



# Review History for “Frictional Plasticity in a Convex Analytical Setting”

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## Summary

The paper was sent to two Reviewers: Prof. Denis Caillerie, Université Grenoble Alpes (Reviewer 1) and Prof. Francesco Solombrino, University of Naples “Federico II” (Reviewer 2). The two reviewers remained anonymous during the entire revision process. After the reviewing process was completed, both reviewers decided to disclose their identity.

In the first round of review, both reviewers recommended that some (major) revisions be made to the manuscript before it could be published.

After a substantial revision of the paper, Reviewer 2 accepted the revised manuscript without any modifications. Reviewer 1 still required some additional changes, which were made in the second revised version of the manuscript.

At the end of the second round of review, Reviewer 1 required some additional minor changes, which have been addressed in the third revised version of the paper.

At the end of the third round of review, the Editor decided that the last changes made had been satisfactory and accepted the paper without any further modification.

## Review Round 1

### Reviewer 1 (Denis Caillerie)

The paper is about the development of models for frictional plasticity of geomechanics in the framework of the hyperplasticity theory and of the convex analysis. The Author introduces a term in the force potential  $z$  in the form of an indicator function (see Eq. 14). That indicator function has to be understood in the sense of the convex analysis which makes it singular, it is not  $C^1$  continuous, hence the resorting to the convex analysis and to subgradients instead of the usual gradients.

As stressed out in the introduction and the conclusion, the purpose of the Author is “to introduce the terminology of Convex Analysis for the description of frictional materials with a view to using that language for more advanced constitutive modelling”. For that reason the present review concerns mainly the mathematical side of the paper.

Globally, it is the Reviewer’s view that some improvements and corrections should be needed to reach the goal set by the Author.

It is true that in the literature, the elastoplasticity is seldom addressed from the angle of Convex Analysis but the Author does not seem to know the works by J. J. Moreau and coworkers who made important contributions in that field for instance Moreau [1974, 1976] and Suquet [1981].

In the Reviewer' opinion, the bases of Convex Analysis are well introduced in the paper, even though the use of  $f'(x_1)$  to denote a subgradient, whereas it usually denotes a derivative, does not seem so well suited and though a more emphasized angle on the curve  $y = f(x)$  at  $x_1$  in Figure 2 and 3 would be more illustrative.

On the contrary the Reviewer considers that the presentation and the use of the Legendre–Fenchel Transform, which is a key tool in the paper, are rather unclear. The Reviewer does not understand how what is written about the Legendre Transform (page 2, column 2, paragraph 3) may define the function  $Y$ . The definition of the Fenchel Dual (page 7, column 1, paragraph 1) is a little better but in the same sequence of equations  $x$  denotes: 1) the variable of the function  $f$ ; 2) the point at which  $x^*$  is a subgradient of  $f$ ; and, 3) the variable with respect to which the sup of  $\langle x^*, x \rangle - f(x)$  is taken to define  $f^*(x^*)$ . The vagueness in the use of the Fenchel Dual and the notion of conjugate points [Moreau, 1986, p. 48] persists in the appendix 6 (page 9, column 2) and the Reviewer does not understand the reason of the first equation (completed with the missing part). Indeed, it comes down to look for

$$\sup_{\dot{\alpha}_p, \dot{\alpha}_q} (\chi_p \dot{\alpha}_p + \chi_q \dot{\alpha}_q - z(\dot{\alpha}_p, \dot{\alpha}_q))$$

with  $(\chi_p, \chi_q)$  depending on  $(\dot{\alpha}_p, \dot{\alpha}_q)$  which a priori is different from

$$\sup_{\dot{\alpha}_p, \dot{\alpha}_q} (\chi_p \dot{\alpha}_p + \chi_q \dot{\alpha}_q - z(\dot{\alpha}_p, \dot{\alpha}_q))$$

for  $(\chi_p, \chi_q)$  independent of  $(\dot{\alpha}_p, \dot{\alpha}_q)$ . Moreover the looked for result can be obtained using the notion of conjugated points that reads

$$\forall (\chi_p, \chi_q) \in \partial z(\dot{\alpha}_p, \dot{\alpha}_q), w(\chi_p, \chi_q) = \chi_p \dot{\alpha}_p + \chi_q \dot{\alpha}_q - z(\dot{\alpha}_p, \dot{\alpha}_q)$$

