



Trois essais en économie appliquée du développement

Paul Dutronc-Postel

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DE L'UNIVERSITÉ PSL

Préparée à l'Ecole Normale Supérieure

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Soutenue par

Paul Dutronc-Postel

Le 12 novembre 2019

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SUPÉRIEURE



PARIS SCIENCES ET
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ÉCOLE D'ÉCONOMIE DE
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Présentée et soutenue publiquement le 12 novembre 2019 par

Paul DUTRONC-POSTEL

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ÉCOLE NORMALE
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PARIS SCIENCES ET
LETTRES



PARIS SCHOOL OF
ECONOMICS



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Résumé de la thèse

Cette thèse rassemble trois projets de recherches indépendants en économie appliquée. Tous ont en commun d'étudier, sur la base de données empiriques, le comportement d'offre de travail ou d'effort d'agents dans un cadre institutionnel donné.

Le premier chapitre, co-écrit avec Maiting Zhuang, étudie le comportement des secrétaires préfectoraux du Parti Communiste chinois entre 1996 et 2014, et la performance économique de leur préfecture. Il montre que le système politique chinois fournit à ses cadres des incitations à encourager une forte croissance du PIB dans la perspective d'obtenir une promotion. L'identification de l'effet causal des incitations générées par le contexte institutionnel provient de la variation du nombre de pairs avec lesquels un bureaucrate donné rivalise pour une promotion dans la hiérarchie politique. Grâce à des données inédites sur la bureaucratie chinoise, ce chapitre montre que la taille de la cohorte initiale, c'est-à-dire le nombre de bureaucrates de rang préfectoral qui débutent leur mandat au même moment dans la même province, est négativement corrélée avec l'évolution de leur carrière. Faire partie d'une petite cohorte favorise la promotion, et est positivement corrélé avec le taux de croissance du PIB, le taux d'expropriation et l'investissement dans l'immobilier. Ces résultats suggèrent que l'accent mis sur la croissance du PIB comme critère d'évaluation des cadres du Parti les pousse à privilégier une croissance rapide poussée par des investissements immobiliers, ce qui d'une part entraîne une forte augmentation des expropriations de terres rurales, et d'autre part semble se faire au détriment des services publics.

Le deuxième chapitre s'intéresse aux conséquences du choc économique mondial de 2008 sur les migrations internes en Chine, et montre que la stabilité des taux agrégés de migration masque des réactions hétérogènes selon les individus. En utilisant l'exposition de l'industrie manufacturière chinoise au commerce international comme source de variation, ce chapitre montre que les migrants « typiques » (de jeunes hommes à niveau intermédiaire de qualification) qui ont migré vers les préfectures les plus durement touchées par le choc de la fin 2008, ne migrent pas moins en 2009, alors que les migrants plus marginaux ont quant à eux réagi en migrant d'autant plus que le choc était important. Ceci suggère un effet de revenu sur ces ménages les plus durement touchés. Parmi les ménages ruraux qui n'ont pas été directement affectés par la crise, c'est-à-dire qu'aucun des membres du ménage n'était migrant en 2008, la probabilité qu'un membre du ménage migre en 2009 décroît avec l'intensité du choc dans les destinations des migrants du village, ce qui suggère un effet de transmission d'information au sein des communautés d'origine.

Le troisième chapitre, co-écrit avec Alessandro Tondini, évalue l'effet de la réforme de l'OAP, le minimum vieillesse sud-africain, et ses conséquences sur l'équilibre entre emploi formel et informel. La réforme a fait passer en 2010 le seuil d'éligibilité à ce transfert mensuel de 65 ans à 60 ans pour les hommes, tandis qu'il est resté inchangé à 60 ans pour les femmes. Nous identifions l'effet de l'éligibilité à 60 ans sur l'offre de travail des seniors sud-africains en comparant la discontinuité des comportements à 60 ans avant

et après la réforme. En utilisant cette différence de discontinuités, nous examinons en particulier s'il existe un effet d'entraînement vers le secteur informel. Comme les revenus d'un emploi informel de facto n'entrent pas dans le critère des ressources, les individus seraient implicitement incités à cumuler la pension avec les salaires cachés du travail informel. Nous montrons qu'au contraire, l'effet de la réforme se concentre sur l'emploi informel ce qui suggère des effets de revenu importants, et une absence de substitution entre emploi formel et informel.

Thesis summary

This thesis is a collection of three independent research projects in applied development economics. All three chapters study empirically the behavior of agents set in particular institutional frameworks.

The first chapter, co-written with Maiting Zhuang, studies the behavior of local Chinese Communist Party secretaries between 1996 and 2014, and the economic performance of the prefectures they rule. It shows that the Chinese political system incentivizes local bureaucrats to generate high growth, and that this translates into higher real estate investment and higher expropriation rates of rural land. Identification of the causal effect of the competitive incentives that a bureaucrat faces stems from the variation in the number of direct competitors with which he competes for higher office. Using an original dataset on Chinese bureaucracy that we collected for this study, we show that the size of a starting cohort, that is, the number of prefecture-level secretaries who start their term in the same year and province, is negatively correlated with the career perspectives of said bureaucrats. A smaller starting cohort size is associated with higher nominal and real GDP growth, real estate investment and expropriation rates. These results suggest that the emphasis put by the Chinese political system on the growth of GDP as a criterion for economic performance and career advancement, leads bureaucrats at a local level to spur investment to generate rapid growth, which leads them to increase expropriations of local rural land, and also seems to decrease public good provision.

The second chapter looks at the consequences of the 2008 global financial crisis on the internal migration patterns in China. It shows that the apparent stability of aggregate migration rates hides substantial heterogeneity across individuals of different characteristics. I leverage data on the ex-ante exposure of the Chinese manufacturing industry to international trade to examine the causal impact of the 2008 shock on migrant's destinations on migration decisions in 2009. Typical migrants –young men with intermediate qualification– do not migrate less as a consequence of facing a more adverse shock at their destination. Marginal migrants on the contrary, migrate more. This suggests a substantial income effect on migrant households, who migrate more to compensate the loss in income. In addition, non-migrant households, that is households where no member migrated in 2008, migrate less in 2009 when the migration destinations of 2008 migrants from their village was harder hit by the shock. This suggests that information on the shock transmits through local networks.

The third chapter, co-written with Alessandro Tondini, evaluates the impact of the non-contributory Old Age Pension scheme in South Africa and its consequences on formal and informal employment of the elderly. This scheme consists in a fixed monthly transfer of 1000 RND (200\$ PPP). The reform has changed the eligibility rule from 65 years to 60 years for men, while it remained unchanged at 60 for women. We first show that identifying correctly the effect of eligibility on the labor supply of the elderly requires to correct the simple post-reform discontinuity at 60, as a discontinuity is already observed pre-reform, due to the presence of other pension schemes. We therefore use a

difference-in-discontinuity strategy, and examine in particular whether the reform has had a displacement from formal to informal employment. As informal earnings are de facto not taken into account by the means-test, one could expect that informal employment becomes more attractive for newly eligible 60-to-64 year olds, who could cumulate informal labor earnings and the pension. On the contrary, we observe no displacement effect, and the drop in elderly labor supply at 60 is almost entirely driven by informal employment. This suggests a large income effect from the pension, and that substitution effects are negligible.

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Introduction générale

Cette thèse est une thèse par articles, et rassemble trois projets de recherches indépendants, bien qu'ils présentent des similitudes dans leur méthodologie et leur objet d'étude. Cette introduction présente ces points communs, puis détaille le contenu de chaque chapitre.

Du point de vue méthodologique, chaque chapitre de cette thèse procède d'une même démarche microéconomique et empirique. Ceci signifie que chaque chapitre identifie une population d'individus et un type de décisions que ceux-ci prennent, et étudie à l'aide de données empiriques individuelles les mécanismes à l'oeuvre dans cette décision. Les populations considérées sont différentes dans chaque cas, et pour chacune, les décisions à prendre concernent le niveau d'effort que les individus dans ces populations fournissent à l'égard d'une certaine tâche. Ainsi, le premier chapitre considère la population des cadres locaux du Parti Communiste Chinois, et s'intéresse au niveau d'effort encouru par ces cadres pour obtenir une promotion au sein de la hiérarchie politique et administrative. Dans ce cas, l'effort, difficilement observable, n'est pas mesuré de manière directe ; ce sont diverses mesures de performance de la division administrative sur laquelle chaque cadre a autorité qui permettent d'identifier le comportement de ceux-ci. Dans cet exercice, nous autorisons également l'effort fourni par un cadre donné à être multidimensionnel, c'est-à-dire que la décision de l'agent ne se résume pas à choisir quel niveau d'effort choisir, mais encore quel type d'effort fournir pour atteindre son objectif bureaucratique. Dans le deuxième chapitre, qui a pour objet d'études la population des ménages ruraux chinois et leurs mouvements migratoires à l'intérieur de la Chine, l'effort est celui que les membres de ces ménages fournissent en vue de migrer pour des raisons professionnelles. Pour un individu, le fait de rechercher du travail dans une préfecture différente que celle où son ménage est établi et enregistré représente un effort, tant par les coûts directs

qu'une telle migration -dans ce cas, temporaire- implique, que par les coûts d'opportunité associés aux activités, ou à l'inactivité, que l'individu pourrait mener en l'absence de migration. Dans le troisième chapitre, l'offre de travail des hommes sud-africains autour de 60 ans est étudiée, et sa réactivité à la présence d'une retraite minimum, assimilable à un minimum vieillesse, à l'âge de 60 ans ; et dans ce dernier cas le niveau d'effort dont ces individus décident correspond au nombre d'heures travaillées et au secteur d'activité qu'ils choisissent.

Considérée dans un sens général, c'est donc à l'offre de travail des individus que chacun de ces chapitres s'intéresse. Dans cette mesure, chaque chapitre considère un contexte institutionnel précis et la manière dont celui-ci informe l'offre de travail des individus qui lui sont confrontés. Chaque contexte fournit à la fois des contraintes au comportement des agents, et détermine la nature des incitations qui font l'objet de l'étude. En effet, c'est le contexte qui dans une grande mesure détermine les choix possibles pour les individus : exproprier ou non des paysans et favoriser ou non la croissance de sa préfecture, migrer ou non vers une autre préfecture, prendre ou non sa retraite. C'est lui aussi qui détermine l'utilité potentielle associée à chacun de ces choix ; qui prend la forme, dans le cas des bureaucrates chinois, de leur probabilité de promotion à l'issue de leur mandat ; dans le cas des ménages ruraux, de leur utilité espérée de la migration et de rester chez soi ; et dans le cas des seniors sud-africains, de l'utilité associée à leur statut d'emploi. Ces trois chapitres constituent donc, dans un sens classique du terme, des études en économie, étudiant le comportement d'individus dans leur allocation d'une ressource limitée, leur travail, en vue de leur objectif.

Ce sont encore, par leur objet, des études en économie du développement. La première traite du rapport entre la performance économique de régions chinoises et les incitations individuelles de ses décideurs. Le taux de croissance de l'économie chinoise ayant été remarquablement haut pendant plus de trente ans, la Chine est un terrain d'enquête privilégié pour l'étude du développement économique. Cependant, les spécificités du régime politique chinois sont souvent invoquées, notamment par les principaux intéressés, pour expli-

quer les dynamiques de l'économie chinoise ; mais participent dans le même temps parfois à écarter, dans le domaine académique, la Chine du champ de l'économie du développement. Le premier chapitre de cette thèse interroge donc directement la relation entre économie politique, c'est-à-dire l'étude du processus de décision politique, politiques économiques, c'est-à-dire les décisions politiques en elles-mêmes, et croissance économique, c'est-à-dire un des résultats de ces décisions. Il est difficile de trouver d'autres pays, époques ou régimes politiques où les conclusions de ce chapitre trouveraient un parallèle univoque, tant l'histoire récente chinoise et les caractéristiques démographiques et économiques du pays sont spécifiques. Cependant, au-delà de l'intérêt propre que présente l'étude de la Chine par la taille de sa population, le poids de son économie, ou encore son importance historique, l'analyse du rôle des bureaucraties dans la performance d'une organisation est riche en enseignements d'une portée plus générale. D'une certaine manière, ce premier chapitre permet d'interroger, en creux, la notion même de performance d'une organisation et des critères appropriés pour la mesurer. Le deuxième chapitre, en analysant les dynamiques migratoires internes à une vaste région géographique, touche également à un domaine éminent de l'économie du développement. Le contexte chinois se distingue une nouvelle fois par sa spécificité institutionnelle, et par les contraintes qu'il impose sur le mouvement des individus à l'intérieur du pays. Celles-ci participent à l'émergence d'importants flux répétés de travailleurs qui parcourent régulièrement plusieurs centaines ou milliers de kilomètres pour un emploi. Dans la mesure où les économies en développement se caractérisent par des flux migratoires de plus en plus nombreux et soutenus, notamment régionaux, l'étude du cas de la Chine offre un point d'entrée particulièrement utile pour comprendre les dynamiques migratoires. Enfin, le troisième chapitre aborde la question des prestations sociales dans les pays émergents. La construction et l'élaboration d'un Etat social fait partie des enjeux auxquels sont progressivement confrontés les pays en développement et émergents à mesure que leur niveau de revenu se rapproche de celui de pays industrialisés de longue date, à l'appareil social généralement plus élaboré. L'Afrique du Sud et son minimum vieillesse non-contributif, l'Old Age Pension, précoce dans son

implémentation d'une couverture sociale universelle des personnes âgées, constitue ainsi un contexte particulièrement adapté pour aborder les questions auxquelles sont et seront de plus en plus confrontés de nombreux pays émergents.

Le premier chapitre, co-écrit avec Maiting Zhuang, étudie le comportement des secrétaires préfectoraux du Parti Communiste chinois (PCC) entre 1996 et 2014, et la performance économique de leurs préfectures. Dans le contexte chinois, en effet, les principaux décideurs politiques sont les cadres du PCC, et ce à chaque échelon de la hiérarchie complexe de celui-ci. Il est donc particulièrement pertinent d'identifier les mécanismes qui motivent les décisions de ces cadres, ce qui permet d'apprécier l'importance que les institutions politiques et administratives peuvent avoir dans les trajectoires d'économies locales.

Le système politique chinois fournit à ses cadres des incitations à encourager une forte croissance du PIB dans la perspective d'obtenir une promotion ; ainsi, un critère principal officiel pour l'évaluation des cadres politiques est la performance économique de leur région. Le premier chapitre de cette thèse examine si cette structure d'incitations a un rôle dans la performance effective des régions chinoises. D'un point de vue empirique, une des principales difficultés pour étudier ce lien est le double lien de causalité entre perspectives de carrière et performance économique : de bonnes performances économiques peuvent favoriser la carrière d'un cadre, et inversement, de bonnes perspectives de carrière par ailleurs peuvent démotiver un cadre politique à fournir un effort en vue d'accroître la performance économique de sa préfecture. Nous nous intéressons principalement aux effets que les institutions peuvent avoir sur le comportement des agents, alors que la relation causale inverse, c'est-à-dire les déterminants de la carrière de cadres du Parti, ont plus communément fait l'objet de travaux universitaires.

L'identification de l'effet causal des incitations générées par le contexte institutionnel provient de la variation du nombre de pairs avec lesquels un bureaucrate donné rivalise pour une promotion dans la hiérarchie politique. Plus le nombre de pairs avec lesquels

un bureaucrate donné est en concurrence pour des postes plus élevés dans la hiérarchie est grand, plus les perspectives de promotion sont ténues ; et plus les gains marginaux -en termes de probabilité de promotion- issus de l'effort d'un bureaucrate sont faibles. Grâce à des données inédites sur la bureaucratie chinoise, ce chapitre montre ainsi que la taille de la cohorte initiale, c'est-à-dire le nombre de bureaucrates de rang préfectoral qui débutent leur mandat au même moment dans la même province, est négativement corrélée avec l'évolution de leur carrière. Pour un cadre du Parti Communiste, faire partie d'une petite cohorte favorise la promotion, et est positivement corrélé avec le taux de croissance du PIB, le taux d'expropriation et l'investissement dans l'immobilier. Nous mobilisons de nombreuses sources de données administratives, de données satellites, et de données d'enquête pour corroborer nos résultats.

Ces résultats suggèrent que l'accent mis sur la croissance du PIB comme critère d'évaluation des cadres du Parti les pousse à privilégier une croissance rapide poussée par des investissements immobiliers, ce qui d'une part entraîne une forte augmentation des expropriations de terres rurales, et d'autre part semble se faire en partie au détriment des services publics.

Le deuxième chapitre s'intéresse aux conséquences du choc économique mondial de 2008 sur les migrations internes en Chine. La Chine des années 2000 et 2010 se caractérise par des flux migratoires internes importants, principalement des régions rurales vers les centres urbains. Une part importante de ces migrations sont temporaires, et durent moins d'une année ; elles sont le fait de migrants qui restent encore fortement attachés à leur ménage rural, et effectuent des mouvements pendulaires entre leur préfecture d'origine et des marchés du travail urbains. Ces migrants sont plus généralement des hommes, âgés de 25 à 35 ans, avec un niveau intermédiaire de qualifications ; leur secteur d'emploi principal est l'industrie manufacturière. A partir du mois d'octobre 2008, les exportations chinoises ont connu une période de croissance négative pendant huit mois. Cette crise a touché principalement les secteurs où les migrants ruraux étaient fortement

représentés. Le deuxième chapitre de cette thèse étudie dans quelle mesure cette crise, dont l'origine est une baisse de la demande mondiale de biens chinois, a modifié les comportements migratoires des ménages ruraux chinois. A l'hiver 2009 en effet, d'importants flux migratoires ont été observés et ont fait l'objet de l'attention des medias chinois.

Ce chapitre répond à deux enjeux empiriques importants. Le premier est l'endogénéité du choc macroéconomique qui affecte les marchés urbains et des décisions de migration. En effet, les migrations ont également un impact sur la situation macroéconomique des différents marchés urbains. Il importe donc de trouver une mesure du choc sur les emplois de migrants qui ne soit pas elle-même déterminée par les comportements de migration des ruraux. Le second enjeu empirique est d'étudier la réaction à un même choc d'individus et de ménages très hétérogènes. Ce chapitre, pour répondre au premier enjeu, utilise l'exposition avant la crise de l'industrie manufacturière chinoise au commerce international comme source de variation, en mesurant la part des exportations l'année avant la crise dans la production manufacturière totale des différentes destinations de migration. Il montre ainsi que tous migrants de 2008 confondus, l'intensité du choc sur les exportations a plutôt un effet négatif sur la probabilité de continuer à migrer en 2009 : plus les destinations de migration des migrants de 2008 ont été durement touchées par la crise, plus les migrants concernés ont tendance à continuer à migrer en 2009. Cet effet globalement négatif, mais assez imprécisément estimé, cache une grande disparité de réactions. En distinguant selon les caractéristiques observables des individus, il est possible de détailler ce résultat : les migrants « typiques » (de jeunes hommes à niveau intermédiaire de qualification) qui ont migré vers les préfectures les plus durement touchées par le choc de la fin 2008, ne migrent pas moins en 2009, alors que les migrants plus marginaux (des femmes, ou des migrants plus âgés) ont quant à eux réagi en migrant d'autant plus que le choc était important. Ceci suggère un effet de revenu sur ces ménages les plus durement touchés. La crise rend en effet les destinations urbaines moins attractives ; mais au premier ordre, elle a surtout un impact négatif sur le revenu des ménages. Comme les gains espérés à la migration demeurent plus élevés que les gains espérés à la non-migration, même si

moins qu'auparavant, les ménages ruraux compensent cette perte de revenu en migrant davantage.

