1 Reply to Referee #2

1.1 Introduction

> **The aim of the paper is to study how policy makers should optimally set interest rates, exchange rates, and taxes on foreign debt.**

The paper aims to show that the 'possible trinity' is quite generally not only possible but optimal, since the CB obtains a lower loss when it implements a policy with three interventions. It is not its objective to determine how the CB **should** set the instruments but to show that, whatever its objectives, most of the time they will be met more closely if it uses all 3 policies.

1.2 Major Comments

> **The way the paper is written is often confusing. The paper is hardly understandable without consulting a previous paper of the same author (Escudé, 2012). The author himself is aware of this and frequently requests the reader to consult the parent paper.**

First, I must give recognition to my referees for the difficult task of evaluating a paper that is a direct outgrowth of another. Although I have tried to make it as self-contained as possible through the use of the two Appendixes, it is possible that some readers may need to also read parts of the parent paper in order to have a more in-depth understanding of the model. This is especially so because practically all DSGE policy models use one policy rule: either a) one that reflects the targeting of the nominal interest rate through a feedback rule that responds to deviations of the inflation rate and possibly also GDP from certain reference values, or b) one where there is an exchange rate policy (say a fixed or pegged exchange rate). With such a simple policy rule frameworks, the models can avoid getting into the "nuts and bolts" of central banking that are key to the functioning of the complex feedback mechanism. For example, what the central bank actually implements in order to influence the interest rate is an organized intervention in the bond market (open market operations). But modeling this can be sidestepped because the simplicity of the policy makes the more complete model (that would include the bonds actually bought and sold) decomposable, so there is no harm in leaving these bonds out of the (core) model. However, if one wants to model an economy where the central bank uses both interest rate and exchange rate policies, a major difficulty is introduced and there is no way of avoiding the need to use a bigger model that includes the assets involved and their interconnections. This is what I have done in the parent paper (and in various other earlier and even bigger models that I decided to pare down for communicability). The present paper complicates things in a new direction, which is the use of a third policy rule that aims at influencing capital flows. If one has not internalized the earlier, in itself complicated model in relation to the usual SOE macro model, reading the present paper may not be easy. So I fully sympathize with the difficulties my referees may have faced. But if the model has complications it is because essential aspects of the reality of the interactions between policy actions and the macroeconomy are themselves complicated. There is no (formally correct) way of simultaneously facing the fact that so many central banks have some form of exchange rate policy (even if it is presumably secondary to their interest rate policy) and avoiding the complications regarding the assets (and liabilities) involved. These assets are basically the domestic currency bonds bought and sold in order to influence the interest rate and the CB international reserves that increase or decrease every time the CB intervenes in the FX market. And these assets are linked through the CB balance sheet. The present model includes all of this and adds a systematic taxing policy on household foreign debt. The combination of these three systematic policies generates dynamics that are by no means simple, but it is the reality that is complex. I built what may seem a complex model because it is the only way (I have found) for modeling this complex everyday and quite universal reality.

> **Major parts of the outline of the model are relegated to the Appendix. This is fine. However, the structure of the model should be explained (at least verbally) in the paper. Moreover, the paper should be streamlined and considerably shortened.**

I find this paragraph somewhat contradictory since it suggests, on the one hand, extending the paper by more exhaustively explaining the structure of the model (much of which is included in the six full pages of Appendix A, in addition to the listing of equations and identities in Appendix B) while, on the other hand, shortening it considerably. One can't have it both ways. Also, I find no indication by the referee of what part of the paper could be eliminated or condensed.

The structure of the model is a rather straightforward extension of standard SOE models, except for what is perhaps the main innovation (present in both this and the parent paper) of including in the model some of the "nuts and bolts" of central banking. This includes the relation between the "policy targets" (interest rate, rate of nominal depreciation) and the actual instruments moved by the CB (CB domestic currency bonds and CB international reserves), and the fact that they are linked through the CB budget constraint and the assumption that quasi-fiscal surpluses (deficits) are transferred to (funded by) the Treasury.