That relation makes sense only for  $\dot{\alpha}_p + N|\dot{\alpha}_q| \leq 0$  since for  $0 < \dot{\alpha}_p + N|\dot{\alpha}_q|$ ,  $\partial z(\dot{\alpha}_p, \dot{\alpha}_q) = \emptyset$  and in that case it yields  $\forall (\chi_p, \chi_q) \in \partial z(\dot{\alpha}_p, \dot{\alpha}_q), w(\chi_p, \chi_q) = 0$ . That question of Legendre–Fenchel transform should be made clearer and in the Reviewer's opinion, the main points are: 1) a clear definition of the Fenchel dual; 2) the fact that the dual  $f^{**}$  of the dual  $f^*$  of  $f$  is  $f$ , the notion of conjugate points [Moreau, 1986, p. 48]; and, 3) the theorem that tells that the three propositions:  $x^* \in \partial f(x)$ ,  $x \in \partial f^*(x^*)$  and  $f(x) + f^*(x^*) = \langle x^*, x \rangle$  ( $x^*$  and  $x$  are conjugate) are equivalent, see Moreau [Moreau, 1986, p. 60] and Rockafellar [Rockafellar, 1970, p. 218]; that theorem is important to assert that  $\dot{\alpha} \in \partial_\chi w$ .

There are other points that are worth being clarified or put right.

- The equations (15)b and (15)c seem inconsistent since (15)b entails that  $\Lambda = 0$  for  $x = 0$  (it is the same in equations (18)). A compact and unambiguous formulation of those conditions is  $0 \leq \Lambda, x \leq 0, \Lambda x = 0$ .
- Even if it is written, the reason why the  $\Lambda$ 's in the equations (16) and (17) are the same is not obvious, the use of partial subgradients, that suggests that the  $\Lambda$ 's may be different, is misleading. On that subject the Reviewer wonders whether the notion of partial subgradient presents any interest, at least in the present case. A direct determination of the subgradient of the function  $(\dot{\alpha}_p, \dot{\alpha}_q) \mapsto h_1(\dot{\alpha}_p, \dot{\alpha}_q) = \mathcal{I}_{[-\infty, 0]}(\dot{\alpha}_p + N|\dot{\alpha}_q|)$  could be preferable. As  $\partial h_1(\dot{\alpha}_p, \dot{\alpha}_q)$  coincides with the normal cone at  $(\dot{\alpha}_p, \dot{\alpha}_q)$  of the shaded region in the figure 6.a, the determination can easily be done graphically, that might be quite illustrative even though it is not mathematically rigorous. It yields

$$\partial h_1(\dot{\alpha}_p, \dot{\alpha}_q) = \{(\Lambda, N\Lambda\Sigma_1), \text{ with } \Sigma_1 \in S(\dot{\alpha}_q) \text{ and } 0 \leq \Lambda, x = \dot{\alpha}_p + N|\dot{\alpha}_q| \leq 0, \Lambda x = 0\}$$

