ON THE CORRESPONDENCE BETWEEN THE ENERGY CRITERION OF FRACTURE AND MATHEMATICAL MODELLING OF STRAIN PHENOMENA AT THE END OF CRACKS

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In recent years a detailed analysis has been given and a complete elucidation was achieved of the meaning of many papers published on crack theory.

The investigation of phenomena of crack propagation in brittle solids is connected, on the one hand, with combinations of methods and the formulations of classical elasticity theory, and on the other hand, with taking account of some special physical effects associated with the formation of dicontinuities within the deformable solids.

The complexity of this problem generates difficulties in comprehension and some confusion among specialists accustomed to be active in just one of the mentioned scientific directions.

A paper by Raizer [1] has appeared in the "Uspekhi Fizicheskikh Nauk" (*) which attempts to clarify for the readers of that journal the state of crack theory, and simultaneously to refute a number of published critical remarks [2–5].

A number of well-known statements from crack theory are explained correctly in the paper of Raizer.

Meanwhile, this paper can lead the reader away from a correct understanding of the fundamental simple ideas on which crack theory is based

To clarify the essence of the matter, let us examine the problem of plane strain of a body having an elliptical cutout (Fig. 1). Let the ratio between the ellipse semi-axes be $b/l \ll 1$.

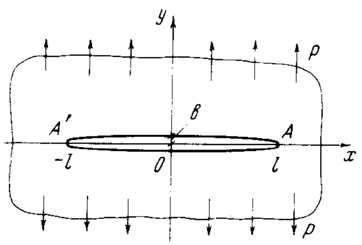

Fig. 1

Let p denote the external tensile stress "at infinity". Normal and tangential stresses vanish at the surface of the hole.

Let us first examine this problem within the framework of the model of an elastic body for adiabatic processes (**) (only the equilibrium equations in the presence of a definite finite connection between the stress and strain tensors, Hooke's Law, must be solved). As is known, this problem can be solved by two methods in the theory of small strains.

I. The "exact" solution in which the boundary conditions are satisfied on the contour of the cutout in the initial unstrained state.

II. The approximate linearized solution in which the boundary conditions on the contour of the elliptical cutout are reduced on the basis of the property $b/l \ll 1$ and are satisfied on two sides of a segment $(-l, +l)$ of the x-axis.

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*) Editorial Note: "Progress in Physics", the U.S.S.R. Monthly.

**) An analogous situation holds for isothermal processes if the free energy replaces the internal energy.

III. Within the scope of elasticity theory, this problem can be considered in a more exact formulation than I. without changing the equations of state or refining it (nonlinear elasticity theory), when the boundary conditions on the cutout are satisfied on the strained surface.

This last problem is mathematically difficult, but this is negligible from the viewpoint of the physical essence of this formulation. This problem has a unique solution in the mathematical formulations I and II, and is easily solved for any p (l and b are given constants). For small p the solutions in formulations I and III are quite close.

The solutions I and II are close everywhere with the exception of small neighborhoods of the points A and A' (Fig. 1). At these points the stress components σ_{yy} are always finite in the solutions of I, and always tend to infinity in the solutions of II on approaching the point A, according to the law

$$\sigma_{yy} = \frac{K}{\sqrt{2\pi(x-l)}}, \qquad K = K(p, l, E, \nu) \tag{1}$$

where E, ν are the elastic modulus and Poisson's ratio, respectively.

For these two solutions the character of the distribution $\sigma_{yy}(x)$ along the x-axis is shown in Fig. 2. It is hence clear that the approximate formulation II results in a large numerical error near the points A and A'; however, it should be emphasized that the results of the solutions of I and II are close outside small neighborhoods of these points and that the total elastic energy calculated for the solutions of the appropriate problem of a finite body is practically identical in the formulations I and II. Moreover, it is very important that this energy be different for fixed b and different l.

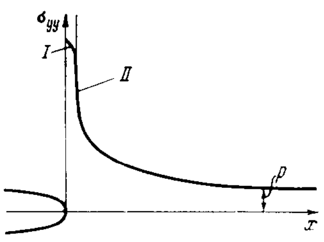

Fig. 2

The first fundamental deduction hence follows:

1. To solve the elastic problem, linearization is not applicable near the points A and A', but it yields a correct result for the total energy.

Let us now turn to the question of the correspondence between theory and experiment for materials whose strain is described within the scope of models of elasticity theory.