Pourtant, au niveau agrégé, les taux de migrations restent stables entre 2008 et 2009. En effet, parmi les ménages ruraux qui n'ont pas été directement affectés par la crise, c'est-à-dire qu'aucun des membres du ménage n'était migrant en 2008, la probabilité qu'un membre du ménage migre en 2009 décroît avec l'intensité du choc dans les destinations des migrants du village, ce qui suggère un effet de transmission d'information au sein des communautés d'origine.

Le troisième chapitre, co-écrit avec Alessandro Tondini, explore le rôle des incitations dans la répartition sectorielle sur un marché du travail segmenté. La présence d'un secteur informel dans l'économie, un trait commun à beaucoup d'économies en développement, suggère que la mise en place de programmes sociaux peuvent introduire des distorsions sur le marché du travail. En effet, les programmes sociaux soumis à condition de ressources peuvent entraîner des flux d'emploi du secteur formel vers le secteur informel non réglementé. Dans la mesure où les revenus informels n'entrent pas dans le calcul des ressources soumises au critère de ces programmes sociaux, les individus peuvent être incités à choisir le secteur informel pour pouvoir continuer à travailler -et percevoir des revenus- tout en bénéficiant du transfert. L'importance des flux du secteur formel au secteur informel est fonction des raisons pour lesquelles les choix entre ces deux secteurs se font, et si ces secteurs sont substituables du point de vue des travailleurs.

Pour répondre à cette question, ce troisième chapitre évalue le programme social de pension retraite minimale non-contributive sud-africaine, la *Old Age Pension*. Ce dernier chapitre adopte ainsi par certains aspects une démarche méthodologique différente des deux autres, dans la mesure où il évalue l'effet de la réforme d'une politique publique, et tire, à partir du comportement des individus, des conclusions à propos de la structure du marché du travail sud-africain. La réforme en question a fait passer en 2010 le seuil

d'éligibilité au minimum vieillesse de 65 ans à 60 ans pour les hommes, tandis qu'il est resté inchangé à 60 ans pour les femmes.

Un enjeu empirique majeur de ce travail est l'identification de l'effet causal de l'éligibilité à 60 ans sur l'offre de travail des seniors sud-africains. En effet, en l'absence de l'OAP, c'est-à-dire avant la réforme, il existe une discontinuité dans les comportements de travail des hommes au seuil de 60 ans, en raison de l'existence d'autres régimes de retraite privés. C'est donc en faisant la différence de deux discontinuités, avant et après la réforme, que nous identifions correctement l'effet de l'éligibilité à 60 ans. Nous montrons par là que l'effet de la réforme se concentre sur l'emploi informel : la probabilité d'être employé dans le secteur informel diminue fortement au seuil de 60 ans, alors que celle d'être employé dans le secteur formel reste globalement inchangée.

Nous distinguons ensuite les réactions de l'offre de travail en fonction du niveau de revenus associés au travail. Nous montrons que les effets sont encore plus concentrés sur l'emploi à de bas niveaux de salaires, dans le secteur informel comme dans le secteur formel. Ils restent cependant importants dans le secteur informel à des niveaux intermédiaires de revenu, et ne disparaissent qu'à des niveaux élevés. Ceci suggère que ce ne sont pas seulement les travailleurs aux salaires les plus bas qui réagissent à la présence de la retraite minimale en abandonnant leur travail. De plus, à niveau de salaire égal, les travailleurs informels quittent leur travail alors que les travailleurs formels ne le font pas. Cette asymétrie est révélatrice des différences entre travail informel et formel, ou bien par les caractéristiques des individus qui se portent vers l'un ou l'autre secteur, ou bien par les caractéristiques intrinsèques de ces secteurs.

Nous étudions enfin si la réforme de l'OAP, en incitant les travailleurs sud-africains à quitter leur travail, a eu un effet positif sur l'emploi des autres catégories de travailleurs. Bien qu'elle ait un impact relatif très important sur l'emploi à 60 ans des hommes, la réforme de l'OAP n'a eu qu'un effet modéré sur l'emploi agrégé, à cause de la petite taille de la population bénéficiaire concernée. Nous concentrons donc notre analyse sur les groupes de travailleurs a priori les plus proches de ceux directement touchés par la

réforme, et étudions l'emploi des 55-59 ans dans les secteurs où les hommes de 60-64 étaient les plus nombreux avant la réforme. Nous sommes en mesure de rejeter l'hypothèse d'une substitution parfaite entre ces deux groupes -par laquelle un travailleur de 55-59 ans aurait remplacé chaque travailleur partant à la retraite- mais le pouvoir statistique offert par la magnitude de la réforme est trop faible pour nous permettre de tirer des enseignements plus précis. En tout état de cause, dans des pays où la structure démographique est telle que celle de l'Afrique du Sud, c'est-à-dire avec relativement peu de personnes âgées de 60 ans et plus, il est vraisemblable que ce type de réforme des retraites n'ait qu'un impact limité sur le marché du travail dans son ensemble.

Cette thèse mobilise ainsi des méthodes économétriques variées pour traiter de trois sujets au coeur de l'économie du développement. C'est le caractère résolument empirique qui unit ces trois chapitres dans l'ambition de faire dialoguer la modélisation microéconomique et le travail sur des données issues du monde social.

Chapter 1

Economic performance, land expropriation and bureaucrat promotion in China

co-written with Maiting Zhuang (PSE, EHESS)

We assess the effectiveness of using career incentives to motivate bureaucrat performance. We collect data on the career history of all Chinese Communist Party (CCP) secretaries, who were in power in any of China's prefectures between 1996 and 2014. We identify the causal effect of career incentives on performance by exploiting variation in the competitiveness of promotions. We find that the presence of fewer competitors increases bureaucrats' promotion likelihood and causes them to adopt a strategy that relies on real estate investment and rural land expropriation, resulting in faster growth in construction employment, higher secondary sector GDP growth; but also suggestive evidence that they lower investment in education, public transport and health. We corroborate our findings using survey and remote sensing data, and present evidence that expropriations are positively correlated with adverse individual outcomes, with arrests of local officials, and with the emergence of "ghost cities".

1.1 Introduction

Bureaucrats around the world are responsible for implementing social and economic policies. Yet the question of how to motivate them remains poorly understood. Unlike in private sector organizations, bureaucracies tend to be characterized by strict selection criteria for entry. Once admitted, chances of being demoted or fired are low and pay follows rigid seniority-based rules. Potential moral hazard problems in bureaucracies can be mitigated through performance-based promotion criteria. The implementation of a system of yardstick competition between local government officials has often been cited as key for China’s fast growth in recent decades (see, for example, Li and Zhou, 2005; Maskin, Qian and Xu, 2000). In this paper, we identify the causal effect of promotion incentives on performance. We find that career incentives push bureaucrats to choose a policy mix that leads to faster growth through increased construction and real estate investment, but at the cost of increased expropriation of rural land; we also find suggestive evidence of detrimental effects of those incentives on public good provision. The welfare effects of performance-based promotions can thus be ambiguous.

Using both administrative and online sources, we identify the party secretaries of the Chinese Communist Party (CCP) of all prefectures in China from 1996 and 2014 and collect a number of personal characteristics, as well as their past and future career path. This information allows us to assess whether a bureaucrat was promoted, demoted, transferred laterally, or retired at the end of their term. To the best of our knowledge, this is one of the most comprehensive datasets collected on this level of the Chinese bureaucracy.

Identifying the causal effect of promotion incentives on policy choices is subject to many potential endogeneity concerns. A bureaucrat’s characteristics may jointly determine his performance and his ability to advance in the hierarchy. For instance, prefecture party secretaries with personal connections might be placed in higher growth locations. We construct an exogenous measure of competition intensity, which determines the like-

likelihood that a CCP secretary will be promoted at the end of their term.

The primary competitors of a prefecture-level CCP secretary are other prefecture-level CCP secretaries in the same province. The probability that a CCP secretary of a given prefecture is promoted after his term depends negatively on the number of other prefecture party secretaries in the province who start their term at the same time. We show that, conditional on prefecture and start-year fixed effects, the size of a prefecture CCP secretary's starting cohort is unrelated to his own age, ethnicity, education and whether or not he is a native. A prefecture party secretary who starts with only four other party secretaries is 10.4 percentage points more likely to be promoted than a party secretary who starts with eight other party secretaries.

The effect of having more competitors on performance is *ex ante* ambiguous. We show that the Chinese bureaucracy's system of yardstick competition generates incentives akin to contests between a varying number of players for a fixed number of higher-ranked seats. A smaller number of competitors increases a bureaucrat's incentives to provide effort as it increases his chances of getting promoted. Empirically, we find that fewer competitors increases GDP growth rates over a prefecture party secretary's term: a decrease by one standard deviation in the number of a CCP secretary's competitors (or 2.4 fewer competitors) increases yearly nominal GDP growth by 0.8 percentage points, and real GDP growth rate by 0.3 percentage points in each year of his term. These faster growth rates are driven by higher real estate investment and faster growth in construction employment. Using satellite data, we find faster urban area growth in the urban areas of prefectures whose party secretaries face better chances of advancing through the CCP hierarchy.

We show that this type construction-led growth strategy comes at the cost of increased expropriations of rural land, which we can measure directly using retrospective land history data from the China Health and Retirement Longitudinal Survey (CHARLS). Bureaucrats can resell the user rights of expropriated land to private developers in order to boost government revenue and investment and GDP figures, as well as undertake large-scale infrastructure projects, which is one way for gaining prominence within the

bureaucracy. A decrease by 1 standard deviation in the size of a CCP secretary's cohort increases the individual probability of being expropriated by 14 percent in the prefecture and years of this secretary's term.

For the farmers losing their lands, however, we find that being expropriated is associated with adverse outcomes later in life. According to news reports, inadequate compensation and forced eviction of farmers trigger two thirds of all social protests in China.¹ The Chinese central government itself estimated in 2007 that over 20 percent of land was illegally expropriated by local officials and the problem has been growing since (see Figure 1.B.6).² We find that our measure of expropriation is positively correlated with bureaucrats' arrests during the recent anti-corruption campaign.

A smaller number of competitors, which increases the promotion probability, also may lead to worse outcomes for policies that are less politically visible. For instance, we find suggestive evidence that these party secretaries underinvest in education, transport and health, as measured by the number of teachers per capita, the number of buses per capita, and the number of doctors and hospital beds per capita in the prefecture. These results, while statistically less precise, point to a potential substitution effect between more and less visible efforts from the part of bureaucrats.

Our paper contributes to a wider literature on understanding the impact of bureaucracies on economic performance (see, for example, Finan, Olken and Pande, 2015, for a recent review). We use the particular institutional context to identify exogenous variation in the intensity of promotion incentives that a bureaucrat faces. In this respect, our paper is similar to Bertrand et al. (forthcoming) who use a bureaucrat's age at entry into the Indian civil service as a measure for career incentives and find that stronger incentives result in better performance. While our results also show that career incentives can lead to a better headline measure, they also highlight the potential detrimental effects of providing measurable performance targets. This connects our work to the literature on multitask-

¹<https://www.reuters.com/article/us-china-land/china-ministry-urges-end-to-forcible-land-requisition-paper-idUSBRE94E04320130515>

²See <http://en.people.cn/90001/90778/6272123.html>.

ing problems in principal-agent relationships, as first studied in Holmstrom and Milgrom (1991). In a context where a principal wants to incentivize the completion of several tasks by an agent, the desirability of providing incentives for any one activity decreases with the difficulty of measuring performance in any other activities that make competing demands on the agent's time and attention. In our context, the emphasis on one measure of performance by the Chinese bureaucracy shifts bureaucrats' effort investment to the completion of the task that is emphasized, namely economic GDP growth.

A large literature in economics and political science have studied the promotion criteria for Chinese bureaucrats (see Section 1.2 for a detailed literature review). While these papers have largely focussed on the question of whether economic performance affect promotion, in this paper we ask the reverse, that is, how promotion incentives affect a bureaucrat's policy choices. Similar to Fisman and Wang (2017); Jia (2017), our results outline some drawbacks of the Chinese bureaucrat promotion system despite its effectiveness in delivering fast growth.

Our paper also relates to the literature on land markets. For instance, Chen and Kung (2018) show how Chinese bureaucrats give land price discounts to firms that are connected to higher-ranking officials in return for better promotion prospects. This is in line with our results that when faced with greater promotion incentives, bureaucrats are more likely to expropriate rural land. Consistent with a large literature in development economics about the importance of land security (see, e.g. de Janvry et al., 2015; Field, 2007; Goldstein and Udry, 2008), we find that being expropriated is correlated with negative outcomes at an individual level.

Section 1.2 provides a brief description of the Chinese bureaucracy and of the context. Section 1.3 describes the various sources of data, notably on CCP secretaries and expropriations. Section 1.4 discusses our empirical strategy and our identifying assumptions. Section 1.5 presents our results, and Section 1.6 discusses the robustness of these results. Section 1.7 concludes.

1.2 Background

1.2.1 Chinese bureaucratic system

The economic benefits of the Chinese promotion system have spurred a recent but rapidly growing literature. (Xu, 2011) details the architecture of the political system and its promotion dynamics. The Chinese political hierarchy is divided into several administrative levels: centre, provinces, prefectures, counties and villages. Each level is administered by a Chinese Communist Party (CCP) standing committee and a government.³ CCP positions wield political power, while government positions relate to the administration and implementation of policies. Therefore, the secretary of the CCP standing committee is the highest-ranking official at any given level.

At the top of the hierarchy is the centre’s Politburo of the CCP (ranks 1 and 2). Lower ranks are split according to either government ministries or geographic subdivisions. Government ministers hold rank 3, the same rank as party secretaries and governors of China’s 31 provinces, and their deputies hold rank 4. This thematic-geographic split is then replicated. Provinces are divided into 334 prefectures, which are then further divided into counties and villages. Province-level ministers hold the same rank as prefecture-level party secretaries and mayors (rank 5). In addition, there are 15 vice-provincial level cities that are ranked in between a prefecture and a province. And so on. In this paper, we focus on prefecture (and vice-provincial level city) party secretaries, who are the highest-ranking officials in their locality and hold rank 5 (and 4).

Bureaucrat promotions are generally decided by their immediate superiors, for example, promotions of prefecture party secretaries are decided by their provincial party standing committee. While there are exchanges of officials across provinces, these are mostly lateral transfers instead of promotions. This means that in practice, the main competitors for a prefecture party secretary are other prefecture party secretaries within

³Some individuals may jointly hold positions in both, for example, Xi Jinping (习近平) is both the General Secretary of the CCP and the President of China.

their province. The criteria for bureaucrat promotions in China have been subject to debate in the literature (see, e.g. Landry, Lü and Duan, 2018, for a recent review). Chen, Li and Zhou (2005); Li and Zhou (2005) show the importance of GDP growth in the promotion of provincial-level officials, while Persson and Zhuravskaya (2016) and Shih, Adolph and Liu (2012) find that other factors, such as networks and patronage effects, also play an important role. Chen and Kung (2016) show that higher land revenue increases county-level bureaucrats' chances of promotion and weakens the link between economic growth and promotions. Landry, Lü and Duan (2018) find that the relationship between economic growth and promotions are weaker at the prefectural level compared to county level.

Bureaucrats face an age limit for promotions which varies with their rank. Prefecture party secretaries who are 55 or older are in theory no longer eligible for promotions, although we find that this rule is not always enforced (see Section 1.3). Due to this age limit, bureaucrats who do not get promoted at the end of each term risk never attaining higher positions in the government hierarchy. Older bureaucrats rarely retire outright when their term end, but are instead moved to consultative positions with little real power outside of the CCP and government. This practice is known as “moving to the second front”.⁴

Officially, a bureaucrat's term should be five years long and bureaucrats should not be promoted before spending at least three years in a given position. However, we show in Section 1.3 that terms tend to be significantly shorter in reality. This is in part a response to the age limits for promotions. Kou and Tsai (2014) show that officials who graduated from university and spent five years in each position will never be eligible for promotion beyond prefecture level. Thus, an optimal career path of a CCP official is a succession of short term or “sprinting with small steps”.⁵

Demotions and dismissals of CCP officials were rare prior to the start of Xi Jinping's

⁴The Chinese expression reads 退居二线, *tuì jū èr xiàn*. Most positions in the People's Congress or People's Political Consultative Conference fall in this category.

⁵小步快跑, *xiǎo bù kuài pǎo*. See, for instance, *Nanfang Ribao*, 15 July 2011.

anti-corruption campaign in 2012. Over 1.34 million officials have been punished in Xi's first five years in office, making this the largest purge of party members in China since the Cultural Revolution.⁶

1.2.2 Land markets in China

In China, land use is strictly governed and all land is divided into rural or urban land. While all urban land is government-owned, rural land can be owned either by the local government or by farmer collectives. Households who are registered rural residents, that is, who have a rural *hukou*, are allocated land use rights for housing and farming. The current land rights system is the result of many reforms since the 1980s, which have marked the pick-up in Chinese agricultural productivity and growth.⁷

However, these incomplete property rights on land have led to many disputes and left farmers vulnerable to government expropriations. In recent years, rapid population growth resulted in a boom in real estate and land prices in cities, but adjacent rural land cannot be developed. This large disparity in the value of land between urban and rural sectors gives rise to an incentive for local governments to convert land status. If the land is owned by farming collectives, local governments are only obliged to compensate farmers for the agricultural productivity of the land which is far inferior to the land's market value. The revenue from such land conversions accrue to the local government and is exempt from the tax-sharing agreement with the centre. Many farmers protest these land seizures due to inadequate compensation.⁸

⁶See <http://www.bbc.com/news/world-asia-china-41670162>.