- It is true that the subdifferential  $\partial(h_1 + h_2)$  of the sum of two functions  $h_1$  and  $h_2$  is the sum  $\partial h_1 + \partial h_2$  of the two subdifferentials, see Rockafellar [Rockafellar, 1970, p. 224] or Moreau [Moreau, 1986, p. 62], but the expression of the 2nd member of the Eq. 17 does not seem correct, at least it is unusual, ambiguous and consequently misleading. It can be seen that the subdifferential  $\partial h_2(\dot{\alpha}_p, \dot{\alpha}_q)$  of the function  $(\dot{\alpha}_p, \dot{\alpha}_q) \mapsto h_2(\dot{\alpha}_p, \dot{\alpha}_q) = M\sigma_p|\dot{\alpha}_q|$  reads  $\partial h_2(\dot{\alpha}_p, \dot{\alpha}_q) = \{(0, M\sigma_p\Sigma_2), \Sigma_2 \in S(\dot{\alpha}_q)\}$ . Consequently,  $(\chi_p, \chi_q)$  reads  $(\chi_p, \chi_q) = (\Lambda, N\Lambda\Sigma_1 + M\sigma_p\Sigma_2)$  with  $\Sigma_1, \Sigma_2 \in S(\dot{\alpha}_q)$  and  $0 \leq \Lambda, x = \dot{\alpha}_p + N|\dot{\alpha}_q| \leq 0, \Lambda x = 0$ . However, here is no reason why  $\Sigma_1$  should be equal to  $\Sigma_2$ , unless  $S(\dot{\alpha}_q)$  is reduced to one element. Consequently the factorization of Eq. (19) does not hold true for  $\dot{\alpha}_q = 0$  indeed in that case  $S(0) = [-1, 1]$ ; fortunately that does not change the inequality (20). What is less fortunate is that the inequality (20) alone does not entail the condition  $0 \leq \chi_p$ . Consequently, the figure 6.b and the equation (23) are wrong, as well as probably the equations (24) and (25). That is confirmed by determining directly  $w(\chi_p, \chi_q)$ . Indeed it can be easily seen that

$$w(\chi_p, \chi_q) = \sup_{\dot{\alpha}_p + N|\dot{\alpha}_q| \leq 0} (\chi_p \dot{\alpha}_p + \chi_q \dot{\alpha}_q - M\sigma_p|\dot{\alpha}_q|)$$

and that, for  $\chi_p < 0$ ,  $w(\chi_p, \chi_q)$  is equal to  $+\infty$ . Indeed, as for any given  $\dot{\alpha}_q$ ,  $\dot{\alpha}_p$  can go to  $-\infty$  and  $\chi_p \dot{\alpha}_p + \chi_q \dot{\alpha}_p - M\sigma_p |\dot{\alpha}_q|$  can be as great as wanted.

- Is figure 4 useful? It seems to the Reviewer that the equation (21) directly follows from the equation (20) with  $\chi_p = \sigma_p$ .
- The Reviewer does not understand the reasoning, even straightened, of the appendix. Is it so obvious that  $w$  should be an indicator function and that the domain of  $w$  should be exactly the set  $\{(\chi_p, \chi_q) \mid 0 \leq \chi_p, |\chi_q| - N\chi_p - M\sigma_p \leq 0\}$ ?

Assuming that the expression of  $w$  in the equation (28) has been corrected, could the Author explain how the model defined completely (according to the Author) by the equations (27) and (28) works and what is its usual formulation? By usual formulation, the Reviewer means the formulation that is considered by most of the authors and that is the starting point of numerical implementations. It is the Reviewer's view that that clarification could justify the use of the intermediate variables  $\chi_p$  and  $\chi_q$  otherwise rather puzzling. In particular could the Author detail the reasoning leading from the equations (27) and (28) to the fact that  $0 \leq \chi_p$  and  $|\chi_q| - N\chi_p - M\sigma_p \leq 0$ ? In the paper this point is presented as obvious but other authors, Suquet [Suquet, 1981, Eq. (5), p. 7] for instance, do not seem to take it for granted.

The Reviewer shares the Author's view about the contribution of mathematics to engineering problems and he considers that, once the above mentioned questions and shortcomings fixed, the paper would indeed be part of this contribution and might be published in Open Geomechanics. However, the Reviewer doubts that the satisfaction with the knowledge that non associated elastoplastic models satisfy the thermodynamics principles would be strong enough to induce the researchers in Geomechanics to use the Convex Analysis formalism and tools, it would need something more, for instance some mathematical proof of existence or the solving of some singular problems.