> **The calibration should be explained and discussed much more carefully. From reading this paper, the calibration is neither justified nor explained.**

In order to avoid making the paper exceedingly long, I basically only discussed the novel features of this model in comparison to the parent model in which there is no taxing of capital flows. This is so for the equations and the calibrations. But the parent paper is available "at the touch of the finger" in the same website and on the same website page since the two papers are related. Most of the calibrations are discussed, including some econometric evidence, in the parent paper. Section 3 of the present paper (Calibration of parameters and the non-stochastic steady state) begins with the following paragraph:

> In this section the calibrated parameters that are used in the exercises below are shown and the calibration procedure used is only detailed inasmuch as it differs from that of the parent paper. Since the only expansion in this paper is that there is either a tax or a tax/subsidy scheme related to foreign debt (even in the NSS), the rest of the calibrations are the same as in Escudé (2013), which the interested reader can consult. It is convenient to stress that, although Argentine data has been used for some of the calibrations, the main objective has been to have a calibrated SOE economy similar in many respects to some of those most cited in the literature (e.g., Galí and Monacelli 2005 and De Paoli 2006) but endowed with the innovations that allow for the systematic and simultaneous use of interest and nominal depreciation policy rules.
>
> **For example, I am very surprised to figure out, from inspecting Table 1, that the government spending to private consumption ratio is above unity.**

Although Table 4 succinctly refers to G as "Gov. Expend. to private consumption", section 2.2 (The public sector) of the text (only 2 pages **preceding** Table 4) says

> The Government spends on goods, receives the quasi-fiscal surplus (or finances the quasi-fiscal deficit) of the CB, and collects taxes. It is assumed that fiscal policy consists of an exogenous autoregressive path for real government expenditures as a (gross) fraction (G_t) of private consumption $\tau_M(.)\, p_t^C C_t$, collecting the tax on private capital flows, and collecting whatever lump-sum taxes are needed to balance the budget each period. The Public Sector real flow budget constraint is hence:
>
> $$tax_t = (G_t - 1)\,\tau_M\left(m_t/p_t^C C_t\right) p_t^C C_t - qf_t - tax_t^{DCol}.$$

Hence, clearly the steady state value $G = 1.19$ of Table 4 means that government expenditure is 19% of private consumption.

> **Moreover, the author should convince the reader that the model is a good model by evaluating the model's capacity to fit the data (standard deviations, relative standard deviations, correlations).**

An econometric estimation of the model would require another paper. And besides, reporting on an econometric estimation of the model would certainly not be compatible with the suggestion that "the paper should be streamlined and considerably shortened".

I will answer most of the rest of the main comments as a whole. These include:

> **One of the main weaknesses of the paper is that it lacks a discussion of the distortions in the economy and of the trade-offs the policy maker faces. Since the paper does not provide this, it is impossible to think about what a policy maker should do in this economy, and what he can or can not achieve. What are potential benefits or costs from intervening in the foreign exchange market? What are potential benefits or costs from adopting capital controls? The present paper just demonstrates that welfare is improved if you add an additional instrument to a restricted instrument set that is smaller than the set of policy targets. But this is well-known. Moreover, the draft does not convey much of the economics of the results. The text just writes what the reader can see in the Tables, without much added.**
>
> **Although the model provides a reasonable welfare metric, namely the utility function of households, the author considers an ad-hoc loss function. Besides the lack of microfoundation of the loss function, the author does not even motivate and justify the choice of this specific loss function. In particular, why should policy-makers that aim to stabilize consumer price inflation (which implicitly takes account of exchange rates movements) also aim to stabilize real exchange rate movements explicitly? Why do policy-makers aim to stabilize the output level and not the deviation of the output level from the natural level that would prevail under flexible prices?**

Answering these comments fully would probably require another paper. And part of my answers are already included in my answers to the referees' comments of the parent paper (easily available in the Economic e-Journal website). Let me say succinctly that I chose to be more general as far as policy objectives are concerned than the usual New Keynesian framework that believes that there is much content in building a model in which there is a representative agent (which, in particular, means that there are no conflicting interests related to income distribution) and a "social planner" that diligently tries to maximize this singular agent's welfare. Since I believe that the use of a "representative agent" is just an expedient for obtaining simplicity and that the true world is one in which there are multiple agents with conflicting interests and policymakers that are not necessarily neutral, I do not think that one should take too literally the utility function of the "representative agent" nor the relevance of the neutral "social planner". This may not be very compatible with the mainstream use of the New Keynesian paradigm, which I think is rather Panglossian since it paints a totally unrealistically optimistic view of the world we live in (no conflicts of interests!). Also, I am too well aware that, although the New Keynesian way of modeling nominal rigidity *a la Calvo* is the best we have, it is nevertheless ad hoc. After all, it is simply postulated that only a fraction of price setters can set their optimal prices in any given period. Woodford (2003, chapter 6) is very clear about these issues when he says:

> Deriving a utility-based welfare criterion in this way can not only allow one to justify a general concern with price stability, but can furthermore provide exact answers to the questions raised previously about the precise formulation of the appropriate loss function. These answers depend, of course, upon the assumptions one makes about the structure of the economy; for example, they depend crucially upon the nature of the nominal rigidities that are present. Insofar as the correct structural relations of the present model of the economy remain controversial, the proper welfare criterion to use in evaluating policy remains controversial as well, and my goal here is more to illustrate a method than to reach final conclusions.

Unfortunately, in economics (as in many other realms of knowledge) good illustrations are sometimes converted into dogma. I believe that giving inordinate importance to what one specific model says about the "distortions" generated by nominal rigidities is not a robust procedure. This is even more so when there are serious gaps between the way concepts such as "natural GDP" are often constructed and the theoretical model-specific "natural GDP" that consists in taking the GDP that a parallel and reference model would have: one which only differs from the original in that one of the crucial assumptions that place a drag on the utility of the representative agent is eliminated (the presence of nominal rigidities). If instead of a model with one "representative" agent, we had a model with two, three,...or n different agents, the "benevolent government" or "social planner" fictions would be of little use. I prefer to approach the matter of optimal policies in a way that is more robust to the particular model I use to represent the basic functioning of the macroeconomy. The methodology that is developed would remain valid if there were changes in the model like, for example, introducing some realistic feature like having a majority of the population not owning even a fraction of any firm. In that case, there would be no reasonable way of avoiding the introduction of the decisionmakers' preferences. And it would remain valid if a real theory of nominal rigidities were introduced in the model instead of the ad hoc assumption that a considerable fraction of price-setters cannot set prices for unexplained reasons.

On the other hand, the reality of central bank policies is that never is a single model relied on, that the Board includes members with widely differing views on the way the economy actually functions, that as individuals they have connections to different sectors and even possibly membership to different political parties that have widely different views and aims. So this is another reason for avoiding analyses that are too model specific, at least in a Central Bank.

Policymakers in my models try to reduce the fluctuations of certain endogenous variables around the model's non-stochastic steady state, not the fluctuations of the gaps between those variables and their counterparts in a reference model where there are no nominal rigidities. They try to smoothen the effects of the perturbations generated by the exogenous shocks. They do so by minimizing an admittedly ad hoc loss function that is quite sensible and easy to understand by anyone, one which is a weighted average of the squared deviations of the target variables with respect to their values in the model's non-stochastic steady state. And the paper uses an array of alternative CB preferences where different weights are given to the deviations of inflation (with respect to a target that is also the non-stochastic steady state), of output, and of the real exchange rate, with respect to their non-stochastic steady states. This is more general than the standard procedure of maximizing a second order approximation of the representative agent's utility function, especially when a broad set of possible CB preferences are taken into account. For me this is a virtue, not a defect.

As to it being well-known "that welfare is improved if you add an additional instrument to a restricted instrument set that is smaller than the set of policy targets", it would help if the referee pointed out some other paper or model that uses this to actually construct a macroeconomic model that reflects it. The parent paper is one of the very few (and probably the first) that can actually model simultaneous interest rate and exchange rate policies, and implement them in a numerical DSGE model. The present paper shows that the same basic framework can also reflect the advantages of simultaneous interest rate, exchange rate, and taxes-on-capital-flows policies. If it were merely a matter of knowing that including additional instruments helps in minimizing a loss function, why are there not a plethora of models representing simultaneous interest rate and exchange rate policies, not to mention the three policies of the 'trinity' represented in this paper? The fact that almost all existing models use either an interest rate policy rule or an exchange rate policy rule is a reflection of the difficulties involved in doing so. The present paper shows that it is very often optimal (in the specific sense of "optimal" used here) to use all three policies but that in some circumstances the loss from abstaining from one of them is actually very small. Finally, although I am well aware of the traditional results on the "number of targets vs. number of instruments" question for purely backward-looking models, I know of no generalization for forward-looking models and would appreciate any reference to one.