For very small p the solution computed by method I corresponds well qualitatively and quantitatively with experiment and describes the effect of stress concentration near the points A and A'.

Computations by method II differ from the computations by method I only in close proximity of the points A and A', where this computation yields clearly erroneous results.

The second fundamental deduction hence follows:

2. The unboundedness of the stresses at the ends A and A' in the method of the Solution II is not the result of deficiencies in the physical formulation of the problem, the physical modelling, but is a result of the approximate method of solving this problem.

For small p the problem of stress concentration is solved by elasticity theory methods in the formulation I. The appearance of infinite stresses near the slit ends is not confusing in the formulation II, and it does not come to mind to use the formulation II with the introduction of suitable artificial surface cohesive forces applied to the lips of the slit and assuring finiteness of the stresses to achieve the "physics" of the modelling. It is quite clear that the approximate method of the Solution II is inadmissible for the description of the details of strain near the ends where stress concentration occurs.

If it is required to calculate the total elastic energy, then despite the approximateness of the method of Solution II, a true answer can be obtained by using this solution. And in particular, the difference between the energies for two values of the slit length l and $l + \Delta l$ can be found quite accurately.

The above is simple and evident for an elliptical cutout, however all these deductions are conserved for thin cutouts of any shape with finite curvature in finite solids under a complex system of external loadings.

Let us consider yet another idealized case when the edge of the initial cutout is, in the absence of loadings, a corner or caspidal point within the scope of continuum mechanics. In this case, the stress components are generally infinite at the corner point in the formulation I, but if we turn to the more exact formulation III, it can be shown that for the stresses there is no singularity with an asymptotic law of the form (1) in the solution of this elastic problem near a cusp.

Stresses at a caspidal point will be infinite with an asymptotic law of the form (1), in the solution based on the approximate formulation II.

Therefore, for sufficiently small external loadings w i t h i n t h e s c o p e o f e l a s t i c i t y t h e o r y , the problem can be posed and solved in good conformity with the reality of stress concentration near the sharply curved parts of the contours of the body boundaries. In this connection a complete highly developed area of elasticity theory devoted to these problems exists. The formulation II is not applied in this area, and naturally no surface cohesive forces are introduced on the body boundaries.

What happens when the external loadings of type p increase? As p grows the magnitudes of the stresses and their gradients increase at the stress concentration locations. As is known, for sufficiently large, sharply changing stresses the mechanical macroscopic properties of real solids change, plasticity effects start to appear, diverse defects originate in the structure of the macroscopic particles, etc.

In this connection, as the loading p grows the mechanical state of the particles at the stress concentration locations is not described within the scope of Hooke's linear elasticity theory in either the exact formulation I or the ultra-exact formulation III even long before the appearance of the actual discontinuities (i.e. crack growth). However, for brittle solids experience shows that the equations of state are violated only in quite small domains near the edges of the developing discontinuities.

At the present time there are only few theoretical results on the structure and mechanisms of interaction within domains with high stress concentrations.

However, it can be affirmed with complete certainty that this mechanism may be different; this can be associated with the fracture energy γ in the origination of new areas of discontinuity for brittle fracture (silicate glass, quartz) or with the formation of thin plastic strain layers on the lips of the discontinuity for quasi-brittle fracture (metals in which $\gamma_{e\phi\phi} \gg \gamma$). The size of the domains in which the model of an elastic body is not applicable, can be on the order of the interatomic spacings for some materials, and on the order of millimeters, or even centimeters, for others.

The shape of the plastic domains can also be different; in the plane problem these domains can have a shape similar to a circle or to a narrow layer of length d commensurate with the slit length $2l$; $d/l \sim 1$ or $d/l \ll 1$. These plastic layers can be arranged as a continuation of the crack clearance or can be arranged symmetrically at an angle $\sim 45°$ to the crack direction, etc. [6, 7].

In this connection, let us note the paper [8] in which an exact solution is given of the elastoplastic problem for the antiplane strain of a space containing a slit, at whose ends finite plastic domains are formed. Not only is a solution of the elastic problem given in this paper, but the shape of the plastic zone is also found, and the stress and strain field within the plastic domain is constructed.

In addition,in the paper [9] a numerical solution of the elastoplastic solution in the case of a plane stress state and plane strain under tension was obtained. Let us mention the interesting fact that if the Dugdale hypothesis were verified in the plane stress case, then the plastic domain would be a thin ellipse at the end of the crack with a major axis perpendicular to the line of the crack in the plane strain case. There exist also analogous papers abroad. All these researches show that the structure of the edge of the crack does not at all agree with the model in which cohesive forces act on the lips of the crack clearance.