Typos:

- page 3, column 1, line 7 before the end of the column, the “d” of “ the dissipation d for the special ...” should be in italic.
- page 4, column 1, line 6 and 7, the expression  $f(x_1)$  of line 6 should be  $f'(x_1)$  as well as the first term of the equation  $f(x_1)(x_2 - x_1) \leq f(x_2) - f(x_1)$ .
- page 5, column 1, line 4 after equation (9). “As  $q$  is in fact  $C_1$  continuous...” should be “As  $g$  is in fact  $C_1$  continuous ...”. By the way, is not  $C^1$  the usual notation?
- page 5, column 2, equation (16), the “ | ” right after  $\dot{\alpha}_p$  is superfluous.
- page 9; column 2, the end of the first equation is missing.

## Reviewer 2 (Francesco Solombrino)

The paper introduces a clean mathematical formulation for a simple model of frictional plasticity by making use of tools from Convex Analysis. The model couples hyperelasticity with a maximal dissipation principle for plastic strains. Since the force potential to be considered combines a frictional dissipation term and a dilation constraint, resulting in a nonsmooth potential, evolution must be formulated in terms of differential inclusions involving possibly multi-valued subdifferentials. A reformulation by Fenchel duality allows the author, additionally, to exchange the role of the variables and write evolutionary inclusions for the rates of the plastic strains, which have the (generalized) stresses as independent variables.

The paper is well-written and the effort of the author for a sound mathematical formulation is in my opinion justified. I therefore think that the paper is suitable for publication. I list below some minor points for the author's consideration.

- Two stress- and plastic strain variables are considered in the model. Their meaning should be accordingly discussed.
- After introducing the force potential (14), an interpretation of the frictional dissipation term is provided. Instead, no heuristic meaning is provided for the dilation constraint. It could be good to compare the equation with the phenomena it accounts, and to conveniently highlight the role of the parameter  $N$ .
- When discussing the differences with the Cam-Clay model, explicitly recalling its equations would allow the reader for a visual comparison with (24) and (25) and for a better understanding of the advantages of the proposed model.

- the terms “angle of friction” and “angle of dilation” are used at page 7. However, it is not made clear before in the paper how they do relate to the equations and the parameters  $M$  and  $N$ . Again, a short discussion seems appropriate.

## Review Round 1: Author Response

### Reply to Reviewer 1

Reviewer’s comments are typed in gray, Author’s replies in red.

It is true that in the literature, the elastoplasticity is seldom addressed from the angle of Convex Analysis but the Author does not seem to know the works by J. J. Moreau and coworkers who made important contributions in that field for instance Moreau [1974, 1976] and Suquet [1981].

**Additional references used.**

In the Reviewer’ opinion, the bases of Convex Analysis are well introduced in the paper, even though the use of  $f'(x_1)$  to denote a subgradient, whereas it usually denotes a derivative does not seem so well suited...

**Changed to more conventional  $x^*$ .**

... and though a more emphasized angle on the curve  $y = f(x)$  at  $x_1$  in Figure 2 and 3 would be more illustrative.

**Figures changed.**

On the contrary the Reviewer considers that the presentation and the use of the Legendre–Fenchel Transform, which is a key tool in the paper, are rather unclear. The Reviewer does not understand how what is written about the Legendre Transform (page 2, column 2, paragraph 3) may define the function  $Y$ .

**In the modified paper I show both the original text and a new description which I hope satisfies the reviewer.**

The definition of the Fenchel Dual (page 7, column 1, paragraph 1) is a little better but in the same sequence of equations  $x$  denotes: 1) the variable of the function  $f$ ; 2) the point at which  $x^*$  is a subgradient of  $f$ ; and, 3) the variable with respect to which the sup of  $\langle x^*, x \rangle - f(x)$  is taken to define  $f^*(x^*)$ .