⁷The most significant reform to land tenure date back to the era of great economic reforms post-1978. At the time, ownership of rural land is transferred from the central State to local authorities, and land use rights are allocated to households, on the basis of their registered residence, or *hukou*. The relative rigidity of the *hukou* system, and the power given to local authorities to reallocate land, has made land tenure a central topic of Chinese rural development and of migrations. Indeed, land use rights accrue to people who are registered as rural dwellers, this means that migrating either officially -with a change of *hukou*- or unofficially -the most common case- puts the availability of land at risk, given that the transferability of land use rights is limited outside of the household.

⁸These protests often lead to violence, and sometimes to extreme events, such as self-immolations, that have received attention from Western media in recent years: <https://www.nytimes.com/2013/09/09/world/asia/as-chinese-farmers-fight-for-homes-suicide-is-ultimate-protest.html>

1.2.3 Local bureaucrats' promotion incentives

We think of our setting as a contest between multiple players with similar characteristics, who compete against each other for a fixed number of prizes. In our setting, there is a fixed number of players, prefecture-level CCP secretaries, who are competing for higher offices. The relevant players for each contest are the CCP secretaries from a given provincial cohort, that is, the prefecture-level secretaries who start in the same year and in the same province. Players compete against each other by exerting effort, which comes at some cost; and the probability that a given player is awarded a position is an increasing function of his effort. In our setting, the effort that prefecture-level secretaries make in order to secure promotion to higher-ranked positions is effort towards construction-led growth. CCP secretaries have a fixed amount of effort resources that they can spend on construction-led growth, or on other types of effort, such as public good provision. The first type of effort yields utility in the form of promotion probability, the other type yields a utility component that is unrelated to promotion and independent of the behaviour of other contestants.

This type of rent-seeking games has been extensively studied since the seminal contribution by Tullock (1980). A general form and analysis of the game have been formalized by Pérez-Castrillo and Verdier (1992). The common prediction that we take away from them is that the outlay or effort of each player is a decreasing function of the number of competitors.⁹ The predictions from the literature on political tournaments with regards to the number of players is that average efforts towards promotion should decrease with the number of competitors; and in our context, that substitute efforts towards public good provision should increase.¹⁰

⁹This prediction is robust to the relaxation of several assumptions of the original model by Tullock (1980). more general functions for contest success have been studied; the increasing returns to effort notably make asymmetric equilibria possible. Additionally, a similar negative relationship between the number of competitors and the (expected) effort of each competitor exists in games where the game is perfectly discriminating, that is, when the highest effort wins the contest with certainty (see, for instance, Nitzan, 1994); in these games pure strategy equilibria do not exist, but the expectation of mixed strategies in equilibrium is also a decreasing function of the number of competitors.

¹⁰An important feature of these models is that CCP secretaries have information on their competitors

This modelisation of bureaucrat incentives relies on the important assumption that the relevant competitors of a given bureaucrat are his “starting cohort”. We argue that the most similar, and thus primary, competitors of a given bureaucrat are the ones of the same provincial cohort. These are bureaucrats who start at the same rank in the same year in the same province. This does not exclude some degree of competition from other bureaucrats, such as the other bureaucrats in the province from a different cohort, or other bureaucrats of similar characteristics from neighbouring provinces, or even bureaucrats in the central government branches. We could in theory include in our analysis statistics that would characterize more fully the competitive environment in which bureaucrats anticipate their promotion chances. However, several reasons prevent us from doing so. First, the availability of data constrains us to consider only prefecture-level party secretaries; the Chinese bureaucracy is extensive and the collection of data on all potential positions is not tractable. Second, different measures of competition intensity would likely be correlated, making the interpretation of each individual coefficient difficult. Third, some measures of competition such as age or term seniority of competitors would likely be endogenous to the within-term decisions of each bureaucrat.

We detail our conceptual framework in Appendix 1.C in a stylized formal model. This simplified model includes a substitution, promotion-irrelevant type of effort, and predicts that both types of effort, promotion-relevant and promotion-irrelevant are respectively decreasing and increasing functions of the number of competitors that each bureaucrat faces.

(notably the number of their competitors) and expect their competitors to behave in a similar way as themselves. This assumption seems reasonable in the case of the CCP secretaries.

1.3 Data and descriptive statistics

1.3.1 Prefecture CCP secretaries

We collect the name of the CCP secretary and the beginning and start of each term in all 334 prefecture-level administrative units of China using official Provincial Yearbooks from 1996 to 2014.¹¹ We also collect birth dates, other individual characteristics (such as education, gender, ethnicity) and the full career history of each CCP secretary using official sources, Wikipedia and Baidu Baike (a Chinese online encyclopedia similar to Wikipedia). We describe data summary statistics of data availability in Table 1.A.1. There are 1310 terms in the prefecture-level administrative units in China that start in 1996 or later and end in 2013 or earlier. For 98 percent of those, we are able to collect information on the birth date of the CCP secretary, and on competitors' seniority. We collect data on future careers of CCP secretaries: for 1022 out of the 1310 terms (78 percent), we are able to identify at least one position in which the secretary takes office in a period of up to 24 months after his current term ends.

We assign an administrative rank for each position and code promotions as an increase in the administrative rank. We complement this data with internal moves from our dataset, that is, officials who transfer to other prefectures. In our rank assignment, we assign a higher rank to sub-provincial level cities according to official rules.¹² An official is promoted if they attain a position with a higher rank after the end of their term. We also explicitly account for the practice of moving older officials into honorary positions outside of the party and government by coding these moves as retirement, even if these positions are theoretically higher ranked.¹³ More details on how we treat the data on bureaucrats' careers are given in Appendix 1.D.

¹¹Since the precise date of nomination is not available for all secretaries, we use the name of the bureaucrat in office at the end of the year.

¹²Thus, a move from Taizhou (台州市), a prefecture-level city, to neighbouring Ningbo (宁波市), a sub-provincial city, is considered a promotion.

¹³This is a similar approach to Li and Zhou (2005), who consider provincial CCP secretaries.

A prefecture party secretary in China has a 49 percent chance of being promoted at the end of their term over the period from 1996 to 2014 (Table 1.A.1).

Table 1.A.2 shows that there is considerable variations across provinces in promotion rates and length of terms; and Figure 1.B.2 shows that geographical variation also exists within province. This important geographical cross-sectional variation suggests to control for location-specific fixed effects. As there are also substantial temporal swings in promotion probability (see Figure 1.B.3), that are likely to be driven by national dynamics, it is also important to control for year fixed effects.

This is why an important feature of our instrument for promotion incentives is that its variation across time and space is important. The average size of secretary cohorts varies with time (see Figure 1.B.4), but national-level 5-year cycles do not seem to be prevalent. The size of provinces also provides an additional source of variation. In the end, our instrument's values span 0 to 14, with an average of 4 and a standard deviation of 2.5 (see Figure 1.B.1 for the full distribution).

Finally, we identify party secretaries who were investigated or arrested under Xi Jinping's anti-corruption campaign by searching the Baidu Baike entry of each official for a set of keywords.¹⁴ We manually check the correctness of our coding and cross-validate our findings with data from ChinaFile.¹⁵ Of 1310 prefecture party secretaries, 165 (13 percent) were investigated and faced disciplinary sanctions.¹⁶

¹⁴We identify these by looking through the records of a given CCP secretary for a match on any of the following keywords: 'Expulsion from public office' (开除公职), 'Expulsion from Party membership' (开除党籍), 'Corruption' (腐败), 'Bribery' (受贿), 'Legal inspection' (依法审查), 'Suspended for inspection' (停职检查), 'Double designation' (双规; this term is specific to the Chinese Communist Party and refers, as 双开, to a type of internal investigation, that is generally kept secret), 'Illegal' (违法), 'Violation of regulations' (违规), 'Violation of rules' (违纪), 'Crime' (犯罪), 'Imprisonment' (有期徒刑). We then manually check the entries that match those keywords.

¹⁵See <http://www.chinafile.com/infographics/visualizing-chinas-anti-corruption-campaign>.

¹⁶According to Chen and Kung (2018), the campaign singled out land as one of its main targets.

1.3.2 Land expropriations

We obtain data on rural land expropriations from the 2014 CHARLS survey. The dataset is a representative sample of the Chinese population that is aged 45 and above and contains 11,942 individuals¹⁷. They originate from 450 communities across 120 prefectures and 28 provinces of China. CHARLS contains the full retrospective life history of surveyed individuals, as well as their personal characteristics. This allows us to track their *hukou* registration and land tenure histories: all changes of *hukou* are recorded with their date and in almost all cases with the new prefecture. Additionally, all instances of land expropriation are documented, with the corresponding date, acreage and potential compensation. Since land use rights depend on administrative registration, we use the registration history of individuals and the date at which they declare expropriation to infer the prefecture of expropriation. As Figure 1.B.6 shows, the majority of expropriation events occurred after 1996, which corresponds to the start of our data on CCP secretaries.

Since the source of variation that we are exploiting is across prefecture-years cells, it is important to have a sense of the magnitude of the variation that we observe in the data. Of the individuals in the sample, 67 percent have always resided in the same prefecture since their birth. We construct a panel dataset based on the retrospective life history of individuals. Since we have 11,942 interviewed individuals whom we observe for 18 years, we should end up with 214,956 individual-year observations. Of these, we have information on prefecture of residence for of them, or 95.1 percent. If we restrict our sample to individual and year for whom we have complete information over their lifetime residence (ie, no missing value at any point in their life), we end up with 200,608 observations, or 11,146 individuals. Hereafter we call this sample the balanced panel sample, by comparison to the unbalanced panel sample, which corresponds to all year-individual observations with non-missing residence.

We use this individual-level data to construct a prefecture-level yearly panel dataset

¹⁷The sampling of the CHARLS survey is at the individual level; we discard the land information from respondent's spouses.

of expropriation rates. We compute the expropriation rate of a given prefecture in a given year as the number of individuals who were expropriated in that year over the total number of individuals who had a rural hukou in that prefecture in that year. The average yearly expropriation probability in the CHARLS sample over the period is 0.6 percent. 11 percent of the total sample declare having ever been expropriated in their lifetime.

1.3.3 Macroeconomic data

We complement these two datasets with macroeconomic data available through the National Bureau of Statistics of China at the prefectural level. The Chinese City Statistical Yearbooks collect data on an annual basis in all prefecture-level units of China.

The list of available variables sometimes changes over time. The definition of some variables also vary dramatically during our period, which prevents us from using them. This is notably the case of the administrative classification of areas into urban and rural ones, or of the sectoral split of employment in the late 1990s.

We rely in our analysis on the following measures, all of which are consistently measured throughout the period: nominal GDP and nominal and real GDP growth, nominal GDP growth of each sectors, employment in the construction sector and total employment, real estate investment and real estate investment in residential buildings; the number of primary school and middle school teachers employed in the prefecture, the number of doctors employed in public hospitals and clinics in the prefecture, and the number of hospital beds in public hospitals and clinics in the prefecture.

Summary statistics are presented in Table 1.A.3. The average prefecture over the period of interest is 4 million people and 23,000 km² large, with an average nominal GDP growth of 16 percent, and average real GDP growth at 13 percent.¹⁸ Real estate represents

¹⁸These values, even when weighted for the size of prefectures, are larger than the official national-level average GDP growth rates. This can come from several reasons. One is that City Statistical Yearbooks' coverage is better in prefecture-level cities than in other prefecture-level administrative areas, that are typically poorer and more rural. Another is that local statistics have been said to be subject to methodological pitfalls -such as double-counting or lack of data- which increases the discrepancy with aggregate-level data, that is either based on or corrected via centrally implemented surveys. See (Holz, 2014) for a thorough discussion.

an important part of GDP, with an average of 6 percent throughout the sample years.

Because promotion data is missing for a selected subsample of prefectures and years, we conduct balancing tests on our outcome variables. We do this by regressing each of the macroeconomic variables we use on prefecture-level and year-level fixed effects, and a dummy variable for whether promotion data is available or not. We report the point estimates and standard errors of this coefficient in Table 1.A.4. Reassuringly, we do not find a systematic pattern emerging from the availability of promotion data.

Chinese official macroeconomic statistics have been the subject of suspicion of manipulation for years. In a context where performance evaluation is based on figures that are (i) difficult to verify and (ii) reported by the administration which is the object of the evaluation, figure manipulation is a strategy that comes at potentially little cost and high yield. Journalistic and highly publicized official sources on the topic are numerous.¹⁹ Yet the growing academic literature on the topic is not unanimous. At any rate, this suspicion over the quality of Chinese macroeconomic data is particularly focused on local indicators that are considered politically sensitive, such as real GDP growth, or pollution. This is a concern for our empirical strategy, because any relationship between political incentives and economic performance could refer to causal effects on actual performance, or on official data manipulation, or both.

We try to address this concern in several ways. First, we focus on different macroeconomic outcomes that are not as politically sensitive as GDP, and that corroborate our preferred interpretation. Such is the case of real estate investment figures. Second, we try to address directly data manipulation, using the presence and distribution of decimals in reported growth figures. This strategy is inspired by the existing literature on data manipulation, as developed for instance in Holz (2014). Third, we turn to alternative

¹⁹A diplomatic telegram from the ambassador of the United States of America to China, addressed to the USA Secretary of State, dating 2007 and released by Wikileaks, states for instance: “GDP figures are ‘man-made’ and therefore unreliable, [then-executive vice premier] Li [Keqiang] said. [...] When evaluating Liaoning’s economy, he focuses on three figures: 1) electricity consumption, which was up 10 percent in Liaoning last year [...] All other figures, especially GDP statistics, are ‘for reference only,’ he said smiling.” https://wikileaks.org/plusd/cables/07BEIJING1760_a.html This leaked declaration has become infamous, to the extent where electricity consumption has been dubbed the “Li Keqiang index”.

measures of GDP growth using external data.

1.3.4 Remote sensing data

GDP growth Remote sensing techniques have been used to measure economic development starting in the 2000s and the increase in the accessibility, range and precision of satellite data. Since the seminal work of Henderson, Storeygard and Weil (2012, 2011), the intensity of night lights in a given area has been commonly discussed as a potential proxy of economic development. Using this type of data raises technical issues. Because of local economic and physical characteristics, inter-regional comparisons of levels are seen as less informative than comparisons of growth rates. Recent work has suggested that even the relationship between economic growth and growth of light intensity might be unstable across regions (Bickenbach et al., 2016). An additional problem in the construction of time series is that satellite data typically come in shorter series of a few years that must be appended, each series stemming from a different satellite system. These changes in satellite systems induce substantial breaks in the levels of nightlight intensity, in its growth rate, and in the temporal trends of this growth rate. Those breaks shed doubt on the capacity of nightlight data to approximate economic growth in a way that is consistent over time and space (see for instance Zhang and Seto, 2013). Advances in the treatment of the raw data have been made in recent years; we use in this paper data that was compiled by Zhang, Pandey and Seto (2016), and that uses overlapping periods between satellite systems to smooth time series of night light intensities. We give a comparison of how well the raw nightlight data and the processed data approximate aggregate-level Chinese GDP in Appendix 1.E. As an alternative measure to absolute growth rates, we compute, for each year, the percentile rank of each prefecture in the distribution of growth rates, and use this as an outcome variable.

Given the important caveats pertaining to remote sensing data as a proxy for GDP, its capacity to disentangle true effects on GDP growth from data manipulation is limited to positive results. Robust results using such data might corroborate findings using official

data, but the absence of statistically precise results can hardly be interpreted as evidence that official sources are corrupt or manipulated.

Urban expansion The wide availability of remote sensing data has also fed a large increase in the measurement capacity of urban expansion via remote sensing data. We use readily available data processed by Yao et al. (2018).²⁰ For similar reasons to those pertaining to “satellite” GDP growth rates, we also use percentile ranks of urban area growth as outcome variables.

1.3.5 Supplementary data

The characteristics of urbanization in China have raised numerous concerns about its efficiency and its potential externalities. The size distribution of Chinese cities has notably spurred a lot of academic attention, starting with Au and Henderson (2006). Patterns of development of cities also raise interesting efficiency issues (see Burchfield et al., 2006), and the measurement of urban sprawl in China has been a growing field in recent years (see for instance Yue, Zhang and Liu, 2016). The welfare implications of urban sprawl however are not unequivocal, and difficult to identify. We rely on an arguably more direct assessment of potential externalities of urban expansion. A small literature based on remote sensing and big data analysis has developed techniques to identify so-called “ghost cities” in China, or cities where the apparent vacancy rate of real estate is particularly high.²¹ Jin et al. (2017) provide a list of areas identified as ghost cities.²² Ghost cities are identified through discrepancies, at a fine-grid local level, of activity between older and newer urban settlements, where activity measured by data on Internet usage and local points of interest. We use these sources and match them to average expropriation rates.

²⁰We also check that data from He, Huang and Ye (2014) yields similar observations

²¹One of the examples that has received the most media exposure is Ordos (鄂尔多斯) in Inner Mongolia.

²²They also provide alternative lists found in different sources, which we also collect.

1.4 Identification strategy

We are interested in the causal effect of promotion incentives on bureaucrat performance, and we measure the intensity of these incentives by the number of competitors that CCP secretaries face for promotion.

The first step of our empirical analysis is to show that our measure of promotion incentives has an impact on the promotion likelihood of CCP secretaries. We estimate the following equation:

$$P_{c,t} = \zeta Z_{c,t} + \alpha_c + \delta_t + \eta_{c,t} \quad (1.1)$$

where $P_{c,t}$ is a dummy variable that takes the value of 1 if the prefecture's party secretary is promoted at the end of his term, $Z_{c,t}$ is the number of prefecture party secretaries in the same province as a given party secretary of prefecture c , who start their first year of term in the same year as said party secretary, and α_c and δ_t are prefecture and year fixed effects, respectively, and $\eta_{c,t}$ is a residual term at the year-prefecture level. $Z_{c,t}$ is defined at the time a CCP takes office in a given prefecture, so that it takes the same value for all the years that this given CCP secretary stays in office in this prefecture.²³ $Z_{c,t}$ is also symmetric for all prefecture party secretaries who start their term in the same year in the same province. We cluster $\eta_{c,t}$ at the province - start year level.