It is remarkable that the fundamental problem on crack development and stability in brittle bodies turns out to be simpler when the model of an elastic body at the location of the stress concentration is inapplicable than the problem of high stress concentration without fracture (*) and of the generation of fracture. This jump in the theory turns out to be possible because of application of the energy equations in which global energies occur.

It turns out to be possible to calculate these energies with an accuracy needed in practice by using approximate methods in which it is not required to find the detailed mechanisms of the strain and stress fields at the locations of stress concentration.

Starting with the universal energy conservation laws and thermodynamic principles, a large number of specific examples with analogous situations can be indicated in physics and mechanics.

Thus, in many cases in classical gas dynamics the shockwave structure need not be known. To determine the ideal efficiency of a Carnot cycle it is not required to know the properties of the working fluid or the arrangement of the appropriate machine, etc. Let us recall that the great advantage of extensive utilization of such "ideal models" as a material point, absolute solid, etc., is explained by the insignificance of many details in a number of fundamental problems.

The general assertion that in every real success in the cognition of nature we encounter this kind of insensitivity to ever-existing details and mechanisms hidden from us, is probably correct.

The establishment of the remarkable governing effects, and their physical characteristics on which crack propagation depends, is contained in papers [10 – 13].

Their fundamental idea is associated with the fact that independently of the concrete mechanism of physical interactions at the ends of the slit, the total expenditure of energy in rupture, a characteristic alien to the model of an elastic body, is of fundamental value. At present there are no theoretical computations of this energy in the physical modelling of fracture, but it is determined easily from tests.

According to Irwin [11, 12], the energy expenditure in fracture is expressed in terms of the critical value of the constant K_c in (1). Irwin introduced the constant K_c as a physical

*) The stress concentration associated with the appearance of complex properties of the material not described by Hooke's law is understood here.

characteristic corresponding to the propagation of fracture at $K = K_c$ and the conversion of the slit into a crack thereby. The coefficient K originates as a mathematical characteristic of the asymptotic behavior of the elastic field in the approximate linearized solution of problems of elasticity theory in formulation II. The mechanical meaning of K_c is made apparent from the energy equations.

This approximate solution does not describe details of the strain near the ends of the crack, but its asymptotic behavior near the ends of the crack, associated with the value of the constant K_c, correctly determines the expenditure of the volume elastic energy and the influx of external energy which figure in the energy equations for the body as a whole in describing crack propagation phenomena.

No refinements or additions have been introduced in the formulation of elastic problems of cracks in brittle solids since the time of Griffith and Irwin, and most importantly this is the only theory corresponding to experiment for the solid as a whole when the domain in which elasticity theory is violated is negligibly small.

Now, let us turn to remarks concerning the ideas advocated and expressed in the paper by Raizer.

1°. Firstly, the elasticity theory problem of a thin elliptical crack formulated above is discussed on pp. 329 and 330. Raizer writes: "... stresses and strains near the edge of the crack are infinite for *any* (therefore even for an elliptical slit in the problem of stress concentration in the absence of discontinuities. E. M.) finite loadings and slit dimensions. Since a real body sustains only stresses which do not exceed a definite limit, it hence follows that a body weakened by a slit shoud be fractured for any small loadings." (*)

Raizer sees a fundamental contradiction here, and does not notice that this circumstance is connected with linearization. Further he writes: "This question interested many physicists and mechanicians who tried to explain the appearance of physically improbable singularities of the crack edge which follow from the theory of roundness of the profile and unboundedness of the stresses." Following Barenblatt and some other authors, Raizer repeats this idea many times and considers these "physically inadmissible" phenomena as the result of unsuitable physical modelling. The meaning and purpose of constructing perfected models are seen to be the introduction of actually-existing physically-real appropriate cohesive forces applied to the lips of the slit at its edges *while retaining the approximate formulation II for the solution of the elastic problem.* (**)

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*) This erroneous assertion is a basis in the paper by Raizer and in papers of other authors. The authors forget that within the scope of elasticity theory there are diverse mathematical solutions of problems (rough approximate or more accurate) for the same physical formulation.