**New text proposed here as well.**

The vagueness in the use of the Fenchel Dual and the notion of conjugate points [Moreau, 1986, p. 48] persists in the appendix 6 (page 9, column 2) and the Reviewer does understand the reason of the first equation (completed with the missing part). Indeed, it comes down to look for

$$\sup_{\dot{\alpha}_p, \dot{\alpha}_q} (\chi_p \dot{\alpha}_p + \chi_q \dot{\alpha}_q - z(\dot{\alpha}_p, \dot{\alpha}_q))$$

with  $(\chi_p, \chi_q)$  depending on  $(\dot{\alpha}_p, \dot{\alpha}_q)$  which a priori is different from

$$\sup_{\dot{\alpha}_p, \dot{\alpha}_q} (\chi_p \dot{\alpha}_p + \chi_q \dot{\alpha}_q - z(\dot{\alpha}_p, \dot{\alpha}_q))$$

for  $(\chi_p, \chi_q)$  independent of  $(\dot{\alpha}_p, \dot{\alpha}_q)$ . Moreover the looked for result can be obtained using the notion of conjugated points that reads

$$\forall (\chi_p, \chi_q) \in \partial z(\dot{\alpha}_p, \dot{\alpha}_q), w(\chi_p, \chi_q) = \chi_p \dot{\alpha}_p + \chi_q \dot{\alpha}_q - z(\dot{\alpha}_p, \dot{\alpha}_q)$$

That relation makes sense only for  $\dot{\alpha}_p + N|\dot{\alpha}_q| \leq 0$  since for  $0 < \dot{\alpha}_p + N|\dot{\alpha}_q|$ ,  $\partial z(\dot{\alpha}_p, \dot{\alpha}_q) = \emptyset$  and in that case it yields  $\forall (\chi_p, \chi_q) \in \partial z(\dot{\alpha}_p, \dot{\alpha}_q), w(\chi_p, \chi_q) = 0$ .

I acknowledge that the logic was flawed, although the final results was correct. The appendix has been completely rewritten.

There are other points that are worth being clarified or put right.

- The equations (15)b and (15)c seem inconsistent since (15)b entails that  $\Lambda = 0$  for  $x = 0$  (it is the same in equations (18)). A compact and unambiguous formulation of those conditions is  $0 \leq \Lambda, x \leq 0, \Lambda x = 0$ .

Agreed. I have adopted that form.

- Even if it is written, the reason why the  $\Lambda$ 's in the equations (16) and (17) are the same is not obvious, the use of partial subgradients, that suggests that the  $\Lambda$ 's may be different, is misleading. On that subject the Reviewer wonders whether the notion of partial subgradient presents any interest, at least in the present case. A direct determination of the subgradient of the function  $(\dot{\alpha}_p, \dot{\alpha}_q) \mapsto h_1(\dot{\alpha}_p, \dot{\alpha}_q) = \mathcal{S}_{[-\infty, 0]}(\dot{\alpha}_p + N|\dot{\alpha}_q|)$  could be preferable. As  $\partial h_1(\dot{\alpha}_p, \dot{\alpha}_q)$  coincides with the normal cone at  $(\dot{\alpha}_p, \dot{\alpha}_q)$  of the shaded region in the figure 6.a, the determination can easily be done graphically, that might be quite illustrative even though it is not mathematically rigorous. It yields

$$\partial h_1(\dot{\alpha}_p, \dot{\alpha}_q) = \{(\Lambda, N\Lambda\Sigma_1), \text{ with } \Sigma_1 \in S(\dot{\alpha}_q) \text{ and } 0 \leq \Lambda, x = \dot{\alpha}_p + N|\dot{\alpha}_q| \leq 0, \Lambda x = 0\}$$