The OLS estimator of ζ is unbiased and consistent if the conditional expectation of $\eta_{c,t}$ with regards to $Z_{c,t}$, α_c and δ_t is 0. As we take into account prefecture fixed effects, we implicitly account for differences in the number of prefectures across provinces, as well as any other time-invariant prefecture-level characteristics. Year fixed effects allow us to rule out China-wide macroeconomic shocks that coincide with changes in the central CCP leadership which also generates increased turnover at lower levels. It also allows to

²³If a CCP secretary executes two terms in different prefectures, $Z_{c,t}$ refers to the starting cohort of each term in each prefecture.

account for all nation-wide shocks that may affect the supply of bureaucrat candidates to the position of prefecture-level CCP secretary. A potential identification challenge would arise if particularly able or motivated bureaucrats could choose their prefecture and start year so as to minimise competition. We show in the next sections that $Z_{c,t}$ is not correlated with a range of party secretary characteristics and that all our results are robust to controlling for CCP secretary characteristics. We show in Section 1.6 that the size of a bureaucrat’s starting cohort is not correlated to trends in the bureaucrat’s prefecture’s outcomes *prior* to the bureaucrat’s term. In the same spirit, we also perform placebo tests, replacing a bureaucrat’s actual starting cohort size with its lagged value, that is the value it would have taken had the bureaucrat started his term one year earlier.

We identify the effect of competition incentives off of the remaining variation in starting cohort size after controlling location and time fixed effects. The way we interpret this variation is that of a residual and unpredictable variation in the number of bureaucrats that exit each province each year. This is in part driven by the age and the seniority of bureaucrats. We interpret it as the cumulative result of idiosyncratic shocks to the careers of the bureaucrats that we aggregate at the provincial level. In the following sections we show evidence that it is not driven by location-specific pre-trends, nor by individual characteristics, nor by other potential confounding factors.

Having established that the number of competitors matters for a secretary’s probability to be promoted, we examine the impact of competition on the behaviour of CCP secretaries. We take a reduced-form approach and test the relationship between the number of competitors of a CCP secretary, and various measures of his political efforts. As we observe a secretary’s number of competitors at the beginning of his term, we estimate the following equation:

$$Y_{c,t} = \beta Z_{c,t} + \alpha_c + \delta_t + \epsilon_{c,t} \tag{1.2}$$

where $Y_{c,t}$ denotes an outcome in prefecture c in year t , $Z_{c,t}$ is, as before, the number of prefecture party secretaries in the same province as a given party secretary of prefecture c , who start their first year of term in the same year as said party secretary, and α_c and δ_t are prefecture and year fixed effects, respectively. Note that, as before, $Z_{c,t}$ is constant for all the years of a given CCP secretary's term in a given prefecture.²⁴ Similarly as in 1.4, we cluster $\epsilon_{c,t}$ in equation 1.2 at the province - start year level, because $Z_{c,t}$ is still symmetric for all prefecture party secretaries who start their term in the same year in the same province.

There are two features of this model that are important to highlight. The first is that the effect of β is assumed to be invariant throughout the years of a CCP secretary's term. We assume that the number of competitors at entry is immediately observable to a given secretary and yields information on the competitiveness of his particular contest, and that this information shifts his behaviour throughout his term. We can relax this assumption and let β vary over time, that is, with the years of a secretary's term. This would capture the fact that competitive pressure may have different effects on behaviour depending on how advanced a secretary is in his term.²⁵ We test these possibilities in Section 1.6 by letting β vary with the time elapsed since the beginning of one's term and show that our baseline specification does not seem to mask significant within-term dynamics.

A second feature of Equation (1.2) is that longer terms implicitly carry more weight in the estimation, as more observations of the sample belong to them than to shorter terms. One test of the robustness of our results to this feature is to collapse our data at the level of terms, rather than using a panel of prefectures and years, whereby we give equal weights to all terms. We test this alternative specification in Section 1.6. This does not allow for the inclusion of calendar year fixed effects, which may be important if national

²⁴If a CCP secretary executes two terms in two different prefectures, $Z_{c,t}$ takes two values, each corresponding to the starting cohort in each prefecture.

²⁵How to relax this assumption is not trivial, as the length of a secretary's term is not fixed. β could be a function of the time elapsed since the beginning of one's term, or of the time remaining in one's term; and it is not obvious that the dynamics should be linear. In order to exhibit a political "cycle" with endogenous cycle length, one would ideally instrument for the a CCP secretary's most probable date of exit.

variations in these outcomes are important. Another way of controlling for these unequal contributions of different terms to our estimations is to inversely weight observations by the number of years that a term is observed for (that is, the length of the term). We check the sensitivity of our estimations to this reweighting procedure in Section 1.6.

In order to leverage all the information available in the CHARLS survey, we also estimate a modified equation at the respondent-level, in order to assess whether promotion incentives cause party secretaries to seize farmers' land in their prefecture to a greater or lesser extent:

$$E_{i,c,t} = \beta Z_{c,t} + \mathbf{X}'_{i,t} \gamma + \alpha_c + \delta_t + \mu_{i,c,t} \quad (1.3)$$

where $E_{i,c,t}$ is a dummy variable that equals to 1 if individual i was expropriated in prefecture c in year t . $\mathbf{X}'_{i,t}$ are controls for a range time-varying and time-invariant individual characteristics, such as age, age squared, gender, ethnicity and a full set of educational attainment dummies.²⁶ All other notation is as before and standard errors are clustered at the province-start year level.

1.5 Results

1.5.1 Promotions and the number of competitors

Our first set of results relate to the impact of our measure of the size of the competitive field, that is the size of the starting year cohort, on the promotion likelihood of CCP secretaries (Table 1.A.5). An increase by 1 standard deviation (or 2.4) in the number of competitors in a CCP secretary's cohort decreases his likelihood of promotion by 6.5 percentage points (or 13 percent of a standard deviation), relative to a mean probability of promotion of 48 percent. This effect is statistically significant at the 1 percent level.

²⁶In our most demanding specification, we add individual level fixed effects.

The age at which a secretary takes office also has a significant and substantial negative effect on his probability to be promoted. This appears consistent with anecdotes that well-connected bureaucrats (for example, heirs of CCP leaders or “Princelings”) systematically take office at a young age and are quickly promoted. Age at entry is not correlated with the number of competitors at the beginning of one’s term (Figure 1.B.5). We include this variable in all our subsequent regression to increase precision.

A broader threat for our identification is the potential correlation between the variable we choose as a measure of promotion incentives, and characteristics of CCP secretaries, such as age, education, ethnicity or gender, which may impact both their career path and their policy-taking during their term. Unfortunately, the data we are able to collect on education, gender and ethnicity of CCP secretaries is only partial, so that we cannot correct for these characteristics in all estimating equations, for fear of losing too much statistical power and of selecting even more our working sample. We address this concern by showing, in the data we do have, the correlation between the size of the starting cohort and CCP secretary characteristics (Table 1.A.6). Reassuringly, these variables are not statistically significantly correlated with our measure of competition. This is true of the full sample of data on CCP secretary characteristics, and to its restriction to our final estimating sample (for which all outcome variables are available).

The variation in starting cohort size can be interpreted as the provincial-level aggregated residual unexplained shocks that affect the careers of a bureaucrat’s predecessors. We show in Table 1.A.27 that the average age of prefecture-level secretaries in the province in the year prior to office-taking positively correlates with one’s cohort size, but that this effect not driven by whether these predecessors have been promoted or went into retirement. As a further robustness check, we instrument in the next section the size of one’s starting cohort by the average age of prefecture-level bureaucrats in the province in the year preceding office-taking. We show in Table 1.A.30 that the initial structure of terms in 1996 explains part of the variation in cohort size. We do this by simulating what cohort sizes would be if we fixed the seniority structure in 1996 and let each secretary have terms

of 5 years of length, and so on until 2014. Columns (1) to (3) show that there is a positive correlation with actual cohort size, but that this effect fades over time, as some shocks affect each generation of bureaucrat's term lengths. Unfortunately the weakness of the theoretical cohort size's power to explain actual cohort size does not allow us to perform the same type of reduced form exercise as with the predecessors' average age.

We interpret these results as evidence that the hierarchical structure of the CCP generates tournament-like incentives. CCP prefecture-level secretaries compete for higher-ranked seats against their competitors, who are -mostly- the neighbouring CCP secretaries in their provincial cohort. The number of seats available for promotion is limited, so that the presence of more competitors decreases each bureaucrat's chances to get promoted. This decreases the expected payoff from the effort that a CCP secretary is willing to incur in order to secure a seat. We therefore expect an increase in the number of competitors to decrease effort, as predicted by the formal model in Appendix 1.C. As the promotion probability of a CCP secretary exogenously increases as a consequence of fewer competitors, increased effort increases measures of performance relevant for promotion.

1.5.2 Official economic performance measures

In Table 1.A.7, we show that the size of one's cohort has a negative impact on GDP growth. An increase by 2.4 competitors, or 1 standard deviation, increase in one's probability of promotion, decreases nominal GDP growth by 0.8 percentage points on average (5 percent of the sample average of 15.8 percent), or 9 percent of a standard deviation. This effect is significant at the 1 percent level, even when adjusting for multiple hypothesis testing. We find a similar result for reported real GDP growth: a 1 standard deviation increase in the number of competitors of a CCP secretary is associated with a 0.3 percentage point decrease in real GDP growth, or 2.5 percent of the mean growth rate of 12.7 percent. We disentangle this result across sectors in columns 4, 5 and 6. Most of the aggregate effect on GDP growth is driven by the secondary sector, where the impact of promotion probability is both the largest and the most precisely estimated: a 1 standard deviation increase in

the number of competitors of a CCP secretary increases growth of the secondary sector GDP by 1.2 percentage points, or 7 percent of the sample average of 18.3 percent.

We next turn to employment. A priori, employment figures are less politically sensitive than GDP figures. The fact that employment growth is also positively correlated with exogenous increases in promotion probability (Table 1.A.8) suggests that our results on GDP growth are not entirely the product of data manipulation. The magnitude of the effects on total employment growth (column 1), and employment growth by sector (columns 1 to 3) are large. The effect of an increase in the number of competitors by 1 standard deviation is a 1.2 percentage points increase in growth of employment in the primary sector, which is large.

We further explore the characteristics of the additional economic growth that is associated with promotion incentives. The Chinese economic growth model has been characterized by high levels of investment, notably in infrastructure and in construction. We therefore investigate whether measures of the prevalence of the construction sector in the economy are positively correlated with promotion probability. Table 1.A.9 displays our results. We find that the ratio of real estate investment to GDP is on average higher in prefectures and years where the CCP secretary in office has fewer competitors. An exogenous decrease by 1 standard deviation in a CCP secretary's cohort size, which increases promotion likelihood, also increases the real estate investment to GDP ratio of the prefecture under his authority by 0.3 percentage points, or 5 percent relative to a 6.6 percent sample average. This is in large part due to investment in residential real estate, which increases by 0.2 percentage points of GDP (or 3.5 percent). We also find a large and positive, but not statistically significant at conventional levels, effect on the growth of employment in the construction sector.

These results are based on official data from the National Bureau of Statistics, that are compiled by local branches of the national-level statistical office. Although they converge and support the idea that economic performance strongly reacts to promotion incentives, they could also reflect incentives that local bureaucrats face in reporting favourable statis-

tics, regardless of their actual performance. We address data manipulation explicitly in Section 1.6, but find no evidence of substantial manipulation, although we cannot exclude that some manipulation of GDP figures could be driven by the intensity of within-cohort competition. We therefore turn to external data in order to corroborate our findings.

1.5.3 Land expropriations and remote sensing data

Using remote sensing data, we find that the percentile rank of nightlight growth of a prefecture within a province is positively associated with a smaller number of competitors (Table 1.A.7, column 3). This relationship is, however, only marginally statistically significant at conventional levels, which could be due to the degree of imprecision that this approximation typically shows (see Appendix 1.E).

We compute average expropriation rates at the prefecture-year level based on the CHARLS survey data. Despite a substantial decrease in sample size, we find that an exogenous increase in the number of competitors of a CCP secretary decreases the expropriation rate under a CCP secretary's term (Table 1.A.9, column 4). This effect is large: a 1 standard deviation increase in a CCP secretary's starting cohort size induces a 0.1 percentage points (or 15 percent) increase in expropriations.

Table 1.A.10 displays individual-level regressions, where the outcome variable is a dummy variable taking value 1 when the CHARLS respondent declared having been expropriated in a given prefecture in a given year. Individual data allows us to control for time-invariant individual characteristics that are likely correlated with the probability of being expropriated. Across all specifications, our results show that smaller cohort size, that is associated with higher exogenous promotion probability of a CCP secretary, is also associated with a higher probability of being expropriated for individuals who live in said CCP secretary's prefecture during his term.²⁷ The magnitude of the effect of a 1 standard deviation increase in cohort size of a CCP secretary ranges from a 0.09 percentage points

²⁷We use the prefecture of the respondent's registration, or hukou, as it is this registration that determines land use rights.

(12 percent) to a 0.11 percentage points (14 percent) increase in the yearly expropriation probability of a CHARLS respondent. These effects are statistically significant at the 5 percent level.

Finally, we turn to the growth rate of the urban areas of prefectures. If economic growth at the local level is spurred by investments in construction and real estate, and is supported by rural land expropriations, the urban areas of prefectures where a CCP secretary has fewer competitors should be growing. Because of the noisiness of absolute growth rates, we use provincial decile ranks of urban area growth rates. In column 5 of Table 1.A.9, we show that promotion probability positively impacts the growth rate of urban areas.

1.5.4 Public good provision

We next ask whether this emphasis on real-estate, construction-driven growth happens at the cost of other policies. We examine whether CCP secretaries, in their policy choices, substitute between construction investment and public goods provision. The intuition we rely upon here is that (i) public good provision does not directly play a role in a CCP secretary's future career advancement and (ii) CCP secretaries have limited resources, so that they may face policy trade-offs.

Table 1.A.11 displays our results. We look at the causal impact of the size of the starting cohort on the provision of health services (as measured by the number of hospital beds per capita and the number of doctors per capita), transport services (bus per capita) and education (primary school teachers per capita and middle school teachers per capita), while controlling for confounding factors such as population size and student body size. While all coefficients are positive and relatively large in magnitude, none are statistically significant at conventional levels. We normalize these 5 measures and aggregate them into an average z-score.²⁸ The effect of the size of the competitor pool is positive and

²⁸Each variable is normalized by subtracting the yearly mean of the variable from each observation and dividing by its yearly standard deviation. The aggregate z-score is the average of the 5 normalized indices.

significant at the 5 percent level. An increase by 1 standard deviation in the number of competitors at the start of a CCP secretary's term increases this measure of public good provision by 1.5 percent of a standard deviation.

We interpret this as suggestive evidence that some substitution might be at play. The presence of many competitors decreases each secretary's chance of being promoted, and thereby dilutes the incentives to provide effort towards promotion, via highly visible investment- and real estate-driven growth. Because of this, local officials may decide to foster public good provision, for instance if they derive utility from their prefecture's overall welfare, in addition to their personal promotion chances.

1.5.5 Land expropriations and individual-level, secretary-level, and prefecture-level outcomes

A last contribution of our analysis is to shed light on the potential implications of such construction-driven strategy. We address this in three steps.

First, we use the CHARLS survey to analyze the consequences of expropriations on individual's life histories and trajectories (Table 1.A.12). The CHARLS survey has rich data on the entire life history of respondents. The fact of having ever been expropriated is negatively correlated with the probability to have ever changed residence: having ever been expropriated is associated with a 3 percentage points (8 percent) lower probability to have changed residence at least once. It is also associated with greater incidence of adverse health outcomes, such as having suffered injuries (2 percentage points, or 22 percent of the mean value) or being hospitalized (2.8 percentage points, or 22 percent of the mean value); lower access to rural collectives or local state-owned enterprises, lower access to pensions, and fewer assets, such as heating. These relationships are robust to the inclusion of time-invariant individual controls and location specific fixed effects.

Second, we look at the correlation between a CCP secretary's history of expropriations, and his probability of facing adverse outcomes in the rest of his career. We regress the probability that a CCP secretary is caught in an anti-corruption campaign on the average

expropriation rate that is observed during his term. This is indicative of the fact that local officials who engage in expropriations are often met with political repression, and echoes evidence by Chen and Kung (2018) on the ties between the land market activities of local officials and their corruption.

Third, we examine the potential implications of the urbanization that is spurred by bureaucratic incentives. Promotion-incentives-driven booms in construction might be the symptom of an urbanization process that is, at least partly, “a competitive enterprise for local governments who seek to outdo each other in ‘place-making’, both to attract investments and to conjure up highly visible trophy projects” (Yew, 2012). We display in Table 1.A.14 the correlation between a prefecture’s average expropriation rate throughout the period and the probability that it -or part of it- is categorized as a ghost city by various sources listed in Jin et al. (2017). Our results show a positive correlation between the two variables. This is suggestive that some of the extra development induced by promotion incentives could lead to inefficient real estate development, in as much as high vacancy rates are evidence of resource misallocation.

1.6 Robustness checks

1.6.1 Sensitivity to outliers

The results we find on raw figures reported in statistical yearbooks are large, especially those regarding employment. We therefore trim our data to exclude the top and bottom outlying values of each variable. Namely, we set to missing all the values that satisfy:

$$\text{abs}\left(\frac{Y_{it} - \bar{Y}_t}{\sigma_{Y_t}}\right) > 2.15$$

where \bar{Y}_t is the yearly average of Y and σ_{Y_t} its year-specific standard deviation.

We then replicate our analysis on all our outcome variables, using the same specifications. Table 1.A.15 shows that our results on GDP growth rates are all statistically

indistinguishable from the ones obtained on non-winsorized variables; the effect on nominal and real GDP growth rate, and the growth rate of GDP in the secondary sector, remain statistically significant at the 5 percent level. However, Table 1.A.15 shows that results on employment figures are not so robust.

1.6.2 Sensitivity to CHARLS sample definition

The CHARLS survey is retrospective: individuals are surveyed in 2014 and answer about their life history. This means that the estimation of expropriation rates in some prefectures and years is based on few individuals, if those have migrated prior to be located in the prefecture where they are surveyed in 2014.

In order to test the robustness of our results to sample selection, we vary in Table 1.A.16 the scope of our sample to either include all individuals and years where we have information (column 3) or only prefectures and years where at least 20 individuals are reported to reside (column 1), or 50 (column 2). This does not affect the sign, the magnitude nor the statistical significance of our results.

1.6.3 Term level specification

An alternative specification for estimating the effect of a CCP secretary's promotion probability on his behaviour during his term, is to collapse our data at the term level. The information on promotion that we exploit is indeed constant at the level of a term. By averaging data at the level of terms that have different lengths, we are also testing the robustness of our results to the implicit sets of weights, that the prefecture-year panel data on the one hand and the term-level data on the other hand attribute to each term.