I think that the only part of the above comments that I have not addressed is the following:

> **In particular, why should policy-makers that aim to stabilize consumer price inflation (which implicitly takes account of exchange rates movements) also aim to stabilize real exchange rate movements explicitly?**

Consumer price inflation has mainly to do with a tax on the holdings of money and the generation of inefficient price dispersion. On the other hand, the rate of nominal depreciation directly affects relative prices and the real exchange rate, which has mostly to do with trade. Both are obviously related through the various equations of the model. If stabilizing consumer price inflation implied dealing optimally with the nominal exchange rate this would be reflected in the coefficients of the optimal simple rules and the losses in the optimal policy under commitment framework. The results of both this and the parent paper show that it is optimal to have both policies in place. There is a long experience with the problems faced by economies where the domestic currency is either grossly overvalued in real terms (witness the melt-down of the Argentine economy in 2001-2002) or grossly undervalued in real terms (generating an unnecessary contraction of the domestic market and unnecessarily high profits for tradable sector firms). I think it is quite reasonable to aim for the stabilization of the RER as well as the stabilization of inflation and output. Of course, extremely (and unrealistically) simple open economy models are unable to represent such multiple targets.

1.3 Minor Comments

> **It seems that GDP in the production function of the export sector is an externality. Is this true? If so, it should be explained and motivated.**

There is no externality here. The inclusion of the GDP in the production function of the export sector simply means that this sector uses some inputs that can be thought of as coming from all the sectors in GDP, as well as inputs that can be thought of as coming from all sectors in gross output Q (which in the model differs from GDP=Y). This is (I think) sufficiently well explained in the paper's Appendix A, from which I quote:

> Firms in the export sector use domestic goods and the composite of goods that defines GDP. I assume that the export good is a single homogenous primary good (a commodity). Firms in this sector sell their output in the international market at the foreign currency price P_t^{*X}. They are price takers in factor and product markets. The price of primary goods in terms of the domestic currency is merely the exogenous international price multiplied by the nominal exchange rate: $S_t P_t^{*X}$. Let the production function employed by firms in the export sector be the following:
>
> $$X_t^* = \left(Q_t^X\right)^{b^A} Y_t^{1-b^A}, \qquad 0 < b^A < 1,$$
>
> where Q_t^X is the amount of domestic goods used as input in the export sector and Y_t is real GDP.

The relation between Q_t^X and Y_t is given by the first order condition for profit maximization in the export sector, yielding the export sector's (factor) demand for domestic goods as a function of GDP (Y_t), the RER (e_t) and the terms of trade (p_t^*):

$$Q_t^X = \left(b^A e_t p_t^*\right)^{\frac{1}{1-b^A}} Y_t.$$

The choice of the parameters of the simple policy rules (see Table 4 and 5) should be explained.

Section 5.1 of the paper (Preliminary illustration of the effects of introducing capital controls through simple policy rules) is merely meant to be an illustration of the dynamics of the model, on the one hand, and of the effects on the dynamics of the main variables of introducing a tax/subsidy scheme related directly to household external debt. Being an illustration I believe there is no necessity to explain why I used the specific coefficients that I used. Nevertheless, let me say that when I constructed the exercise I obviously looked at what I had obtained as optimal coefficients for the two policy regimes used for this exercise (which I call managed exchange rate -MER- and managed exchange rate with capital controls -MER+CC-) and which can be seen in the upper panels of Tables 9 and 9 (cont.) which I reproduce below for the reader's convenience.

Why is the value for the smoothing parameter in the Taylor rule larger than unity?

It turns out to be optimal in some circumstances to have such a "super-inertial" policy. Theoretically, the possibility was already shown and stressed in chapter 2.3 (Inertial Responses to Inflation Variation) of Woodford (2003). But in my case it simply helped to provide a reasonable illustration of the standard-deviation reducing ability of introducing a third simple policy rule by means of IRFs. When I constructed this illustration I looked at the optimal simple policy rule coefficients that are spelled out in Table 9 of the paper (reproduced below). And in most (indeed, in 15 of the 16 policy regime-CB style combinations in which there is a feedback response to inflation) the h_0 coefficient on the Taylor rule turned out to be superinertial. So I began my search for a good example using a h_0 that was greater than one.