- It is true that the subdifferential  $\partial(h_1 + h_2)$  of the sum of two functions  $h_1$  and  $h_2$  is the sum  $\partial h_1 + \partial h_2$  of the two subdifferentials, see Rockafellar [Rockafellar, 1970, p. 224] or Moreau [Moreau, 1986, p. 62], but the expression of the 2nd member of the Eq. 17 does not seem correct, at least it is unusual, ambiguous and consequently misleading. It can be seen that the subdifferential  $\partial h_2(\dot{\alpha}_p, \dot{\alpha}_q)$  of the function  $(\dot{\alpha}_p, \dot{\alpha}_q) \mapsto h_2(\dot{\alpha}_p, \dot{\alpha}_q) = M\sigma_p|\dot{\alpha}_q|$  reads  $\partial h_2(\dot{\alpha}_p, \dot{\alpha}_q) = \{(0, M\sigma_p\Sigma_2), \Sigma_2 \in S(\dot{\alpha}_q)\}$ . Consequently,  $(\chi_p, \chi_q)$  reads  $(\chi_p, \chi_q) = (\Lambda, N\Lambda\Sigma_1 + M\sigma_p\Sigma_2)$  with  $\Sigma_1, \Sigma_2 \in S(\dot{\alpha}_q)$  and  $0 \leq \Lambda, x = \dot{\alpha}_p + N|\dot{\alpha}_q| \leq 0, \Lambda x = 0$ . However, here is no reason why  $\Sigma_1$  should be equal to  $\Sigma_2$ , unless  $S(\dot{\alpha}_q)$  is reduced to one element. Consequently the factorization of Eq. (19) does not hold true for  $\dot{\alpha}_q = 0$  indeed in that case  $S(0) = [-1, 1]$ ; fortunately that does not change the inequality (20).

I agree with these observations and have changed the text accordingly.

- Is figure 4 useful? It seems to the Reviewer that the equation (21) directly follows from the equation (20) with  $\chi_p = \sigma_p$ .

Figure deleted as it was not very clear.

- The Reviewer does not understand the reasoning, even straightened, of the appendix. Is it so obvious that  $w$  should be an indicator function and that the domain of  $w$  should be exactly the set  $\{(\chi_p, \chi_q) \mid 0 \leq \chi_p, |\chi_q| - N\chi_p - M\sigma_p \leq 0\}$ ?

See above. The appendix has been completely rewritten.

Assuming that the expression of  $w$  in the equation (28) has been corrected, could the Author explain how the model defined completely (according to the Author) by the equations (27) and (28) works and what is its usual formulation? By usual formulation, the Reviewer means the formulation that is considered by most of the authors and that is the starting point of numerical implementations. It is the Reviewer's view that that clarification could justify the use of the intermediate variables  $\chi_p$  and  $\chi_q$  otherwise rather puzzling. In particular could the Author detail the reasoning leading from the equations (27) and (28) to the fact that  $0 \leq \chi_p$  and  $|\chi_q| - N\chi_p - M\sigma_p \leq 0$ ? In the paper this point is presented as obvious but other authors, Suquet [Suquet, 1981, Eq. (5), p. 7] for instance, do not seem to take it for granted.

I have added a paragraph briefly setting out the incremental solution.

The Reviewer shares the Author's view about the contribution of mathematics to engineering problems and he considers that, once the above mentioned questions and shortcomings fixed, the paper would indeed be part of this contribution and might be published in Open Geomechanics. However, the Reviewer doubts that the satisfaction with the knowledge that non associated elastoplastic models satisfy the thermodynamics principles would be strong enough to induce the researchers in Geomechanics to use the Convex Analysis formalism and tools, it would need something more, for instance some mathematical proof of existence or the solving of some singular problems.

I have not attempted to extend the scope of the paper, but agree that this formalism is intended as a starting point for further developments.

Typos:

- page 3, column 1, line 7 before the end of the column, the “d” of “ the dissipation d for the special ...” should be in italic.
- page 4, column 1, line 6 and 7, the expression  $f(x_1)$  of line 6 should be  $f'(x_1)$  as well as the first term of the equation  $f(x_1)(x_2 - x_1) \leq f(x_2) - f(x_1)$ .
- page 5, column 1, line 4 after equation (9). “As  $q$  is in fact  $C_1$  continuous...” should be “As  $g$  is in fact  $C_1$  continuous ...”. By the way, is not  $C^1$  the usual notation?
- page 5, column 2, equation (16), the “|” right after  $\dot{\alpha}_p$  is superfluous.
- page 9; column 2, the end of the first equation is missing.