We collapse the various outcomes that we test in the previous section at the term level and reproduce our results. We estimate the following relationship between starting cohort size and promotion likelihood:

$$P_m = \nu_m + \mu_{s_0} + \xi Z_{m,s^0} + \eta_m \quad (1.4)$$

where ν_m and μ_{s_0} are fixed effects for the prefecture in which term m takes place, and the starting year s_0 of term m . Our main specification on the effect of competition on policy outcomes becomes:

$$Y_m = \mu_m + \tau_{s_0} + \pi Z_{m,s^0} + \zeta_m \quad (1.5)$$

We display our results in Table 1.A.17. Overall, the results are in line with those obtained with our main specification using the panel of prefectures. Effects on nominal GDP growth and GDP growth in the secondary sector, on the share of real estate investment in GDP, on the growth of urban areas, are of similar magnitude and statistical significance. Some effects lose statistical precision, such as real GDP growth; but the magnitude of these effects is indistinguishable from those obtained in the panel, with a sample that is by definition much smaller in size.

Collapsing the data at the term level by taking the average of our outcomes over the years of each term does not allow us to control for annual variations in those outcomes. As an alternative robustness check to our main result from Section 1.5, we also estimate Equation (1.2) using the inverse of the length of a term as weights for each observation, to counterbalance the fact that longer terms carry more weight in our estimation than shorter ones. We display our results in Table 1.A.18. Overall, we find that the magnitude, sign and significance of our results in Tables 1.A.7 to 1.A.11 are largely unchanged by this modification.

1.6.4 Predecessors' average age as an instrument for starting cohort size

A robustness check that can alleviate part of the concerns as to where the identifying variation in starting cohort size comes from, is to use the average age of predecessors as an independent variable instead. For each term we compute the average age of prefecture-level secretaries in the same province the year before the term starts, and use this as a regressor rather than one's starting cohort size. The idea behind this is that mechanical factors, such as seniority rules, are driving the starting cohort size and these cannot be manipulated by each individual bureaucrat. We display the results of our strategy in Table 1.A.28.

The results are broadly in line with our main results, notably with regards to nominal and real GDP growth, and expropriation rates. However some coefficients' magnitude and some signs are not aligned with our main results. The fact that the precision of all estimates is lower indicates that other factors affect the size of a cohort, and that the statistical power of the predecessors' average age is therefore likely to be low. We prefer to use the actual starting cohort size in our main results, since it is a more precise measure of the competitive incentives that bureaucrats face.

1.6.5 CCP secretary characteristics

One important threat to our identification strategy is the validity of the exogeneity assumption. Local bureaucrats with power and knowledge of the internal structure of the CCP could in theory influence where and when they are nominated as prefecture-level secretaries, with the aim of locating in "high-profile" prefectures.

As a robustness check, we include in our main specifications (see Eq. 1.1) additional CCP secretary characteristics, such as education, ethnicity and local origin.²⁹ This considerably reduces the size of our sample, from 306 to 260 clusters and from 2577 to 1826

²⁹Local origin has been discussed for instance as having positive impacts on the probability of receiving patronage returns from higher-ranked bureaucrats.

observations. Yet, all of our results are robust in magnitude and sign to this new specification, and most are robust in statistical significance (see Table 1.A.19).

1.6.6 Pre-trends of the outcome variables and placebo tests

We verify whether the size of the starting cohort correlates with the pre-trends the outcome variables that we measure, and with the promotion likelihood of bureaucrats. This would happen for instance if bureaucrats with higher expected promotion likelihood were nominated, at the prefectural level, in prefectures where and in years when greater performance has just been observed (which would correlate with their predecessors' promotion probability).

In order to alleviate this concern, we check whether the size of a bureaucrat's starting cohort is correlated with the pre-trends of our outcome variables, as measured by the average of the outcome variables in the two years preceding a bureaucrat's term. Table 1.A.26 displays our results. Reassuringly, the coefficients of the starting cohort size do not display any statistically or economically significant pattern.

We also perform a placebo test, replacing the value of the cohort size by the value it would have taken had the bureaucrat taken office one year earlier. We present the results in Table 1.A.32. The coefficient of the placebo cohort size is never precisely estimated and does not show a pattern that matches our main results.

1.6.7 Inclusion of the pre-term provincial level promotion probability

A similar concern to that addressed in the previous subsection would be that a larger starting cohort indicates predecessors with a higher promotion probability. The negative relationship between cohort size and promotion probability could actually reflect the dynamics of promotion rates rather than the impact of changes in the cohort size.

In order to address this concern, we include in our main specifications the promotion

rate of the bureaucrat’s predecessors. As shown in Table 1.A.29, this does not alter significantly the magnitude or the precision of our estimates. As it does reduce the sample size (because of the availability of promotion data on predecessors), we do not include this variable in our main specifications.

1.6.8 Time-varying effect of the number of competitors

Equation 1.2 assumes that the effect of the number of competitors in a cohort is homogeneous for all the years of a given term. In other words, the competitiveness of the competitive field for promotion shifts the level of a CCP secretary’s outcomes.

We test in this section whether the effect of the number of competitors varies with the number of years that a CCP secretary has stayed in office. We do this by letting the coefficient of the number of competitors in Eq. (1.2) vary with the years elapsed since the bureaucrat has taken office. Tables 1.A.20, 1.A.21, 1.A.22 and 1.A.23 present our results. Overall, most of the interaction terms of the variable of interest (the cohort size) with the dummy variables for the time elapsed since the beginning of a secretary’s term show a statistically imprecisely estimated effect. For most of our outcome variables, the only significant interaction terms are those with the last years of the longest terms in our sample, beyond the 6th year. Interpreting these coefficients is difficult, because it is likely that these interaction terms actually capture the fact that these long terms are very specific with regards to the majority of the terms in our sample (see Figure 1.B.8 for a distribution of term lengths). For instance, longer terms could signal a set of characteristics that lower promotion likelihood, thus attenuating any variation in incentives due to cohort size.

1.6.9 Data manipulation

Measuring data manipulation is, by definition, challenging. We use tests for the presence of decimal digits in reported GDP growth rates, as suggested in Ji (2019). The rationale for this strategy is based on Benford’s law, also named ‘first digit law’, which states that

“the frequency distribution of first digits in many—but not all—real world data is not distributed uniformly, but according to the widths of gridlines on a logarithmic scale” (Holz, 2014). This also applies to further digits following the first one (Smith, 1997).

We first test for the presence of first decimal and second decimal digits, with the idea that the presence of more or less detailed information may be a signal of manipulation. Table 1.A.24 displays our results. A smaller cohort size seems to increase the likelihood that the reported real GDP growth rate is reported with two decimal digits of precision; but this coefficient is imprecisely estimated.

Overall, the distribution of second decimal digit does not seem to follow Benford’s law (Figure 1.B.7), as was previously noted by Ji (2019). This in itself is not necessarily proof that the underlying data are manipulated.³⁰ However, we show that the absence or presence of some digits is associated with the size of a CCP secretary’s cohort (Table 1.A.25). A larger size of a secretary’s cohort decreases the likelihood that the real GDP growth rate ends with a 2 or 5, and increases the probability that it ends with an 8, and these effects are statistically significant at the 5 percent level: the distribution of decimal digits differs more from Benford’s law when the number of competitors of a CCP secretary is small. This however should be taken with caution as the testing of multiple hypotheses at once is likely to make some tests incorrectly reject the null hypothesis.

We do not treat this as evidence that the impacts we identify in terms of economic performance and policy choices are the pure results of manipulation. The fact that a body of evidence from several data sources converges to support our theory of a construction-led growth suggests the presence of real effects. It is possible however that *in addition* to real effects, promotion incentives may also increase official data manipulation at the local level.

³⁰Rounding errors can for instance explain the over-representation of zeros and fives

1.7 Conclusion

In this paper, we investigate the relationship between promotion incentives, economic performance and expropriation practices by prefecture-level CCP secretaries in China in the past 25 years. We use variations in the competitiveness of promotions based on the structure of the competition local officials will face in their immediate future, and find that more competitors reduce the probability of being promoted to higher offices at the end of one's term. Smaller cohorts of competitors increase promotion likelihood, and causally impact economic performance, increasing GDP growth, real estate investment, expropriations and growth of the urban area of a prefecture. We also find some suggestive evidence that the provision of public goods may be negatively impacted by such an increase in promotion probability. Finally, we document that expropriations are linked to negative outcomes for the individuals who face them, for the local officials who engage in them, and for the cities where they take place.

We contribute to understanding the performance and policy incentives of a competitive bureaucracy like that of China. Much like in a tournament, Chinese bureaucrats see their chances of promotion increase when there is less competition. This increases the marginal return of effort in dimensions that matter for their evaluation. This drives these local officials to rely on a model of economic growth driven by real estate investment and expropriations, at potentially high social cost.

1.A Tables

Table 1.A.1 – Term-level descriptive statistics

Variables	Mean	Std. deviation	Min	Max
Length of term	2.635	1.778	0	10
Age at entry	49.75	4.231	30	60
Size of starting cohort	4.072	2.524	0	14
Promotion (official rank)	0.526	0.500	0	1
Promotion (actual)	0.487	0.500	0	1
Retirement	0.263	0.440	0	1
Anti-corruption campaign arrest	0.126	0.332	0	1
Data availability				
Age data available	0.946			
Competitor's data available	0.981			
Promotion data available	0.732			
All data available	0.726			

The statistics in this table are computed on the 1310 terms of prefecture-level CCP secretaries that start no earlier than 1996 and end in 2014 or before. The length of one's term is equal to 0 if the secretary is observed in office once at the end of a year, 1 if he is observed in office at the end of two consecutive years, etc. Officially, terms are 5 years long, but this rule is little enforced, so that we concatenate consecutive 'official' terms if any. The age at entry of a secretary is the age of the secretary when he takes office. The variable 'Competitors data available' is a dummy variable equal to 1 if data on the seniority and age of all other secretaries in the province at the time a given secretary takes office is available. Promotion data is based on the positions that a given secretary occupies following his term. We consider all positions for which the starting date is between the last year at the end of which a secretary is in office and two years after. For most individuals this corresponds to a duration of less than one year, and this is by definition strictly less than 2 years. Promotion is officially defined by ranks in the hierarchical structure of the CCP. Some of these promotions are to 'retirement positions'. Some secretaries are nominated to several positions, which explains why the retirement rate and actual promotion rate do not add up to the total promotion rate. The size of the starting cohort corresponds to the number of competitors of a given secretary, ie: the number of secretaries in the same province who start their term in the same year as a given secretary.

Table 1.A.2 – Province-level descriptive statistics of careers

Variables	Average term length	Average (actual) promotion probability	Average age at entry	Average number of competitors
Hebei	2.05	0.44	51.2	3.6
Shanxi	1.90	0.49	51.6	4.4
Inner Mongolia	2.22	0.67	50.5	3.8
Liaoning	2.33	0.45	50.8	5.1
Jilin	2.30	0.56	50.0	2.7
Heilongjiang	2.40	0.30	50.3	4.2
Jiangsu	3.05	0.67	49.6	3.2
Zhejiang	2.67	0.57	49.1	3.1
Anhui	3.11	0.41	48.3	4.7
Fujian	2.87	0.63	49.9	2.3
Jiangxi	2.33	0.49	49.5	2.8
Shandong	3.27	0.58	49.5	6.3
Henan	2.73	0.56	50.6	5.7
Hubei	2.47	0.56	49.8	3.8
Hunan	2.90	0.44	48.8	3.7
Guangdong	2.57	0.46	50.3	5.6
Guangxi	2.35	0.41	48.3	4.6
Hainan	2.57	1	52	0.3
Sichuan	2.67	0.22	49.1	5.8
Guizhou	3	0.67	48.2	2.9
Yunnan	2.59	0.44	47.7	4.1
Xizang (Tibet)	3.91	0.73	44.9	1.9
Shaanxi	2.47	0.41	50.3	2.7
Gansu	2.32	0.39	48.6	4.9
Qinghai	2.86	0.50	46.4	1.6
Ningxia	3.58	0.75	48.9	1.6
Xinjiang	3.05	0.25	49.2	3.3
Total	2.62	0.49	49.6	4.1
Observations	951			

This table is based on 951 terms of prefecture-level CCP secretaries who start no earlier than 1996 and end before 2014 and for whom all data are available. We exclude province-level municipalities (Beijing, Tianjin, Shanghai and Chongqing) from our sample. Length of term is calculated as end year minus start year, i.e., if a party secretary is in office for two consecutive years in a given prefecture (observed at the end of each year), his term length is 1. This definition implies that we may concatenate consecutive terms, as we do not observe the start date of any second terms. Age of entry is the age of the party secretary when he takes office. Size of starting cohort are the number of prefecture party secretaries in the same province who start their term in the same year as a given party secretary. Anti-corruption campaign arrest is a dummy variable that takes the value of 1 if a party secretary was investigated during the anti-corruption campaign beginning in 2012. Promotion data are based on the positions that a party secretary occupies following his term. We consider all positions for which the start date is between the last year at the end of which a secretary is observed in office and two years afterwards. For most individuals this corresponds to a duration of less than one year and this is by definition strictly less than 2 years. Promotion (actual) is a dummy variable that equals 1 if a party secretary attains a position with an officially higher rank in the CCP hierarchy that is not a retirement position after the end of his term.

Table 1.A.3 – Summary statistics - macroeconomics variables

Variables	Mean	Std. Deviation	Min	Max
Population (in millions)	4.158	2.334	0.160	12.50
Provincial capita	0.0903	0.287	0	1
Total prefecture area (thousands of km ²)	22.96	45.87	0.978	496.3
Nominal GDP (billion RMB)	75.85	110.5	0.702	1542.0
Nominal GDP growth	0.159	0.0964	-0.736	1.462
Real GDP growth	0.135	0.233	-0.488	7.908
Nominal GDP growth (Primary sector)	0.145	2.187	-0.638	125.0
Nominal GDP growth (Secondary sector)	0.188	0.206	-0.761	6.068
Nominal GDP growth (Tertiary sector)	0.172	0.270	-0.885	11.20
Employment (thousands)	440.4	508.4	40.50	5852.9
Employment growth	0.00200	0.532	-0.957	26.06
Employment share (Primary sector)	0.0524	0.102	0.01	0.740
Employment share (Secondary sector)	0.425	0.137	0.0446	0.844
Employment share (Tertiary sector)	0.523	0.134	0.0991	0.948
Employment growth (Construction sector)	1.022	4.368	-1	91.21
Real estate investment (% GDP)	6.352	5.967	0	91.22
Residential RE investment (% GDP)	5.189	4.353	0.00887	61.50
Expropriation rate	0.00711	0.0430	0	1
National rank in urban area growth	177.2	101.9	1	334
Number of hospital beds	11444.3	7952.1	860	66721
Number of doctors	6473.3	4802.0	377	63193
Number of buses per capita	6.402	6.519	0	115
Number of teachers (primary school)	17756.6	10187.0	595	57887
Number of teachers (middle school)	15062.0	8634.5	400	46287
Number of teachers (higher education)	3095.6	6135.0	0	55416
Observations	2899			

Data included in this table are from the National Bureau of Statistics City Statistical Yearbooks, except for the expropriation rate, which we compute from the CHARLS survey, and the total prefecture area, which is measured using satellite data from the DMSP/OLS. The statistics are computed on our main estimation sample, the 2899 prefecture-year observations where macroeconomic data and CCP secretary characteristics are available.

Table 1.A.4 – Outcome variables - Balance tests

Variables	$\mathbb{1}\{\text{Promotion data available}\}$	
	OLS Coefficient	Std. Error
Population (in millions)	0.0409	0.0254
Provincial capita	-1.54e-18	1.87e-13
Total prefecture area (thousands of km ²)	1.34e-15	2.80e-10
Nominal GDP growth	0.00969	0.00518
Real GDP growth	0.0204*	0.00855
Nominal GDP growth (Primary sector)	0.0817	0.0562
Nominal GDP growth (Secondary sector)	0.0159	0.0115
Nominal GDP growth (Tertiary sector)	0.0129	0.0122
Employment (thousands)	-45.11	25.02
Employment growth	-0.0332	0.0280
Employment share (Primary sector)	-0.00906	0.00599
Employment share (Secondary sector)	-0.00718	0.00524
Employment share (Tertiary sector)	0.0164**	0.00506
Employment growth (Construction sector)	0.0410	0.138
Real estate investment (% GDP)	0.174	0.176
Residential RE investment (% GDP)	0.126	0.184
Expropriation rate	0.00286	0.00192
National rank in urban area growth	3.660	3.332
Number of hospital beds	-287.2	149.2
Number of doctors	-116.1	104.7
Number of buses per capita	-0.154	0.171
Number of teachers (primary school)	333.1	221.9
Number of teachers (middle school)	94.16	183.0

Each row in this table corresponds to the OLS regression of the left-hand side variable on a set of prefecture-level and year-level fixed effects, and a dummy for whether CCP secretary data are available. We report the point estimate of the dummy variable and its robust standard error. Data included in this table are from the National Bureau of Statistics City Statistical Yearbooks, except for the expropriation rate, which we compute from the CHARLS survey, and total prefecture area, which is measured using satellite data from the DMSP/OLS. The statistics are computed on our main sample, the 2899 observations where macroeconomic data and CCP secretary characteristics are available. Standard errors are clustered at the term-level, with significance with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively.

Table 1.A.5 – Promotion probability and number of competitors (prefecture panel)

	Promotion likelihood
Size of starting cohort (Normalized)	-0.0647*** (0.0188)
Age at entry	-0.0353*** (0.00448)
Nb. of obs.	2680
Nb. of clusters	306
Mean of dep. var.	0.48

Observations are at the prefecture-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. The dependent variable is a dummy variable for whether the CCP party secretary is promoted at the end of his term. The regressor of interest is the size of the starting cohort, i.e., the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as a given prefectural CCP secretary. Controls include the CCP secretary's age in the year he takes office (reported in the table), as well as prefecture and calendar year fixed effects. Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively.

Table 1.A.6 – Instrument correlation with individual CCP secretary characteristics

Variables	Ethnicity	Origin		Education				
	Ethnic minority	Native of the province	Top university (A) graduate	Top university (B) graduate	Top specialist university graduate	Party school graduate	Teaching college graduate	
Panel A: all available data								
Correlation with instrument	-0.195* (0.113)	-0.0156 (0.0708)	-0.0959 (0.0873)	-0.0304 (0.177)	-0.102 (0.132)	0.00696 (0.0755)	-0.130 (0.118)	
Nb. of obs.	1008	980	733 315	733 315	733 315	733 315	733 315	
Nb. of clusters	370	357						
Panel B: Estimation sample								
Correlation with instrument	-0.329 (0.221)	0.0266 (0.104)	-0.238* (0.130)	-0.0887 (0.237)	-0.00873 (0.144)	-0.0946 (0.101)	-0.125 (0.176)	
Nb. of obs.	575	549	431 212	431 212	431 212	431 212	431 212	
Nb. of clusters	244	235						

Observations are at the term level. The sample only includes terms that start at the earliest in 1996 and finish before 2014. Panel A includes all terms for which data on the prefecture party secretary's characteristics and the size of his starting cohort is available. Panel B restricts the sample further to our main estimation sample, for which we observe the macroeconomic variables of interest. Each column is an OLS regression of the instrument (size of starting cohort, i.e., number of other prefecture-level CCP secretaries in the province who start their terms in the same year as a prefectural CCP secretary) on said party secretary's individual characteristics. Ethnic minority is a dummy variable for non-Han ethnicities. Native of the province is a dummy variable that takes the value of 1 if the prefecture party secretary was born in the same province. The prefecture party secretary's education level is characterised by a set of dummy variables for his reported alma mater. Top universities are as those named as "Double Firsts" by the Chinese Ministry of education in 2015. Controls include the CCP secretary's age in the year he takes office, as well as prefecture and start-year fixed effects. Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively.

