Solid Earth Discuss.,
https://doi.org/10.5194/se-2019-59-RC2, 2019

Solid Earth
Discussions

Interactive comment on "What happens to Fracture Energy in Brittle Fracture? Revisiting the Griffith Assumption" *by* Timothy R. H. Davies et al.

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Received and published: 24 June 2019

General Comments

This article focuses on fracture surfaces created in Pyrex glass cylinders subject to confined (and unconfined) compression. Both new results and previous results from Kolzenberg (2013) are used in the analysis. Assuming these Pyrex cylinders have material properties as described in the literature (surface potential energy) then the authors conclude the amount of surface area created in the experiments exceeds the amount that would predicted from early material failure theory. The article sets up the problem, describes the experiments and interprets the results. Some broad statements are made suggesting Griffith theory (1921) may be invalid, inferred from these exper-

imental results. To aid such statements I suggest the authors outline the theory with equations clearly in the introduction chapter to show it is understood and provide an explanation later in the manuscript some reasons as to why this long-standing assumption has previously prevailed for so long.

Major revision is suggested, the results are interesting but clarification of some assumptions and additional details on the setup must be provided in certain parts of the manuscript to validate the interpretation.

Specific Comments

- The abstract attempts to describe the paper but is more leading than clearly describing the setup and results that test the quoted long-standing assumptions concisely. It also conclusively suggests that most of the energy is radiated from the tip as elastic wave energy but no data in the article is provided to validate this statement, this is only postulated later in the discussion section. I suggest the abstract is rewritten such that both the experiments/results are summarised, not just the interpretation of the results. The experimental procedure employed in this work is suitable, well documented (both setup and results) and clearly thought out. A discussion on the limitations and potential error introduced while calculating the fracture surface calculations is well documented and appropriate. Some assumptions/questions:

- In chapter Machine strain energy and specimen strain energy I am unsure to whether there is enough detail on the mechanical system. In chapter 9.2.1 Pollard and Fletcher, Fundamentals of structural geology (2005) machines can be categorised as a 'stiff or 'soft' testing machines, relative to the sample. This definition includes the energy stored in machines frame and does not only concentrate on the material properties of parts of the machine in contact with the sample and as such, is an important detail. Some evidence that the post failure behaviour of this setup is stable is desired. Without such discussion how can readers be certain about the energies in the system that are discussed?

- How confident are the authors on stating a single value for the empirically derived free surface energy criteria of Pyrex described in Wiederhorn (1969)/Lange (1971). For example, Table 2 of Wiederhorn gives a standard deviation of $\sim \pm$ 5% for borosilicate glass fracture surface energy and Lange also gives a $\pm$ 5% scatter. Some remarks on potential variations in the literatures empirical value of this properly with some estimated standard deviation or maximal/minimal values would be more suitable and figure 8 should be updated to show this. Following on from this, is there evidence the glasses used in the experiment have the same values as those in the literature? Note the detailed annealing setup to create identical thermal history of the glass slides used in Wiederhorn (1969) is different. It would help to add some discussion of how well the material properties of the glasses used match/deviate from those detailed the literature.

- It is of interest to discuss if the findings of this study only work for solids in compression. If not, discuss why previously estimated surface energies of previous studies such as Wiederhorn (1969) and Lange (1971) match so well?

- Chapter 10.7 & 10.8: Jaeger, J.C., Cook, N.G. and Zimmerman, R., 2009. Fundamentals of rock mechanics. John Wiley & Sons. Here it is stated:

"Irwin (1958) extended Griffith's concept by pointing out that in many materials, as a crack grows, energy must also be expended to create a damaged zone of irreversible, plastic deformation ahead of the crack tip. In rock, this zone may consist of crushed grains, microcracking, etc."

Do the authors believe there is a way to differentiate such micro cracking in their particle diameter distributions (Fig 3) and would this account for the additional fracture surface they see? It must also be noted that the experiments of Wiederhorn and Lange calculate the fracture surface energy based on the propagation length of a pre-existing crack at the scale of the sample macroscale sample (75 by 25mm/ 300 by 150 mm). No focus in these studies is given to estimating micro cracking (if there was any) close to the crack plane/tip, as such are these empirical values valid for the scales used in

this study?

- Two of the references (Hungr/Livne) in chapter 2 page 3 line 17/18 seem irrelevant to the statement in hand (free surface energy is treated as an energy sink):

Reference 1 (Miller): This is fine, the article details that free surface energy is an energy sink, tensional numerical experiments.

Reference 2 (Chester): This is fine, estimation of free surface energy in fault gouge.

Reference 3 (Hungr): No reference to free surface energy or the energy sink in this publication, it deals with. Frictional experiments.

Reference 4 (Grady): This is fine, the article states "Fracture in brittle solids is weakly dissipative in the sense of converting available energy into surface energy or plastic work. Thus, failure through one or several through-going cracks is far from adequate for absorbing the initial stored elastic strain energy. Consequently, during failure, fracture on successively finer length scales proceeds through a cascade of crack branching until a length scale adequate to the dissipation of the initial elastic strain energy is achieved."

Reference 5 (Livne): I am not sure this this is appropriate for the point being made, firstly they are not using free surface energy in the publication, instead this focusses on testing how well linear elasticity applies and how plasticity (or at least non-linear elastic deformation) at the tip is an energy sink. Correct me if I am wrong but from what I understand basic Griffith failure theory is not directly being used here.

Technical corrections

In introduction page 2 line 9: change 'gravity' to 'gravitational'.

In introduction page 3 line 4 change 'to understanding' to 'to the understanding'.

In Machine strain energy and specimen strain energy line 14 change 'failure of cylinder' to 'failure of the cylinder'.

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