**) Let us emphasize that cohesive forces are introduced in the modelling they proposed, which are applied to the lips of the discontinuities already formed for $x < l$ (l is the coordinate of the end of the slit). The work of precisely these cohesive forces (not acting on δS) is considered as the governing energy expenditure when the slit area is enlarged by δS. Later, the doubtful assumption that the work of general forces on a newly formed element of the discontinuity is a higher order infinitesimal than δS is utilized.

In this regard Raizer writes: "The introduction of cohesive forces allowed an explanation of the reason for the existence of infinite stresses (! *E. M.*) at the ends of the cracks, which are inherent to the energy approach,the elimination of these unreal infinities (! *E. M.*) in a physically correct manner, and the clarification (? *E. M.*) of details of the configuration (profile) of the ends of the cracks where, intrinsically, the process of material fracture takes place. "

However, the most important conclusion is that without these cohesive forces, the infinities obtained in the approximate solution by the method of Solution II, are not the result of physical modelling but the result of the approximate mathematical computation because of the inadmissibility of the linearization method and the violation of Hooke's law in some domain at the crack edges.

There is therefore direct misunderstanding of the fact that the sins of the method of computation are not the sins of the physicists and that these calculational inaccuracies cannot possibly be corrected by means of the "cohesive forces".

It can certainly be said that the motivation they present for introducing a model with cohesive forces by referring to infinity in the linearized problem is just a "psychological motive". In reality, independently of this, it could be more correctly said that such modelling is proposed of the fracture edge for which everything is "satisfactory" from the physical viewpoint, and also, most importantly, for which the linearized formulation II becomes completely acceptable.

In this connection it is evident that for very small external loadings, when the problem formulations I and III of the theory of elasticity on stress concentration are absolutely valid and correspond well to reality, no cohesive forces applied to the lips of the slit are needed, and not even physically.

As regards loadings not small, or almost ultimate loadings resulting in expansion of the crack, it is then evident that the linearized formulation of the problem near the edge is actually also unacceptable in substance, and that as the external loadings increase far in advance of the bond ruptures at the atomic level, the macroscopic properties of the material at the sharply curved edge of the boundary of the solid are not described by an elasticity theory with small strains and Hooke's law.(*)

In some questions (such as crack generation, the theoretical determination of energy in fracture, etc.) which were not examined in the papers being criticized, a study of the detailed phenomena at the locations of stress concentration and at the edge of cracks is important and needed. To do this it is necessary to develop appropriate theories taking account of the nonlinear and inelastic properties of the materials. There are examples of such theories in [8, 14, 15]. Therefore we are in favor of studying details when needed, but only in a wellfounded manner.

It has been remarked above that conservation of the linearized formulation of the problem for a correct modelling of the details at the edge of a crack is generally unacceptable.

*) A Hooke boundary of the elastic domain can be introduced at the edge of a crack, and surface forces on this boundary can be considered as "cohesive forces", but in the general case it is impossible to consider this boundary as a continuation of the discontinuity in a rectilinear crack [8, 9] and to apply the linearized formulation of the problem II to analyze the phenomena at the edge of the crack. Likewise, it is impossible to do this even for small loadings to study the stress concentration when the elastic model is applicable.

However, in some particular cases (the model of Leonov [7] and Panasiuk [21]), such modelling is still possible when the domain of inapplicability of elasticity theory is a negligibly thin plastic layer of length d located on the continuation of the crack. But in this case, when d is finite, the Irwin theory and its criteria are unacceptable. Let us recall that only some other foundation for the Irwin criterion for $d/l \approx 0$ is mentioned in the research being discussed.

If details at the edge of the crack are not taken into account, then for small d we can set $d = 0$ directly and to utilize the Irwin theory; if the details are important for very small d, then it is impossible to recognize the described models with appropriate cohesive forces as physically founded. The foundation of a theory connected with the elimination of mathematical infinities originating because of the approximate nature of the mathematical solutions by using the introduction of appropriate cohesive forces, cannot possibly be considered satisfactory.

Therefore, the jargon of infinite stresses in elasticity theory customarily used, which is connected actually only with the approximate methods of solution and not with the physical modelling, was taken as a physical concept which generated a protesting dissatisfaction among some "physicists".

But, most importantly, all this modelling yields nothing new, generally, in the researches under discussion from the viewpoint of the problems to be posed and solved within the scope of elasticity theory with the simple condition $K \leqslant K_c$. The Barenblatt "theory" can merely be considered as some doubtful interpretation of the correct and clear Irwin condition ($K \leqslant K_c$) established earlier.