Table 9
Optimal Simple Rules ('osr')

OPTIMAL VALUE OF THE PARAMETERS				
	CB style			
	A	B	C	D
	MER+CC			
h_0	1.61	3.50	1.16	25.41
h_1	1.46	-2.82	-0.20	-10.54
h_2	-0.02	-10.37	-0.06	-35.33
h_3	-0.04	0.36	0.03	4.27
k_0	-0.29	-0.19	0.43	-0.11
k_1	-0.20	0.19	0.11	0.01
k_2	-0.14	-1.51	-1.78	-0.81
k_3	-0.16	-0.18	-0.26	-0.99
k_4	-0.001	-0.003	-0.003	-0.006
j_0	-0.03	0.00	0.00	0.01
j_1	-0.21	-0.19	-0.20	-0.43
j_2	0.01	0.07	-0.05	0.29
j_3	-0.03	-0.05	-0.03	0.34
j_4	-0.01	-0.02	-0.02	-0.02
	FER+CC			
h_0	414.13	86.72	211.13	259.87
h_1	415.82	16.39	-77.08	-3.68
h_2	29.53	-128.77	-347.02	-321.35
h_3	121.68	-47.11	90.40	-50.94
j_0	0.07	-0.23	0.38	-0.01
j_1	-19.56	-31.43	-47.77	-33.28
j_2	-4.83	-1.11	-102.73	0.69
j_3	-86.47	-43.75	-56.57	-146.92
j_4	-0.02	-0.01	-0.03	-0.02
	PER+CC			
k_0	0.71	0.48	0.48	21.55
k_1	-0.47	-0.18	-0.17	-77.55
k_2	-0.61	-0.97	-0.99	192.64
k_3	-0.06	-0.12	-0.11	36.95
k_4	0.00	0.00	0.00	46.81
j_0	0.17	-0.03	-0.03	-1.00
j_1	-0.21	-0.21	-0.21	26.46
j_2	0.00	0.50	0.50	146.95
j_3	0.00	0.00	0.01	-63.28
j_4	-0.02	-0.01	-0.01	0.01

Table 9 (cont.)
Optimal Simple Rules ('osr')

OPTIMAL VALUE OF THE PARAMETERS				
	CB style			
	A	B	C	D
	MER			
h_0	1.13	0.88	13.53	5.52
h_1	1.34	0.34	-3.22	-2.63
h_2	0.00	1.14	-7.19	-4.18
h_3	0.00	-0.09	0.88	1.68
k_0	0.16	0.43	0.08	0.08
k_1	-0.07	0.03	1.03	0.32
k_2	-0.17	-3.50	-0.93	-2.31
k_3	-0.11	0.05	-0.22	-1.35
k_4	-0.001	0.000	-0.002	-0.006
	FER			
h_0	760.43	1.69	7611.81	5771.43
h_1	8632.73	-0.36	42873.50	32799.90
h_2	-311.79	-3.28	17829.70	12930.20
h_3	2822.09	-1.03	6780.07	6346.12
	PER			
k_0	-0.58	-0.63	-0.67	-0.63
k_1	-1.51	-2.58	-2.83	-1.95
k_2	-0.15	-3.16	-1.93	-1.64
k_3	-0.32	0.07	0.23	-0.13
k_4	0.06	1.52	0.52	-0.02
	CC			
j_0	6.51	1.48	208.93	1.30
j_1	-9148.44	-713.61	-16688.20	-457.81
j_2	-872.94	-334.66	-70288.90	-219.35
j_3	-1189.48	105.25	-6001.14	25.90
j_4	-0.26	-0.08	-6.34	-0.07

Why does the central bank raise the interest rates in recession?

To me, a coefficient of $h_2 = -0.01$ is (practically) zero, especially when coefficients such as $h_0 = 1.3$ and $h_1 = 2.1$ are also used in the exercise, as can be seen in Table 5 of the paper, which I reproduce below. The reason that I used a negative coefficient is again that I looked at the signs of the optimal coefficients obtained. Table 9 above shows that for all 4 CB preferences (A, B, C, D) under the policy regime of MER+CC (where all three policy rules are used) the sign of h_2 is negative, and that for 3 of the 4 CB styles under a policy regime of MER it is zero or negative. But just to show the obvious fact that there usually are no significant changes in the IRFs when there are small changes in coefficients, I ran the model changing the sign of the coefficient in question (to $h_2 = 0.01$). The graph below shows the result, which corresponds to Figure 4 of the paper (which I also reproduce below). As expected (at least by me), there are no noticeable differences between the two.