Table 1.A.7 – GDP growth and promotion incentives

Variables	GDP growth (nominal)	GDP growth (real)	GDP growth rank	GDP growth Primary sector (nominal)	GDP growth Secondary sector (nominal)	GDP growth Tertiary sector (nominal)
Source	NBS (1)	NBS (2)	Satellite (3)	NBS (4)	NBS (5)	NBS (6)
Size of starting cohort (Normalized)	-0.00835*** (0.00244) [0.004]	-0.00333*** (0.00122) [0.040]	-0.0923* (0.0548) [0.560]	-0.00552* (0.00303) [0.419]	-0.0119*** (0.00411) [0.024]	-0.00485** (0.00239) [0.262]
Age at entry	0.000535 (0.000453) [1.000]	0.000235 (0.000227) [1.000]	-0.00792 (0.0147) [1.000]	0.000976 (0.000745) [1.000]	0.000441 (0.000776) [1.000]	0.000359 (0.000501) [1.000]
Nb. obs	2678	2677	2678	2678	2678	2678
Nb. clusters	306	306	306	306	306	306
Mean of dep. var.	0.158	0.127	5.160	0.0996	0.183	0.161

Observations are at the prefecture-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. Controls include the CCP secretary's age in the year he takes office (reported in the table), as well as prefecture and calendar year fixed effects. Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively. P-values adjusted for family-wise multiple hypothesis testing are reported in square brackets.

Table 1.A.8 – Employment growth and promotion incentives

Variables	Employment growth (Total)	Employment growth (Primary sec.)	Employment growth (Secondary sec.)	Employment growth (Tertiary sec.)
Source	NBS (1)	NBS (2)	NBS (3)	NBS (4)
Size of starting cohort (Normalized)	-0.0118** (0.00470) [0.049]	-0.0697** (0.0310) [0.101]	-0.0126*** (0.00453) [0.023]	-0.00874** (0.00413) [0.140]
Age at entry	-0.00262 (0.00190) [0.671]	-0.0162 (0.0323) [1.000]	-0.00196* (0.00112) [0.328]	-0.00185 (0.00159) [0.981]
Nb. obs	2678	2671	2678	2678
Nb. clusters	306	306	306	306
Mean of dep. var.	0.00227	0.0436	0.0122	0.00159

Observations are at the prefecture-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. Controls include the CCP secretary's age in the year he takes office (reported in the table), as well as prefecture and calendar year fixed effects. Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively. P-values adjusted for family-wise multiple hypothesis testing are reported in square brackets.

Table 1.A.9 – Expropriations, real estate and promotion incentives

Variables	Employment Growth Construction sec.	Real Estate investment % GDP	Residential RE investment % GDP	Expropriation rate	Urban expansion rank
Source	NBS (1)	NBS (2)	NBS (3)	Survey (4)	Satellite (5)
Size of starting cohort (Normalized)	-0.0825 (0.0645) [1.000]	-0.00264* (0.00138) [0.279]	-0.00178* (0.000928) [0.283]	-0.00114** (0.000550) [0.200]	-0.105** (0.0526) [0.232]
Age at entry	0.0102 (0.0125) [1.000]	-0.000172 (0.000312) [1.000]	-0.000249 (0.000241) [1.000]	-0.0000829 (0.000123) [1.000]	-0.0118 (0.0159) [1.000]
Sample definition					
Prefectures	All	All	All	$N_{expro} > 20$	All
Nb. obs	2678	2678	2648	1324	3054
Nb. clusters	306	306	305	218	341
Mean of dep. var	1.050	0.0656	0.0518	0.00778	5.195

Observations are at the prefecture-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. In column 4, we restrict the sample to the 120 sampling prefectures of the CHARLS survey, where the annual prefecture-level expropriation rate is estimated on at least 20 observations. In column 5, the dependent variable is the prefecture's annual national rank in the growth of total urban area based on satellite data. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. Controls include the CCP secretary's age in the year he takes office (reported in the table), as well as prefecture and calendar year fixed effects. Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively. P-values adjusted for family-wise multiple hypothesis testing are reported in square brackets.

Table 1.A.10 – Expropriation probability and promotion incentives:
Individual-level results

Variables	$\mathbb{1}_{it}\{\text{expro}\}$ (1)	$\mathbb{1}_{it}\{\text{expro}\}$ (2)	$\mathbb{1}_{it}\{\text{expro}\}$ (3)	$\mathbb{1}_{it}\{\text{expro}\}$ (4)	$\mathbb{1}_{it}\{\text{expro}\}$ (5)
Size of starting cohort (Normalized)	-0.00107*** (0.000384) [0.0188]	-0.00110*** (0.000395) [0.0250]	-0.000948* (0.000522) [0.285]	-0.00107*** (0.000390) [0.0268]	-0.00107*** (0.000399) [0.0296]
Control variables					
Start of term year FE	Yes	Yes	No	Yes	Yes
Calendar year FE	No	No	Yes	No	No
Individual FE	No	No	No	No	Yes
Sample definition					
Prefectures	$N > 20$	$N > 20$	$N > 20$	$N > 20$	All
Individuals	All	Rur. h.	All	All	All
CPC Secretaries	All	All	All	$\text{Age}_{t_0} < 55$	All
Nb. Obs	140608	132332	140608	130035	140603
Nb. individuals	10203	9608	10203	10200	10198
Nb. clusters	256	256	256	239	256

Observations are at the individual-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. Controls include the CCP secretary's age in the year he takes office (reported in the table), as well as prefecture and calendar year fixed effects. Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively. P-values adjusted for family-wise multiple hypothesis testing are reported in square brackets.

Table 1.A.11 – Public good provision and promotion incentives

Source	Average Z-score	Variables				
		# Hospital beds	# Doctors	# Buses	# Teachers	# Teachers
		per capita	per capita	per capita	per c. (primary)	per c. (middle)
	NBS	NBS	NBS	NBS	NBS	NBS
	(1)	(2)	(3)	(4)	(5)	(6)
Size of starting cohort (Normalized)	0.0155** (0.00748)	0.190 (0.147) [0.995]	87.16 (59.50) [0.721]	0.146** (0.0644) [0.123]	0.171 (0.162) [1.000]	0.112 (0.174) [1.000]
Age at entry	0.000400 (0.00167)	0.000783 (0.0333) [1.000]	4.605 (11.80) [1.000]	-0.0156 (0.0171) [1.000]	0.0373 (0.0368) [1.000]	0.000322 (0.0298) [1.000]
Nb. obs	2487	2601	2553	2489	2644	2625
Nb. clusters	303	303	302	303	305	306
Mean of dep. var.	-0.0577	11671.8	6571.8	6.465	18267.8	15629.9

Observations are at the prefecture-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. In column 1, the dependent variable is the average Z-score of the 5 variables in columns 2 to 6, where each variable is normalized by its yearly mean and standard deviation. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. Controls include the CCP secretary's age in the year he takes office (reported in the table), as well as prefecture and calendar year fixed effects. Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively. P-values adjusted for family-wise multiple hypothesis testing are reported in square brackets.

Table 1.A.12 – Land expropriation and individual outcomes

	Mobility		Health		Job History			Social security			Assets	
	Change of residence (1)	Hukou ever converted to Urban (2)	Ever Suffered Injury (3)	Ever Confined in Bed (4)	Ever Hospitalized (5)	Job in SOE (6)	Job in Rural Collective (7)	No job in either (8)	Has Any Pension (9)	Has any Medical Coverage (10)	Has any Access to Healthcare (11)	Has Heating (12)
Ever expropriated	-0.0285** (0.0130)	0.0112 (0.00887)	0.0213** (0.00881)	0.0255** (0.0111)	0.0284*** (0.0101)	-0.0801*** (0.00689)	-0.526*** (0.0110)	0.559*** (0.0114)	-0.0149* (0.00862)	0.000479 (0.00951)	-0.00649 (0.00815)	-0.0291*** (0.00972)
Male	0.00374 (0.00939)	0.00179 (0.00587)	0.0615*** (0.00611)	0.0199** (0.00799)	0.0474*** (0.00714)	0.0427*** (0.00582)	0.00318 (0.00783)	-0.0172** (0.00787)	0.0344*** (0.00596)	0.0544*** (0.00645)	0.00239 (0.00636)	-0.0143*** (0.00706)
Ethnic minority	-0.0140 (0.0243)	-0.0166 (0.0143)	0.0120 (0.0154)	0.0346* (0.0205)	0.0260 (0.0179)	0.00770 (0.0170)	0.00749 (0.0193)	-0.0112 (0.0195)	0.000484 (0.0151)	0.00349 (0.0161)	0.00905 (0.0181)	0.0110 (0.0132)
Education level												
No schooling	-0.0481*** (0.0124)	-0.0291*** (0.00671)	0.0141* (0.00830)	0.00422 (0.0113)	0.0158 (0.0102)	-0.0433*** (0.00652)	0.0375*** (0.0102)	-0.0237** (0.0100)	-0.0374*** (0.00663)	-0.0310*** (0.00739)	0.0149 (0.00973)	-0.0176* (0.0101)
Primary school	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.	ref.
Middle School	0.0315*** (0.0118)	0.0544*** (0.00733)	-0.000696 (0.00749)	-0.0116 (0.00942)	-0.00629 (0.00844)	0.0581*** (0.00776)	-0.0749*** (0.00997)	0.0435*** (0.00985)	0.0581*** (0.00771)	0.0560*** (0.00827)	-0.00914 (0.00737)	0.0152* (0.00855)
High School	0.0540*** (0.0193)	0.129*** (0.0147)	-0.0346*** (0.0107)	-0.0172 (0.0143)	-0.00973 (0.0132)	0.174*** (0.0162)	-0.159*** (0.0165)	0.0439*** (0.0167)	0.168*** (0.0159)	0.141*** (0.0158)	-0.0245** (0.0104)	0.0361*** (0.0124)
More than High School	0.137*** (0.0239)	0.422*** (0.0224)	-0.0358*** (0.0130)	-0.00761 (0.0184)	0.00262 (0.0176)	0.503*** (0.0229)	-0.260*** (0.0216)	-0.120*** (0.0206)	0.548*** (0.0218)	0.510*** (0.0218)	-0.0123 (0.0141)	0.0746*** (0.0154)
Nb. Obs	10510	10510	10502	10500	10502	10510	10510	10510	10510	10510	10510	10510
Mean dep. var.	0.34	0.11	0.09	0.17	0.13	0.13	0.54	0.38	0.19	0.20	0.11	0.73

The sample in all columns consists in all 10,627 individuals in the CHARLS sample for whom land expropriation is available, except in column (2) where it is restricted to individuals whose first hukou is of rural type. Education level is included as a set of dummy variables, of which the reference level is primary school. Additional control variables include prefecture-level and year-of-birth fixed effects, as well as health status as child (5 dummy variables) and type of the first hukou. The main regressor of interest is a dummy variable taking value 1 if the individual has ever been expropriated. All columns correspond to linear models estimated using ordinary least-squares. Standard errors are clustered at the prefecture level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively.

Table 1.A.13 – Expropriation and ex-post arrest probability

Variables	$\mathbb{1}\{\text{Arrest}\}$ (1)	$\mathbb{1}\{\text{Arrest}\}$ (2)
Expropriation rate	3.997* (2.402)	1.935 (2.238)
Province FE	Yes	No
Prefecture FE	No	Yes
Nb. of obs.	380	380
Mean of dep. var.	0.15	0.15

Observations are at the prefecture-level. The sample only includes the CHARLS survey sampling prefectures, and years during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion. The dependent variable is a dummy for whether a CCP secretary is caught in an anti-corruption campaign over the course of his career. The regressor of interest is the average expropriation rate in the prefecture and years where the CCP secretary was in office. Controls include the CCP secretary's age in the year he takes office (reported in the table), as well as province (columns 1) or prefecture (column 2) and start-year fixed-effects. Standard errors are robust to heteroskedasticity, with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively.

Table 1.A.14 – Ex-post ghost city status

	<i>P</i> (Ghost city) (1)	<i>P</i> (Ghost city) (2)	<i>P</i> (Ghost city) (3)
Panel A: Weighted regression			
Prefecture-level average expropriation rate	0.0866* (1.77)	0.131** (2.16)	0.112* (1.72)
<i>N</i>	121	121	121
Panel B: Unweighted regression			
Prefecture-level average expropriation rate	0.0665 (1.36)	0.101 (1.61)	0.102 (1.60)
<i>N</i>	121	121	121
Panel C: Panel A + Compensation rates			
Prefecture-level average expropriation rate	0.0931* (1.69)	0.135* (1.98)	0.120* (1.67)
Prefecture-level average compensation rate	-0.00102 (-0.91)	-0.00136 (-0.97)	-0.00221 (-1.50)
<i>N</i>	114	114	114

The dependent variable in column 1 is a dummy variable indicating whether (all or part of the) prefecture has been identified as a ghost city by Jin et al. (2017). The dependent variable in column 2 is a dummy variable that further includes ghost cities identified by the BiaoZhun study (cited in Jin et al. 2017). The dependent variable in column 3 is a dummy variable that further includes ghost cities identified by Chi et al. (2016). The weights used in panels A, and C are the average population of the prefecture. In all regressions, the expropriation rate for a given prefecture is computed as the number of expropriations that occurred between 1996 and 2014 in the prefecture divided by the number of survey respondents registered in said prefecture (all data reported from the CHARLS survey). In all regressions, the sample is restricted to the 121 endline prefectures (excluding prefectures where the sample size is very small or only represented by migrations). In Panel C, the compensation rate for a given prefecture is the share of expropriations that are reported to have been at least partly compensated. 7 prefectures where the expropriation rate is 0 in all years are dropped from the sample. Standard errors are robust to heteroskedasticity, with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively.

Table 1.A.15 – Prefecture outcomes and promotion incentives:
Robustness to variable winsorization

Panel A: Economic growth						
	GDP growth (nominal)	GDP growth (real)	GDP growth rank	GDP growth Primary sector (nominal)	GDP growth Secondary sector (nominal)	GDP growth Tertiary sector (nominal)
Size of starting cohort (Normalized)	-0.00689*** (0.00230) [0.015]	-0.00283*** (0.000990) [0.023]	-0.0923* (0.0548) [0.466]	-0.00286 (0.00204) [0.813]	-0.00865** (0.00364) [0.091]	-0.00335 (0.00221) [0.660]
Nb. clusters	306	305	306	305	306	306
Panel B: Employment						
	Employment growth (Total)	Employment growth (Primary sec.)	Employment growth (Secondary sec.)	Employment growth (Tertiary sec.)		
Size of starting cohort (Normalized)	-0.00523 (0.00366) [0.616]	-0.00826 (0.0111) [1.000]	-0.00289 (0.00349) [1.000]	-0.00618** (0.00300) [0.160]		
Nb. clusters	304	305	303	306		
Panel C: Real estate and expropriation						
	Employment Growth Construction sec.	Real Estate investment % GDP	Residential RE investment % GDP	Expropriation rate	Urban expansion rank	
Size of starting cohort (Normalized)	-0.107* (0.0550) [0.258]	-0.000789 (0.000880) [1.000]	-0.00105 (0.000784) [0.902]	-0.000644* (0.000376) [0.444]	-0.105** (0.0526) [0.232]	
Nb. clusters	304	299	299	217	341	
Panel D: Public good provision						
	Average Z-score	# Hospital beds per capita	# Doctors per capita	# Buses per capita	# Teachers per c. (primary)	# Teachers per c. (middle)
Size of starting cohort (Normalized)	0.0155** (0.00748)	0.183 (0.145) [1.000]	0.172 (0.112) [1.000]	0.145** (0.0648) [1.000]	0.171 (0.162) [1.000]	0.112 (0.174) [1.000]
Nb. clusters	303	303	301	303	305	306

Observations are at the prefecture-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. In column 4 of Panel C, we restrict the sample to the 120 sampling prefectures of the CHARLS survey, where the annual prefecture-level expropriation rate is estimated on at least 20 observations. In column 5 of Panel C, the dependent variable is the prefecture's annual national rank in the growth of total urban area based on satellite data. The regressor of interest is a dummy variable for whether the CCP secretary of the prefecture is promoted at the end of his term. Controls include the CCP secretary's age in the year he takes office, as well as prefecture and calendar year fixed effects (not reported in table). Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively. P-values adjusted for family-wise multiple hypothesis testing are reported in square brackets.

Table 1.A.16 – Individual-level results:
Robustness to CHARLS sample definition

Variables	$\mathbb{1}_{it}\{\text{expro}\}$ (1)	$\mathbb{1}_{it}\{\text{expro}\}$ (2)	$\mathbb{1}_{it}\{\text{expro}\}$ (3)
Size of starting cohort (Normalized)	-0.00102*** (0.000380) [0.0241]	-0.00107*** (0.000384) [0.0188]	-0.00111*** (0.000382) [0.0129]
Sample definition			
Prefectures	All	$N > 20$	$N > 50$
Nb. Obs	142453	140608	138486
Nb. individuals	10333	10203	10050
Nb. clusters	323	256	249

Observations are at the individual-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. Controls include the CCP secretary's age in the year he takes office (reported in the table), as well as prefecture and calendar year fixed effects. Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively. P-values adjusted for family-wise multiple hypothesis testing are reported in square brackets.