The formulation of problems on cracks in brittle and quasi-brittle bodies is kept exactly according to Irwin, and the single change is purely terminological, the introduction of the new term "Barenblatt modulus of cohesion" instead of the initial term introduced by Irwin, the "critical coefficient of stress intensity", for the same quantity.

2°. In connection with the enormous pretentiousness and character of the general style of presentation of the various assertions, let us note the following. There are such sentences in the Raizer paper, which are formulated carefully and accurately as contrasted to previous publications: "Leonov and Panasiuk independently (barely later) proposed a model of a crack taking account of cohesive forces which was along the same lines, but not in so general and definitive a form". From this sentence it can be deduced unintentionally that the modelling of Leonov and Panasiuk is an almost particular case of the Barenblatt modelling. Indeed, Leonov, Panasiuk and Dugdale assume that d is finite, and the cohesive forces are stresses given on the boundaries of the plastic domain, that there is no self-similarity at the edge of the crack and that the quantity d is determined by the body configuration and the external loadings. The Irwin criterion is inapplicable in their formualtion, hence this is some new theory while the Barenblatt modelling adds nothing new to the formulation of the elastic problem as compared with the Irwin theory. The modelling of Leonov, Panasiuk and Dugdale refers substantially to non-brittle bodies, in which the plastic properties play a principal role in the development of fracture.

We read in Raizer's paper: "Irwin himself considered mainly unstable cracks and some of his expressions relating to cracks growing slowly as the loading increases indicate that he did not quite correctly understand this matter."

All this refers to an elementary question on the stability or instability of the expansion of cracks for small changes in the crack length or the external loadings. Irwin explicitly solved the appropriate problems and indicated the effect of a possible acceleration or cessation of the crack expansion process. What else is necessary for comprehension of this question ! (*)

What then occurs? The basic results of the discoverers are branded as unclear and unsatisfactory, they did not comprehend the essence of the question properly, and a "proper good scientific theory" is proposed, which actually contains no new results. Such methods are characteristic of some compilatory compositions.

Raizer refers to the citation of the Barenblatt researches in foreign and domestic papers; however, these references are not conclusive. Exactly as here, there are many authors abroad, who are either uncomprehending, or not investigating the essence of the matter, or simply trusting. Examples of unfounded references and undeserved praise are known to all in our and international life. It is difficult to control this phenomenon, and simply impossible in many cases. Needless to say, there are papers criticizing the Barenblatt "theory" abroad. Besides in the Soviet papers, a true critique is contained also in the foreign publications of Broberg [18], Cribb and Tomkins (19], Paris and Sih [20]. Raizer does not mention this.

3°. A strange tendency for a physicist to consider energy methods as inferior is clearly manifested in the Raizer's paper. He writes: "In substance, a theory based on the energy approach contains some internal inconsistency" (!?). "A different "force approach" (?) is more perfect and internally consistent". "The force approach permits elimination of the unreal peculiarities of the energy approach." "No additional conditions of energy character need be introduced in such an approach, just as the surface energy concept need not be introduced specially which is foreign to the theories of elasticity."

But from the physical viewpoint the surface energy is connected with the very essence of this phenomenon. Crack tests for glass are used again and again to determine the density of the surface energy [21].

At the same time Raizer contradicting himself, devotes a great deal of attention to correct energy considerations. However, the most important consideration in fracture mechanics is that the physical energy equation must certainly be supplementary to the elasticity theory equations in order to establish a connection between the "critical coefficient of stress intensity" K_c and the fracture energy γ. Note that this connection figures in all the "force approaches", including the author's, which are developed.

The energy approach permits a clarification of the general properties of fracture phenomena in various bodies when the interaction mechanisms in the neighborhoods and on the area of the fracture are different, such as brittle, quasi-brittle fracture, etc. [21]. Let us note that, as is known from physics, we can assert that microscopic and macroscopic interactions within "solids" can always be explained and described by utilizing a "force approach".

4°. Finally, as regards the energy relations. Let U denote the elastic volume energy of some part of the body or of the body as a whole, defined by the strain state of the body, and evaluated by integration over the volume under consideration.

First, generally we cannot consider the energy U as the total body energy for static states of the body in physics, and second, increments in the energy U can occur not only

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*) Let us mention that Obreimoff [16] in 1930 in our country, and Gilman [17] abroad in addition to Irwin studied stable cracks in conformity with the energy representations of Griffith.

because of the work of the macroscopic forces.