All of these typos resulted from the conversion of the original manuscript to LaTeX. They have been corrected in the new manuscript.

## Reply to Reviewer 2

Reviewer’s comments are typed in gray, Author’s replies in red.

- Two stress and plastic strain variables are considered in the model. Their meaning should be accordingly discussed.

Brief comments added.

- After introducing the force potential (14), an interpretation of the frictional dissipation term is provided. Instead, no heuristic meaning is provided for the dilation constraint. It could be good to compare the equation with the phenomena it accounts, and to conveniently highlight the role of the parameter  $N$ .

Additional comment made.

- When discussing the differences with the Cam–Clay model, explicitly recalling its equations would allow the reader for a visual comparison with (24) and (25) and for a better understanding of the advantages of the proposed model.

Done.

- the terms “angle of friction” and “angle of dilation” are used at page 7. However, it is not made clear before in the paper how they do relate to the equations and the parameters  $M$  and  $N$ . Again, a short discussion seems appropriate.

$M$  and  $N$  are measures of friction and dilation, but I do not give conversions to “angles” as there are so many alternative definitions available that I think it could be confusing.

## Review Round 2

### Reviewer 1 (Denis Caillerie)

In the Reviewer’s opinion, the changes made by the Author are going to ease the comprehension of the paper and to make it more accessible to readers not too much used to mathematics tools. However there are still some points to be settled.

Formally the equation (16) is not clear. The second line seems to denote the value of  $\partial_z(\dot{\alpha}_p, \dot{\alpha}_q)$  but it involves products of sets that do not really make sense. In the same line of thought, the sign “=” of the expression  $A\Sigma_1 + B\Sigma_2 = (A+B)S(\dot{\alpha}_q)$  should be a “ $\in$ ” since  $\Sigma_1$  and  $\Sigma_2$  are real numbers and  $S(\dot{\alpha}_q)$  denotes a set. The same remark applies to the equation (18) and to other equations. Those remarks may seem to be trifles but, as the use of sets cannot be avoided in convex analysis, it’s the reviewer’s opinion that those mathematical notions should be dealt with in the rightest possible way. Without making of it a condition of postponement, it would be nice to tie up that loose point, that would make the reading of the paper easier.

A more important point is the apparently persistent disagreement between the Author and the reviewer about the expression (22) of the Fenchel dual function  $w$  of function  $z$ . Indeed, as already pointed out in the first review, the inequality  $|\chi_q| - N\chi_p - M\sigma_p \leq 0$  does not imply  $0 \leq \chi_p$  and it may be easily proved, as the Author does it in the Appendix, that  $w(\chi_p, \chi_q) = +\infty$  for  $\chi_p < 0$ . Therefore, it the Reviewer is right,  $w$  is not the indicator function of the set  $\{(\chi_p, \chi_q) \mid |\chi_q| - N\chi_p - M\sigma_p \leq 0\}$  but of the set  $\{(\chi_p, \chi_q) \mid 0 \leq \chi_p \text{ and } |\chi_q| - N\chi_p - M\sigma_p \leq 0\}$ , consequently the equation (22) is wrong as is the graphic of the figure 5b. Would the Author agree with the Reviewer, the point should be corrected before the paper could be published, otherwise a third reviewer could be appealed to give an opinion.

## Reviewer 2 (Francesco Solombrino)

In the revised version of the paper, the Author has addressed satisfactorily all the remarks made in my review. Therefore, it is my opinion that the paper can now be published without any further changes.

## Review Round 2: Author Response

### Reply to Reviewer 1

I wish to thank the patient reviewer for his review.