Table 1.A.17 – Prefecture outcomes and promotion incentives:
Term-level averages

Panel A: Economic growth						
	GDP growth (nominal)	GDP growth (real)	GDP growth rank	GDP growth Primary sector (nominal)	GDP growth Secondary sector (nominal)	GDP growth Tertiary sector (nominal)
Size of starting cohort (Normalized)	-0.0145** (0.00698) [0.233]	-0.00302 (0.00373) [1]	-0.111 (0.0767) [0.903]	-0.0504 (0.0485) [1]	-0.0228*** (0.00808) [0.0312]	-0.00731 (0.00697) [1]
Mean of dep. var.	0.16	0.13	5.16	0.13	0.18	0.17
Panel B: Employment						
	Employment growth (Total)	Employment growth (Primary sec.)	Employment growth (Secondary sec.)	Employment growth (Tertiary sec.)		
Size of starting cohort (Normalized)	-0.0252* (0.0142) [0.305]	-0.209*** (0.0783) [0.0321]	-0.0192 (0.0131) [0.580]	-0.0148* (0.00827) [0.298]		
Mean of dep. var.	0.00046	0.078	0.013	-0.0020		
Panel C: Real estate and expropriation						
	Employment Growth Construction sec.	Real Estate investment % GDP	Residential RE investment % GDP	Expropriation rate	Urban expansion rank	
Size of starting cohort (Normalized)	-0.108 (0.0835) [0.976]	-0.00464** (0.00212) [0.147]	-0.00265* (0.00145) [0.339]	-0.00128 (0.000897) [0.781]	-0.168** (0.0771) [0.152]	
Mean of dep. var.	0.91	0.063	0.051	0.0080	5.25	
Panel D: Public good provision						
	Average Z-score	# Hospital beds per capita	# Doctors per capita	# Buses per capita	# Teachers per c. (primary)	# Teachers per c. (middle)
Size of starting cohort (Normalized)	0.0225** (0.0102)	0.256 (0.202) [1]	0.0722 (0.161) [1]	0.127 (0.0918) [0.833]	0.311 (0.213) [0.729]	0.216 (0.236) [1]
Mean of dep. var.	-0.04	11775.10	6663.30	6.50	18331.94	15472.92
Nb. of obs.	751	749	753	754	762	760
Nb. of clusters	304	303	302	304	306	306

Observations are at the term level. The sample only includes terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. In column 4 of Panel C, we restrict the sample to the 120 sampling prefectures of the CHARLS survey, where the annual prefecture-level expropriation rate is estimated on at least 20 observations. In column 5 of Panel C, the dependent variable is the prefecture's annual provincial rank in the growth of total urban area based on satellite data. Controls include the CCP secretary's age in the year he takes office, as well as prefecture and calendar year fixed effects (not reported in the table). We further include the following CCP secretary characteristics: ethnic minority (a dummy variable for non-Han ethnicities), native of the province (a dummy variable that takes the value of 1 if the prefecture party secretary was born in the same province), education level (a set of dummy variables for his reported alma mater, see more details in 1.A.6). Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively. P-values adjusted for family-wise multiple hypothesis testing are reported in square brackets.

Table 1.A.18 – Promotion incentives and Prefecture outcomes:
Weighting observations by the inverse of the term length

Panel A: Economic growth						
	GDP growth (nominal)	GDP growth (real)	GDP growth rank	GDP growth Primary sector (nominal)	GDP growth Secondary sector (nominal)	GDP growth Tertiary sector (nominal)
Size of starting cohort (Normalized)	-0.00820*** (0.00232) [0.003]	-0.00260** (0.00118) [0.170]	-0.0740 (0.0636) [1.000]	-0.00460 (0.00345) [1.000]	-0.0125*** (0.00385) [0.008]	-0.00400* (0.00238) [0.567]
Mean of dep. var.	0.16	0.13	5.16	0.100	0.18	0.16
Panel B: Employment						
	Employment growth (Total)	Employment growth (Primary sec.)	Employment growth (Secondary sec.)	Employment growth (Tertiary sec.)		
Size of starting cohort (Normalized)	-0.0134** (0.00598) [0.103]	-0.0823** (0.0359) [0.090]	-0.0141** (0.00553) [0.045]	-0.0105** (0.00522) [0.185]		
Mean of dep. var.	0.0023	0.044	0.012	0.0016		
Panel C: Real estate and expropriation						
	Employment Growth Construction sec.	Real Estate investment % GDP	Residential RE investment % GDP	Expropriation rate	Urban expansion rank	
Size of starting cohort (Normalized)	-0.0887 (0.0606) [0.723]	-0.00273** (0.00138) [0.246]	-0.00168* (0.000906) [0.320]	-0.00109** (0.000550) [0.252]	-0.103 (0.0642) [0.542]	
Mean of dep. var.	1.05	0.066	0.052	0.0078	5.20	
Panel D: Public good provision						
	Average Z-score	# Hospital beds per capita	# Doctors per capita	# Buses per capita	# Teachers per c. (primary)	# Teachers per c. (middle)
Size of starting cohort (Normalized)	0.0187*** (0.00706)	0.167 (0.137) [1.000]	82.84 (56.93) [0.734]	0.122* (0.0619) [0.252]	0.205 (0.158) [0.982]	0.103 (0.178) [1.000]
Mean of dep. var.	-0	11672	6572	6	18268	15630
Nb. of obs.	2487.0	2601.0	2553.0	2489.0	2644.0	2625.0
Nb. of clusters	303.000	303.000	302.000	303.000	305.000	306.000

Observations are at the prefecture-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. In column 4 of Panel C, we restrict the sample to the 120 sampling prefectures of the CHARLS survey, where the annual prefecture-level expropriation rate is estimated on at least 20 observations. In column 5 of Panel C, the dependent variable is the prefecture's annual provincial rank in the growth of total urban area based on satellite data. Controls include the CCP secretary's age in the year he takes office, as well as prefecture and calendar year fixed effects (not reported in table). Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively. P-values adjusted for family-wise multiple hypothesis testing are reported in square brackets.

Table 1.A.19 – Prefecture outcomes and promotion incentives:
Robustness to CCP secretary characteristics

Panel A: Economic growth						
	GDP growth (nominal)	GDP growth (real)	GDP growth rank	GDP growth Primary sector (nominal)	GDP growth Secondary sector (nominal)	GDP growth Tertiary sector (nominal)
Size of starting cohort (Normalized)	-0.00952*** (0.00225) [0.000]	-0.00362*** (0.00127) [0.029]	-0.0203 (0.0762) [1.000]	-0.00362 (0.00413) [1.000]	-0.0127*** (0.00412) [0.013]	-0.00681** (0.00270) [0.073]
Mean of dep. var.	0.16	0.13	5.16	0.100	0.18	0.16
Panel B: Employment						
	Employment growth (Total)	Employment growth (Primary sec.)	Employment growth (Secondary sec.)	Employment growth (Tertiary sec.)		
Size of starting cohort (Normalized)	-0.0137* (0.00708) [0.214]	-0.113* (0.0603) [0.247]	-0.0154** (0.00644) [0.070]	-0.00968 (0.00648) [0.546]		
Mean of dep. var.	0.0023	0.044	0.012	0.0016		
Panel C: Real estate and expropriation						
	Employment Growth Construction sec.	Real Estate investment % GDP	Residential RE investment % GDP	Expropriation rate	Urban expansion rank	
Size of starting cohort (Normalized)	-0.0463 (0.0873) [1.000]	-0.00253 (0.00192) [0.940]	-0.00126 (0.00114) [1.000]	-0.00189*** (0.000481) [0.001]	-0.103 (0.0699) [0.703]	
Mean of dep. var.	1.05	0.066	0.052	0.0073	5.20	
Panel D: Public good provision						
	Average Z-score	# Hospital beds per capita	# Doctors per capita	# Buses per capita	# Teachers per c. (primary)	# Teachers per c. (middle)
Size of starting cohort (Normalized)	0.0169** (0.00741)	0.0234 (0.162) [1.000]	0.0945 (0.138) [1.000]	0.199** (0.0894) [0.133]	0.135 (0.180) [1.000]	0.216 (0.162) [0.917]
Mean of dep. var.	-0	11672	6572	6	18268	15630
Nb. of obs.	1717.0	1768.0	1764.0	1720.0	1797.0	1786.0
Nb. of clusters	252.000	257.000	255.000	252.000	257.000	258.000

Observations are at the prefecture-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. In column 4 of Panel C, we restrict the sample to the 120 sampling prefectures of the CHARLS survey, where the annual prefecture-level expropriation rate is estimated on at least 20 observations. In column 5 of Panel C, the dependent variable is the prefecture's annual provincial rank in the growth of total urban area based on satellite data. Controls include the CCP secretary's age in the year he takes office, as well as prefecture and calendar year fixed effects (not reported in table). We further include the following CCP secretary characteristics: ethnic minority (a dummy variable for non-Han ethnicities), native of the province (a dummy variable that takes the value of 1 if the prefecture party secretary was born in the same province), education level (a set of dummy variables for his reported alma mater, see more details in 1.A.6). Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively. P-values adjusted for family-wise multiple hypothesis testing are reported in square brackets.

Table 1.A.20 – GDP growth and promotion incentives:
Time-varying effect of competition

Variables	GDP growth (nominal)	GDP growth (real)	GDP growth rank	GDP growth Primary sector (nominal)	GDP growth Secondary sector (nominal)	GDP growth Tertiary sector (nominal)
Source	NBS (1)	NBS (2)	Satellite (3)	NBS (4)	NBS (5)	NBS (6)
Size of starting cohort (Normalized)	-0.00875*** (0.00309) [0.0297]	-0.00345** (0.00139) [0.0842]	-0.182* (0.0952) [0.345]	-0.00580 (0.00496) [1]	-0.0132*** (0.00486) [0.0411]	-0.00254 (0.00380) [1]
Size of starting cohort \times 2 nd year	0.00176 (0.00372)	-0.0000752 (0.00158)	0.172 (0.153)	-0.00635 (0.00912)	0.00754 (0.00562)	-0.00429 (0.00490)
Size of starting cohort \times 3 rd year	-0.00238 (0.00556)	0.000576 (0.00246)	0.0454 (0.165)	0.00254 (0.00801)	-0.00520 (0.00916)	-0.00593 (0.00699)
Size of starting cohort \times 4 th year	0.00309 (0.00587)	-0.000433 (0.00219)	0.180 (0.184)	0.00874 (0.00742)	0.00325 (0.0103)	0.000948 (0.00649)
Size of starting cohort \times 5 th year	-0.00491 (0.00747)	-0.000360 (0.00243)	0.0862 (0.211)	0.00289 (0.0108)	-0.0127 (0.0144)	-0.00543 (0.0103)
Size of starting cohort \times 6 th year	0.00631 (0.00612)	0.00240 (0.00250)	0.317 (0.292)	0.0139 (0.0132)	0.0133 (0.0117)	-0.000836 (0.00818)
Size of starting cohort \times 7 th year	0.00373 (0.0139)	0.000455 (0.00402)	0.0191 (0.605)	-0.00290 (0.0148)	0.00601 (0.0295)	0.0230 (0.0244)
Size of starting cohort \times 8 th year	0.0486* (0.0252)	0.00580 (0.00481)	-0.315 (0.812)	-0.0148 (0.0267)	0.0803 (0.0695)	0.0912* (0.0511)
Size of starting cohort \times 9 th year	0.0754* (0.0455)	0.0178** (0.00768)	1.035 (1.435)	-0.0133 (0.0363)	0.149 (0.124)	0.102 (0.0742)
Size of starting cohort \times 10 th year	0.0576*** (0.0120)	0.0188 (0.0164)	0.560 (1.245)	0.000354 (0.0203)	0.171** (0.0819)	0.0207 (0.0264)
Size of starting cohort \times 11 th year	0.206*** (0.0182)	0.0243*** (0.00615)	0.151 (0.318)	-0.0854*** (0.0219)	0.514*** (0.0682)	0.278*** (0.0271)
Nb. obs	2678	2677	2678	2678	2678	2678
Nb. clusters	306	306	306	306	306	306
Mean of dep. var.	0.158	0.127	5.160	0.0996	0.183	0.161

Observations are at the prefecture-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. Controls include the CCP secretary's age in the year he takes office (reported in the table), as well as prefecture and calendar year fixed effects. Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively.

Table 1.A.21 – Employment growth and promotion incentives:
Time-varying effect of competition

Variables	Employment growth (Total)	Employment growth (Primary sec.)	Employment growth (Secondary sec.)	Employment growth (Tertiary sec.)
Source	NBS (1)	NBS (2)	NBS (3)	NBS (4)
Size of starting cohort (Normalized)	-0.00214 (0.00797)	0.0678 (0.0850)	-0.00354 (0.00789)	-0.00620 (0.00727)
	[1]	[1]	[1]	[1]
Size of starting cohort \times 2 nd year	-0.00981 (0.0111)	-0.315 (0.264)	-0.00911 (0.00976)	-0.00152 (0.00914)
Size of starting cohort \times 3 rd year	-0.0294 (0.0195)	-0.239 (0.180)	-0.0228 (0.0180)	-0.0114 (0.0138)
Size of starting cohort \times 4 th year	-0.0105 (0.00897)	-0.0624 (0.0966)	-0.0172 (0.0116)	-0.00191 (0.00744)
Size of starting cohort \times 5 th year	-0.00346 (0.00984)	0.0717 (0.125)	-0.00471 (0.0108)	0.00107 (0.00861)
Size of starting cohort \times 6 th year	-0.00190 (0.0112)	0.0696 (0.197)	-0.00142 (0.0136)	0.00719 (0.00947)
Size of starting cohort \times 7 th year	-0.0348** (0.0166)	0.0794 (0.120)	-0.0316 (0.0258)	-0.0229* (0.0119)
Size of starting cohort \times 8 th year	-0.0239 (0.0179)	-0.346** (0.139)	-0.0280 (0.0222)	-0.00886 (0.0152)
Size of starting cohort \times 9 th year	-0.0416 (0.0317)	-0.0569 (0.132)	-0.0247 (0.0633)	-0.0603*** (0.0119)
Size of starting cohort \times 10 th year	-0.00712 (0.0601)	0.0237 (0.167)	0.00639 (0.0968)	0.0138 (0.0277)
Size of starting cohort \times 11 th year	-0.0213 (0.0202)	0.190 (0.428)	0.0338* (0.0176)	-0.0357** (0.0173)
Nb. obs	2678	2671	2678	2678
Nb. clusters	306	306	306	306
Mean of dep. var.	0.00227	0.0436	0.0122	0.00159

Observations are at the prefecture-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. Controls include the CCP secretary's age in the year he takes office (reported in the table), as well as prefecture and calendar year fixed effects. Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively.

Table 1.A.22 – Expropriations, real estate and promotion incentives:
Time-varying effect of competition

Variables	Employment Growth Construction sec.	Real Estate investment % GDP	Residential RE investment % GDP	Expropriation rate	Urban expansion rank
Source	NBS (1)	NBS (2)	NBS (3)	Survey (4)	Satellite (5)
Size of starting cohort (Normalized)	-0.00665 (0.130) [1]	-0.00367** (0.00163) [0.126]	-0.00238* (0.00121) [0.254]	-0.00103 (0.000892) [1]	-0.185** (0.0862) [0.161]
Size of starting cohort \times 2 nd year	-0.229 (0.187)	0.00183 (0.00142)	0.00140 (0.00117)	-0.000644 (0.00138)	0.0807 (0.130)
Size of starting cohort \times 3 rd year	0.000693 (0.201)	0.00298 (0.00205)	0.000354 (0.00171)	0.0000147 (0.00124)	0.0685 (0.142)
Size of starting cohort \times 4 th year	-0.0218 (0.207)	-0.00101 (0.00399)	0.00109 (0.00208)	0.000781 (0.00151)	0.240 (0.173)
Size of starting cohort \times 5 th year	-0.101 (0.151)	0.000445 (0.00315)	-0.000423 (0.00276)	0.00000885 (0.00192)	0.253 (0.198)
Size of starting cohort \times 6 th year	0.179 (0.309)	0.000576 (0.00395)	0.00108 (0.00302)	0.00241 (0.00400)	0.192 (0.295)
Size of starting cohort \times 7 th year	-0.173 (0.248)	0.00577 (0.00471)	0.00231 (0.00407)	-0.00120 (0.00329)	-0.173 (0.573)
Size of starting cohort \times 8 th year	-0.245 (0.434)	0.0145** (0.00706)	0.0105 (0.00683)	0.00102 (0.00346)	0.635 (0.760)
Size of starting cohort \times 9 th year	0.518 (1.150)	0.0127 (0.0112)	0.0142* (0.00832)	0.00611*** (0.00197)	-1.183 (0.781)
Size of starting cohort \times 10 th year	2.057 (2.693)	0.0295* (0.0155)	0.0263* (0.0158)	0.000654 (0.00232)	0.422 (0.949)
Size of starting cohort \times 11 th year	0.286 (0.415)	0.00271 (0.00415)	0.0142*** (0.00382)	0 (.)	-3.222*** (0.270)
Sample definition					
Prefectures	All	All	All	$N_{expro} > 20$	All
Nb. obs	2678	2678	2648	1324	3052
Nb. clusters	306	306	305	218	341
Mean of dep. var	1.050	0.0656	0.0518	0.00778	5.195

Observations are at the prefecture-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. In column 4, we restrict the sample to the 120 sampling prefectures of the CHARLS survey, where the annual prefecture-level expropriation rate is estimated on at least 20 observations. In column 5, the dependent variable is the prefecture's annual national rank in the growth of total urban area based on satellite data. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. Controls include the CCP secretary's age in the year he takes office (reported in the table), as well as prefecture and calendar year fixed effects. Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively.

