw that there was a distinct difference in erosion of both flows, the heated flow had a better behaviour than the other one.

The next point is that I fully agree with dr. Smith that the erosion threat has been in my opinion exaggerated in the last years. We have unprotected turbine stages in operation without any damage being done to these turbines with rather high back pressure and we intend to use this practice also in future because we are of the opinion that under normal circumstances at a back pressure of more than 0.08 bar or so we do not need any more hardening of the blades because we think there are quite a lot of passive methods to prevent erosion. Two of these turbines without any hardening (shielding) or any protection measure are in operation for approximately 6 years and we do not see any severe erosion damage in these turbines.

I also want to say that we generally see the possibility of using even less erosion protection measures in nuclear turbines than in conventional ones because the part load erosion attack is small due to the characteristics of the reheater, which leads to higher steam temperatures at lower load.

Mr. A. Smith: I would like to thank Mr. Keller for his very interesting contribution. It does not really call for a reply other than to apologise for having confused the turbines fitted with heated nozzle blades.

The problem of part load is of course, of great importance and, although under steady part load this may result in a reduction in moisture levels, machines subjected to frequent load changes such as these on two-shift operation can suffer considerably from erosion because of condensation on cooler parts.

Mr. H. Keller: Those machines are in the Brawnkohlen Revier near Cologne. They have blade lengths of approximately 800 mm and the back pressure is 0.06 or 0.07 bars. These particular turbines have a relatively low pressure in the reheater, so that the expansion line is by this reason more on the right side of the Mollier chart, but still with a final wetness of 8 %.

Chairman: Dr. Barschdorff poses his question to Mr. Somm.

Dr. D. Barschdorff: Mr. Somm, I would like to ask a question which is related to your paper you have given here. It is also related to the question whether nozzle experiments have certain impact on turbine stage designing. What has been reported here several times is the fact that the flow field in model turbines and also in the real turbines is fairly well known due to the accurate measurements between the turbine stages. This means that one uses pressure and temperature probes and measures in radial direction the flow parameters within the turbine stages. As far as I know no measurements have been made inside the curved flow channels between the blades. We all know about the difficulties in obtaining reliable measurements in two-phase flow and especially in super-cooled vapour because as soon as we put a probe into a super-cooled vapour surrounding, we will have condensation on the probe surface and in this way we disturb our measurement. For instance, if we measure the temperature profile in a radial direction with a probe in a turbine, we could measure variation in temperature and from a certain distance on we measure a constant temperature. We deduce from this result that the probe is exposed to a wet steam flow, to a droplet flow, which in reality might not be the case. I wonder whether you have developed more sophisticated methods of measurement so that it is really established that not a super-cooled vapour is existent at the entrance of a curved flow channel. It means that you never observed a flow process with super-cooled vapour entering a blade stage?

Mr. E. Somm: We have made measurements in a test turbine upstream and downstream of the last stage.

In a 600 MW machine and in a 300 MW machine the measurements were only taken at the outlet of the last stage. The agreement between the measurement and the three-dimensional flow was rather good in this area. In the test turbine we can do measurements in between the fixed and the moving row of the last stage. So we never had subcooled steam entering the stage. Perhaps that is the difference between your and our measurements. Are you satisfied?

Dr. D. Barschdorff: I have to be.

Mr. E. Somm: I have a question to dr. Gyarmathy. If you want to be successful in selling turbines there are three requirements, first, your machine must be reliable, secondly, your efficiency must be high, ing two-phase flows. My comments refer to the impact of these results on the design of turbines. I think we are at the stage where the results are accumulating but they have not produced yet that kind of influence on turbine designing which possibly they could.

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Chairman: The next question is one from Mr. Keller to Mr. Smith.

Mr. H. Keller: I want to make some remarks on the contribution by dr. Smith. The first is that my company the Kraftwerk Union in Germany does not use heated vanes in nuclear power stations. But and finally the price must be low. Now, gentlemen, it is your duty to reach this goal. And if dr. Gyarmathy tells you that up to now the influence of all these studies of two-phase flows to the wet steam turbine design was very small, this must be changed in the future.

Chairman: A supplementary question to dr. Saltanov asked by dr. Dvořak.

Dr. R. Dvořak: It is a very well known fact that the transonic flow in the region given by dr. Saltanov is unstable even if there is no condensation. My question is whether he has made, in one facility, any comparison between the flow without condensation and with condensation. In other words, whether he could specify how much these oscillations are due to the inherent instability of transonic flow and how much they are due to condensation.

Dr. G. Saltanov: We didn't notice any instability without condensation in our supersonic nozzle.

The text of the Panel Discussion contributions has been prepared for the publication by Mrs. Barbara Markiewicz and Mr. Mariusz Adelt.