I have taken a careful look at the two important paragraphs provided by the reviewer, and made some adjustments (highlighted in pink in the attached version) at the relevant points:

1. Equations (16) to (18). I think the problem arises because of some residual ambiguity about whether  $S(x)$  is a set or a real number. I have now made it clearer that  $S(x)$  is a real number. I think the adjusted text is now clearer and the equations are correct. The reviewer felt that equation (16) involved a mathematically incorrect multiplication of sets, but now that it is clear that  $S(x)$  is a real number I think that it is mathematically as now written.
2. I take the reviewer’s point on equation (22) and the appendix, and have made adjustments to that and the figure. I was implicitly assuming  $\chi_p = \sigma_p$ , but have now made the text clearer. I hope this satisfies the reviewer.

I do hope the paper is now acceptable. This has been an interesting and instructive exercise!

## Review Round 3

### Reviewer 1 (Denis Caillerie)

The Reviewer does not think that the definition of  $S(x)$  as a real number instead of as a set makes the formulation of the set  $\partial_{\dot{\alpha}_q} z$  in the equation (16) less ambiguous. Why not simply write:

$$(\chi_p, \chi_q) \in \partial_{\dot{\alpha}_q} z = \{(\Lambda, N\Lambda\Sigma_1 + M\sigma_p\Sigma_2) \mid \Lambda \in \mathcal{N}_{]-\infty, 0]}(\dot{\alpha}_p + N|\dot{\alpha}_q|), \Sigma_1, \Sigma_2 \in S(\dot{\alpha}_q)\}$$

where  $S(x)$  is the set defined in the two previous versions of the paper.

The Reviewer has noticed that the Author agrees with the fact that the function  $w(\chi_p, \chi_q)$  (equations (21) and (22)) is equal to  $+\infty$  for  $\chi_p < 0$ . But he added the remark: “Noting, however, that  $\chi_p = \sigma_p$  it follows that any values of the variables that

satisfy  $|\chi_q| - N\chi_p - \sigma_p \leq 0$  also satisfies  $0 \leq \chi_p$ ” seems inadequate to the Reviewer. It gives the impression that  $\chi_p$  could be replaced by  $\sigma_p$  but that is wrong, otherwise why should not  $\chi_p$  be replaced by  $\sigma_p$  from the beginning? The answer is probably that that would make the developments of this work impossible and the reason is certainly deeper than “for formal mathematical purposes  $\sigma$ ,  $\chi$  and  $\bar{\chi}$  need to be treated separately” (page 8). That stresses out the ambiguous status of  $\sigma_p$  in the work, is it a variable? is it a parameter? Anyway, the reviewing process is now too well under way to discuss that point further.

By the way, the Reviewer does not understand the mathematical meaning of the intersection of two *functions*, namely the expression  $\mathcal{S}_{]-\infty,0]}(|\chi_q| - N\chi_p - \sigma_p) \cap \mathcal{S}_{[0,+\infty[}(\chi_p)$  in the equation (22).

With righter formulations of  $\partial_{\alpha,q} z$  (equation 16)) and of  $w(\chi_p, \chi_q)$  (equation (22)) the paper can be published, possibly without a fourth review.

## Review Round 3: Author Response

### Reply to Reviewer 1

As ever, I have found the comments from the very careful reviewer most helpful, and I have made a few small changes that I think completely take care of the matters he raised, which were perfectly valid points.

I have changed the two equations he suggested, and also addressed the issue in the middle paragraph of his latest review. As a result, I have changed the order of presentation of figures 4 and 5, and the corresponding text – I think it flows much better now.

I have highlighted in the revised version n. 2 the points where I have made changes, but this is mostly swapping over the two sections, and as a result shortening the text. I have corrected the two equations, and have adopted the reviewer’s suggestion to define the signum function as a set.

I have also dealt with a handful of very minor typographical matters.

### Editor decision

At the end of Review Round no. 3, the managing Editor has decided to accept the revised version no. 3 of the manuscript for publication without any further changes.

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Rockafellar, R. T. (1970). *Convex Analysis*. Princeton University Press, NJ.

Suquet, P. (1981). Sur les équations de la plasticité: existence et régularité des solutions. *Journal de Mécanique*, 20.