Table 5

MER+CC regime with simple policy rules

Simple policy rules							**Results**		**% Ch.**
Coefficient values						**Variable**	**Mean**	**Std. Dev.**	**vs. MER**
h_0	1.3	k_0	-0.2	j_0	0.5	piC	1.015	0.0057	-37.4%
h_1	2.1	k_1	-0.4	j_1	-0.2	Y	1.443	0.0787	+5.2%
h_2	-0.01	k_2	0.1	j_2	0.0	e	0.5951	0.0425	+5.2%
h_3	0.05	k_3	-0.3	j_3	0.0	ii	1.0253	0.0121	-22.9%
		k_4	-0.1	j_4	-0.03	delta	1.015	0.0499	-26.1%
						d	1.2124	0.4603	+42.5%
						gammaD	0.5	0.186	+58.6%
						varphiD	1.0013	0.0095	+58.3%
						taxsubD	0.1	0.1694	+∞%

Figure 4 with $h_2 = 0.01$

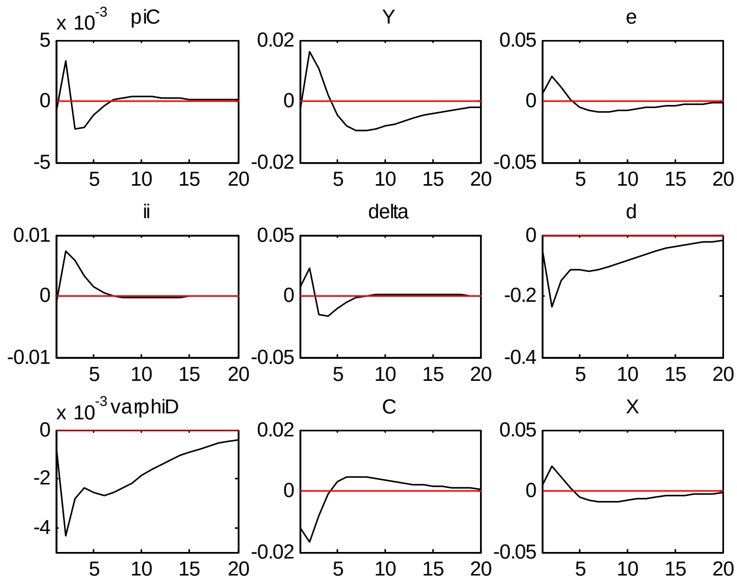
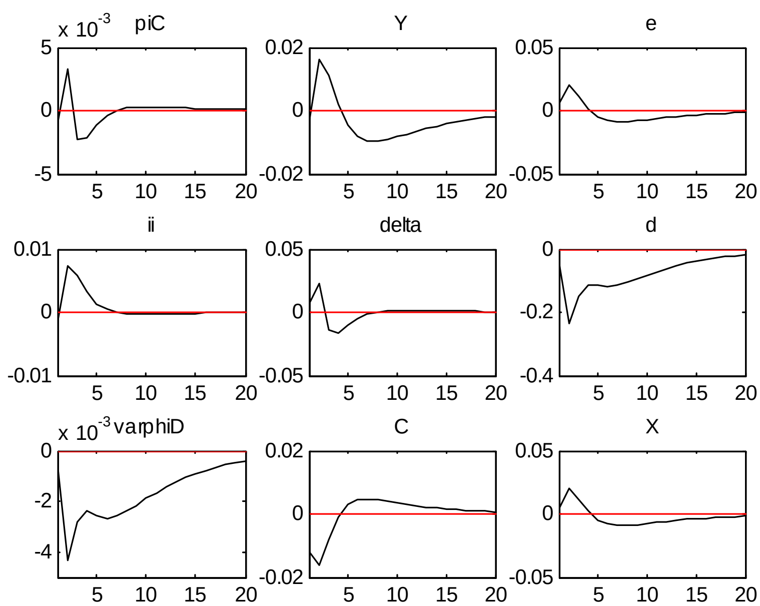


Figure 4

Negative shock to ϕ^*

MER+CC regime



1.4 References

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