Within the scope of elasticity theory the energy U can be identified with the total energy. However, we can not consider that the variation δU will always vanish under constant loadings (in particular, when taking account of heat conductivity, in phase transitions, and also in the expansion of discontinuities, etc.).

The correct formula $$(\delta U)_{p=\text{const}} = (1 - \nu^2)\, E^{-1} K^2 \delta S \qquad (2)$$

is presented in the Raizer paper, where K is the stress intensity coefficient corresponding to given external loadings on the edge of a given crack in (1), and δS is the increase in crack area at the considered point of its edge.

This formula is an elasticity theory formula which is always valid. There are published derivations of this formula relying just on elasticity theory equations.

Even Griffith proceeded from the expression for $(\delta U)_{p=\text{const}} \neq 0$ in specific examples! This is precisely the essence of crack theory! Irwin established (2) to calculate the Griffith energy expenditure. Furthermore, Raizer writes equation (23) (p. 343) [1] as some postulate without any derivation (*)

$$(\delta U)_{p=\text{const}} = 0 \qquad (3)$$

In deriving (2) and in utilizing (3) the variations are understood exactly in the same sense.

Formula (3) contradicts formula (2). It is impossible to prove that (3) is also valid together with (2), since (2) is true for $K \neq 0$ and for nowhere does it follow that K should equal zero (although Barenblatt tries to prove condition (3) within the scope of elasticity theory [22]). We can only assume simultaneous compliance with (2) and (3) as some additional condition, and then this implies that $K = 0$ is a mathematical expression for the condition of finiteness of the stress in the linearized approximate solution (**).

We therefore deal with two equivalent assumptions

$$\text{either } (\delta U)_{p=\text{const}} = 0, \qquad \text{or } K = 0 \qquad (4)$$

But is there any proof here? No! There is only the trivial assertion about the equi-

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*) The difference in energy in conditions (2) and (3) is understood to be in displacements corresponding to two actual equilibrium states while in the general case the energy variations in the principle of possible displacements are written in virtual displacements consistent with the constraints. Hence, $\delta U = \delta A \neq 0$ in the principle of possible displacements, where δA is the work of all the external forces on the virtual displacements. This circumstance is not reflected correctly by Raizer, nor in all the works of Barenblatt relative to this question. The energy variations therein are examined on the actual rather than the virtual displacements, hence condition (3) for continuous displacements is not the principle of possible displacements, but is simply a consequence of the uniqueness of the solution of elasticity theory since under constant loadings and in the absence of fracture, the displacements at two equilibrium positions can differ only in the displacements of a rigid body. In this connection, (3) is trivial for $K=0$.

**) The introduction of cohesive forces on the lips of the slit and the condition $K = 0$ are not equivalent. In general $K \neq 0$ in the presence of cohesive forces and only for special appropriate cohesive forces is the equality $K = 0$ valid.

valence of the situation (4) resulting from (2). But is (3) generally valid? Generally this equation is not valid since the formula (2), in which $K \neq 0$ for actually constructed solutions of crack theory, has been proved rigorously correct.

Condition (3) is obtained from the formulation of (2) only for $K = 0$, i.e. when the proof is assumed a priori.

Therefore, in principle, it is impossible to prove anything here, and if the matter reduces to assumptions, the most explicit assumption is the one (not the physical, but the mathematical one associated with the method of approximate solution) about the finiteness of the stresses in the linearized solution, which can be assured only by utilizing artificially introduced appropriate "cohesive forces" applied to the lips of the slit, whose properties are specified by the approximateness of the method of solution.

Hence, let the reader himself asses the Barenblatt declaration ([22], p. 322](*) : "Thus, the condition of the finiteness of the stresses at the end of the crack and the smooth joining of the opposite sides of the crack at its ends was obtained from the fundamental principle of statics – the principle of virtual displacements." Other authors, Sneddon [23], for example, have made analogous statements.

The essence of the principal assertions in the paper of Raizer has been examined above. An explanation analogous to that given in Sect. 4° with all these and other details has already been given in [2 – 5]. Nevertheless, Raizer took it upon himself to make the rhetorical remark: "Precisely this point of the theory, the question of the stationarity of the elastic potential, for some reason has not been comprehended..."

In this connection, the possibility has not been excluded that the sense of the absolutely elementary and simple explanation given above may also turn out to be incomprehensible to those not wishing to comprehend the actual essence of the matter. The critical remarks of a number of authors, published earlier, have not been given proper attention: the Raizer paper under consideration is an illustration of this.

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