Table 1.A.23 – Public good provision and promotion incentives:
Time-varying effect of competition

Variables	Z-score of 5 variables	Number of Hospital beds	Number of Doctors	Per capita Buses	Primary sch. teachers	Middle sch. teachers
Source	NBS (1)	NBS (2)	NBS (3)	NBS (4)	NBS (5)	(6)
Size of starting cohort (Normalized)	0.0279*** (0.00967)	0.0811 (0.177)	127.4** (57.73)	0.253*** (0.0895)	0.370* (0.208)	0.208 (0.206)
Size of starting cohort \times 2 nd year	-0.0154* (0.00792)	0.141 (0.131)	-67.04 (52.32)	-0.0553 (0.0773)	-0.262 (0.176)	-0.113 (0.134)
Size of starting cohort \times 3 rd year	-0.00896 (0.0101)	0.206 (0.156)	-73.24 (72.43)	-0.193** (0.0936)	-0.315 (0.233)	-0.170 (0.213)
Size of starting cohort \times 4 th year	-0.0288** (0.0144)	0.0833 (0.229)	4.132 (76.75)	-0.276** (0.123)	-0.483* (0.271)	-0.283 (0.247)
Size of starting cohort \times 5 th year	-0.0465** (0.0196)	0.173 (0.297)	-110.6 (100.1)	-0.174 (0.150)	-0.159 (0.456)	0.0269 (0.350)
Size of starting cohort \times 6 th year	-0.00968 (0.0163)	0.248 (0.318)	-82.15 (135.1)	-0.493** (0.241)	-0.127 (0.383)	-0.0375 (0.356)
Size of starting cohort \times 7 th year	0.0408 (0.0286)	0.565 (0.742)	-385.1 (266.6)	-0.441 (0.366)	0.698 (0.459)	0.842 (0.573)
Size of starting cohort \times 8 th year	0.00551 (0.0696)	0.966 (1.903)	-760.5 (536.3)	-0.946 (0.632)	0.615 (1.014)	1.168 (1.073)
Size of starting cohort \times 9 th year	-0.176*** (0.0451)	-0.508 (1.166)	-933.0** (380.1)	-2.383** (0.983)	-1.562 (1.666)	-1.428 (1.269)
Size of starting cohort \times 10 th year	-0.193*** (0.0654)	0.874 (0.978)	-1001.3** (412.5)	-1.824*** (0.444)	-2.580 (2.804)	-0.897 (1.564)
Size of starting cohort \times 11 th year	-0.596*** (0.0426)	-6.095*** (0.715)	-864.5*** (282.6)	-0.669 (0.513)	-14.90*** (1.141)	-8.512*** (0.862)
Nb. obs	2487	2601	2553	2489	2644	2625
Nb. clusters	303	303	302	303	305	306
Mean of dep. var.	-0.0577	11671.8	6571.8	6.465	18267.8	15629.9

Observations are at the prefecture-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. The endogenous regressor of interest is a dummy variable for whether the CCP secretary of the prefecture is promoted at the end of his term. The instrumental variable is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. We report the robust F-statistic for the strength of the first stage (see Kleibergen and Paap, 2006), as well as the Anderson-Rubin p-value promotion for robust inference to weak IVs in the presence of clustered standard errors (see Andrews, Stock and Sun, 2019). Controls include the CCP secretary's age in the year he takes office (reported in the table), as well as prefecture and calendar year fixed effects. Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively.

Table 1.A.24 – GDP data manipulation - Reported real GDP growth rate:
Presence of decimals

Variables	Has a first Decimal (1)	Has a first Decimal (2)	Has a second Decimal (3)	Has a second Decimal (4)
Size of starting cohort (Normalized)	-0.000901 (0.00883)		-0.0122 (0.00837)	
Promotion likelihood		0.0138 (0.128)		0.187 (0.142)
Nb. of obs.	2677	2679	2677	2679
Nb. of clusters	306	306	306	306
Mean of dep. var.	0.84	0.84	0.11	0.11
First-stage F-statistic		12.03		12.03
Weak-IV robust p-value		0.9139		0.1215

Observations are at the prefecture-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. Columns 1 and 3 show IV results and Columns 2 and 4 report the reduced form. The dependent variable is a dummy for whether or not the reported real GDP growth rate has one decimal digit (columns 1 and 2) or two decimal digits (columns 3 and 4). The endogenous regressor of interest is a dummy variable for whether the CCP secretary of the prefecture is promoted at the end of his term. The instrumental variable is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. We report the robust F-statistic for the strength of the first stage (see Kleibergen and Paap, 2006), as well as the Anderson-Rubin p-value promotion for robust inference to weak IVs in the presence of clustered standard errors (see Andrews, Stock and Sun, 2019). Controls include the CCP secretary's age in the year he takes office (reported in the table), as well as prefecture and calendar year fixed effects. Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively.

Table 1.A.25 – GDP data manipulation - Reported real GDP growth rate:
Distribution of decimal digits

Panel A: first decimal digit									
	<i>D1 = 1</i>	<i>D1 = 2</i>	<i>D1 = 3</i>	<i>D1 = 4</i>	<i>D1 = 5</i>	<i>D1 = 6</i>	<i>D1 = 7</i>	<i>D1 = 8</i>	<i>D1 = 9</i>
Size of starting cohort (Normalized)	0.00204 (0.00860) [1.000]	-0.00256 (0.00847) [1.000]	0.00231 (0.00689) [1.000]	0.00230 (0.00581) [1.000]	-0.00249 (0.00738) [1.000]	0.00534 (0.00748) [1.000]	0.00264 (0.00701) [1.000]	-0.00482 (0.00654) [1.000]	-0.00568 (0.00612) [1.000]
Mean of dep. var.	0.14	0.11	0.091	0.066	0.11	0.096	0.071	0.096	0.059
Nb. of clusters	306	306	306	306	306	306	306	306	306
Panel B: second decimal digit									
	<i>D2 = 1</i>	<i>D2 = 2</i>	<i>D2 = 3</i>	<i>D2 = 4</i>	<i>D2 = 5</i>	<i>D2 = 6</i>	<i>D2 = 7</i>	<i>D2 = 8</i>	<i>D2 = 9</i>
Size of starting cohort (Normalized)	0.00171 (0.00254) [1.000]	-0.00809*** (0.00277) [0.076]	-0.00188 (0.00285) [1.000]	-0.00110 (0.00224) [1.000]	-0.00739** (0.00335) [0.557]	-0.00409 (0.00331) [1.000]	0.000937 (0.00264) [1.000]	0.00855*** (0.00280) [0.049]	-0.000825 (0.00224) [1.000]
Mean of dep. var.	0.013	0.012	0.012	0.012	0.016	0.015	0.0090	0.010	0.0097
Nb. of clusters	306	306	306	306	306	306	306	306	306
Nb. of observations	2677	2677	2677	2677	2677	2677	2677	2677	2677

Observations are at the prefecture-year level. The sample only includes observations during terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. In each column, the dependent variable is a dummy equal to 1 if the first (Panel A) or second (Panel B) decimal digit of the reported real growth rate of a prefecture in a given year is equal to 1, 2, ..., 9. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. Controls include the CCP secretary's age in the year he takes office (reported in the table), as well as prefecture and calendar year fixed effects. Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively. P-values adjusted for family-wise multiple hypothesis testing are reported in square brackets.

Table 1.A.26 – Prefecture outcomes and promotion incentives (Reduced forms):
Pretrends (1)

Panel A: Economic growth						
	GDP growth (nominal)	GDP growth (real)	GDP growth rank	GDP growth Primary sector (nominal)	GDP growth Secondary sector (nominal)	GDP growth Tertiary sector (nominal)
Size of starting cohort	-0.000248 (0.00165)	-0.000504 (0.000764)	-0.00137 (0.00315)	0.00121 (0.00246)	0.00257 (0.00297)	-0.0194 (0.0582)
Nb. clusters	299	300	287	286	284	306
Panel B: Employment						
	Employment growth (Total)	Employment growth (Primary sec.)	Employment growth (Secondary sec.)	Employment growth (Tertiary sec.)		
Size of starting cohort	-0.00338 (0.00285)	-0.00463 (0.00548)	-0.00447 (0.00323)	-0.00181 (0.00300)		
Nb. clusters	284	284	281	282		
Panel C: Real estate and expropriation						
	Employment Growth Construction sec.	Real Estate investment % GDP	Residential RE investment % GDP	Expropriation rate	Urban expansion rank	
Size of starting cohort	-0.00281 (0.0320)	-0.000536 (0.000434)	-0.000974* (0.000515)	-0.000215 (0.000301)	-0.0144 (0.0601)	
Nb. clusters	245	285	262	237	301	
Panel D: Public good provision						
	Average Z-score	# Hospital beds per capita	# Doctors per capita	# Buses per capita	# Teachers per c. (primary)	# Teachers per c. (middle)
Size of starting cohort	0.00535 (0.00448)	0.155** (0.0784)	-7.403 (31.20)	0.0567 (0.0529)	0.0362 (0.120)	0.0562 (0.119)
Nb. clusters	209	291	295	210	296	296

Observations are at the prefecture-year level. The sample only includes terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. In column 4 of Panel C, we restrict the sample to the 120 sampling prefectures of the CHARLS survey, where the annual prefecture-level expropriation rate is estimated on at least 20 observations. In column 5 of Panel C, the dependent variable is the prefecture's annual national rank in the growth of total urban area based on satellite data. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who start their terms in the same year as said prefectural CCP secretary. In each cell, the dependent variable is the pre-trends of the outcome variable. It is computed as the mean of the 1-year and 2-year lagged residuals of the outcome variable on year and prefecture fixed effects. Controls include the CCP secretary's age in the year he takes office, as well as prefecture and calendar year fixed effects (not reported in table). Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively. P-values adjusted for family-wise multiple hypothesis testing are reported in square brackets.

Table 1.A.27 – Cohort size and previous events

	(1)	(2)	(3)	(4)
Nb of t-1 cohort secretaries who are older than 55	0.116** (0.0469)			0.197*** (0.0449)
Nb of t-1 cohort secretaries who were promoted		0.972*** (0.0805)		0.924*** (0.0461)
Nb of t-1 cohort secretaries who retired			0.942*** (0.0791)	0.880*** (0.0460)
Nb. clusters	364	364	364	364

Observations are at the prefecture-year level. The sample only includes terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. In each column, the dependent variable is the starting cohort size in a given province and year. Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively. P-values adjusted for family-wise multiple hypothesis testing are reported in square brackets.

Table 1.A.28 – Prefecture outcomes and promotion incentives (Reduced forms):
Age of predecessors as instrument for cohort size

Panel A: Economic growth						
	GDP growth (nominal)	GDP growth (real)	GDP growth rank	GDP growth Primary sector (nominal)	GDP growth Secondary sector (nominal)	GDP growth Tertiary sector (nominal)
Nb of t-1 cohort secretaries who are older than 55	-0.00350*** (0.00116)	-0.00174*** (0.000583)	-0.000692 (0.00124)	-0.00670*** (0.00180)	-0.000294 (0.00120)	-0.0195 (0.0287)
Nb. clusters	291	291	291	291	291	291
Panel B: Employment						
	Employment growth (Total)	Employment growth (Primary sec.)	Employment growth (Secondary sec.)	Employment growth (Tertiary sec.)		
Nb of t-1 cohort secretaries who are older than 55	0.00229 (0.00279)	0.0245 (0.0262)	0.000368 (0.00230)	0.00288 (0.00232)		
Nb. clusters	291	291	291	291		
Panel C: Real estate and expropriation						
	Employment Growth Construction sec.	Real Estate investment % GDP	Residential RE investment % GDP	Expropriation rate	Urban expansion rank	
Nb of t-1 cohort secretaries who are older than 55	-0.0502 (0.0401)	-0.000944 (0.000652)	0.000220 (0.000514)	-0.000845** (0.000425)	0.00760 (0.0288)	
Nb. clusters	291	291	290	235	291	
Panel D: Public good provision						
	Average Z-score	# Hospital beds per capita	# Doctors per capita	# Buses per capita	# Teachers per c. (primary)	# Teachers per c. (middle)
Nb of t-1 cohort secretaries who are older than 55	0.000602 (0.00317)	-0.0845 (0.0742)	65.42** (28.34)	0.0386 (0.0357)	0.0862 (0.0687)	-0.241*** (0.0848)
Nb. clusters	284	289	287	288	290	291

Observations are at the prefecture-year level. The sample only includes terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. In column 4 of Panel C, we restrict the sample to the 120 sampling prefectures of the CHARLS survey, where the annual prefecture-level expropriation rate is estimated on at least 20 observations. In column 5 of Panel C, the dependent variable is the prefecture's annual national rank in the growth of total urban area based on satellite data. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who were older than 55 in the previous year. Controls include the CCP secretary's age in the year he takes office, as well as prefecture and calendar year fixed effects (not reported in table). Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively.

Table 1.A.29 – Prefecture outcomes and promotion incentives (Reduced forms):
Controlling for promotion rate before term

Panel A: Economic growth						
	GDP growth (nominal)	GDP growth (real)	GDP growth rank	GDP growth Primary sector (nominal)	GDP growth Secondary sector (nominal)	GDP growth Tertiary sector (nominal)
Size of starting cohort	-0.0109*** (0.00318)	-0.00335** (0.00150)	0.000893 (0.00334)	-0.0150*** (0.00512)	-0.00811*** (0.00292)	-0.0469 (0.0732)
Promotion rate of year preceding term	-0.0267*** (0.00901)	-0.00661 (0.00412)	0.00151 (0.00915)	-0.0390*** (0.0122)	-0.00911 (0.00914)	-0.251 (0.189)
Nb. clusters	232	232	232	232	232	232
Panel B: Employment						
	Employment growth (Total)	Employment growth (Primary sec.)	Employment growth (Secondary sec.)	Employment growth (Tertiary sec.)		
Size of starting cohort	-0.0227*** (0.00806)	-0.209*** (0.0682)	-0.0160** (0.00622)	-0.0166** (0.00723)		
Promotion rate of year preceding term	-0.0161 (0.0188)	-0.373 (0.272)	-0.00556 (0.0138)	-0.0152 (0.0166)		
Nb. clusters	232	232	232	232		
Panel C: Real estate and expropriation						
	Employment Growth Construction sec.	Real Estate investment % GDP	Residential RE investment % GDP	Expropriation rate	Urban expansion rank	
Size of starting cohort	-0.160* (0.0896)	-0.00521** (0.00218)	-0.00253** (0.00127)	-0.0000548 (0.000829)	-0.0965 (0.0755)	
Promotion rate of year preceding term	0.137 (0.256)	0.00394 (0.00401)	0.00744** (0.00329)	0.00305 (0.00408)	0.0658 (0.218)	
Nb. clusters	232	232	231	193	232	
Panel D: Public good provision						
	Average Z-score	# Hospital beds per capita	# Doctors per capita	# Buses per capita	# Teachers per c. (primary)	# Teachers per c. (middle)
Size of starting cohort	0.00960 (0.00833)	0.267 (0.192)	46.29 (77.53)	0.140* (0.0796)	0.179 (0.198)	-0.143 (0.216)
Promotion rate of year preceding term	0.0342 (0.0220)	1.618*** (0.399)	350.6* (202.7)	0.384* (0.225)	0.243 (0.511)	-0.225 (0.446)
Nb. clusters	231	231	232	231	231	232

Observations are at the prefecture-year level. The sample only includes terms that start at the earliest in 1996 and finish before 2014 and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. In column 4 of Panel C, we restrict the sample to the 120 sampling prefectures of the CHARLS survey, where the annual prefecture-level expropriation rate is estimated on at least 20 observations. In column 5 of Panel C, the dependent variable is the prefecture's annual national rank in the growth of total urban area based on satellite data. The regressor of interest is the number of other prefecture-level CCP secretaries in the province who were older than 55 in the previous year. Controls include the CCP secretary's age in the year he takes office, as well as prefecture and calendar year fixed effects (not reported in table). Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively. P-values adjusted for family-wise multiple hypothesis testing are reported in square brackets.

Table 1.A.30 – Theoretical cohort size and actual cohort size

	(1) All	(2) Before 2004	(3) Before 2000
Theoretical cohort size, initialized in 1996	0.0996* (0.0594)	0.262*** (0.0782)	0.488*** (0.106)
Observations	390	215	118

Observations are at the province-start year level. The dependent variable is the starting cohort size, defined as the number of prefecture-level party secretaries who start their term in the province in a given year. The regressor of interest is the theoretical starting cohort size as computed by extra-polating from the 1996 distribution of term lengths, and applying a fixed length of 5 years (following the theoretical length of a term). Controls include province level and start year level fixed effects. Standard errors are reported in parentheses with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively.

Table 1.A.31 – Prefecture outcomes and promotion incentives (Reduced forms):
Theoretical cohort size

Panel A: Economic growth						
	GDP growth (nominal)	GDP growth (real)	GDP growth rank	GDP growth Primary sector (nominal)	GDP growth Secondary sector (nominal)	GDP growth Tertiary sector (nominal)
Theoretical starting cohort size	0.000324 (0.000885)	0.000354 (0.000352)	-0.00115 (0.00134)	0.000784 (0.00122)	-0.000303 (0.00125)	-0.0130 (0.0360)
Nb. clusters	283	283	283	283	283	283
Panel B: Employment						
	Employment growth (Total)	Employment growth (Primary sec.)	Employment growth (Secondary sec.)	Employment growth (Tertiary sec.)		
Theoretical starting cohort size	0.00438 (0.00279)	0.00243 (0.0166)	0.00442 (0.00269)	0.00107 (0.00185)		
Nb. clusters	283	283	283	283		
Panel C: Real estate and expropriation						
	Employment Growth Construction sec.	Real Estate investment % GDP	Residential RE investment % GDP	Expropriation rate	Urban expansion rank	
Theoretical starting cohort size	0.0609* (0.0368)	0.000501 (0.000356)	0.000248 (0.000203)	-0.0000569 (0.000433)	-0.0241 (0.0289)	
Nb. clusters	283	283	282	227	283	
Panel D: Public good provision						
	Average Z-score	# Hospital beds per capita	# Doctors per capita	# Buses per capita	# Teachers per c. (primary)	# Teachers per c. (middle)
Theoretical starting cohort size	-0.00107 (0.00207)	0.0316 (0.0259)	23.92** (10.58)	-0.0179 (0.0169)	0.00124 (0.0338)	-0.0236 (0.0319)
Nb. clusters	280	281	279	280	282	283

Observations are at the prefecture-year level. The sample only includes terms that start at the earliest in 1996 and finish before 2014 for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. In column 4 of Panel C, we restrict the sample to the 120 sampling prefectures of the CHARLS survey, where the annual prefecture-level expropriation rate is estimated on at least 20 observations. In column 5 of Panel C, the dependent variable is the prefecture's annual national rank in the growth of total urban area based on satellite data. The regressor of interest is the theoretical value of the number of other prefecture-level CCP secretaries in the province who start in that year: this variable is computed by extrapolating the 1996 structure of current term lengths and applying a strict 5-year term length (as per the official administrative rule). Controls include the CCP secretary's age in the year he takes office, as well as prefecture and calendar year fixed effects (not reported in table). Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively.

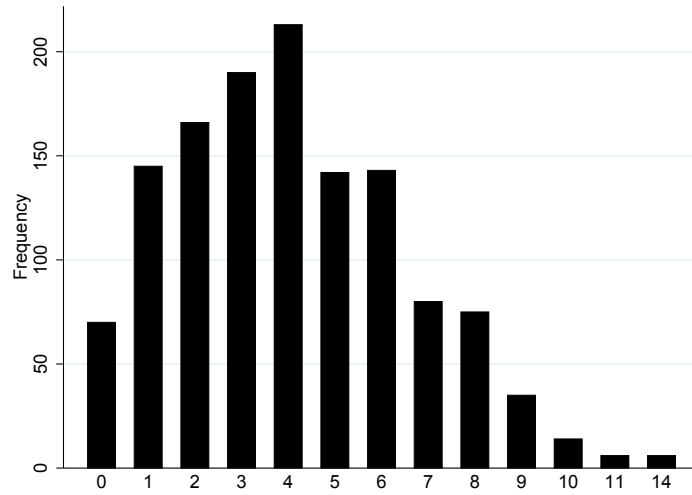
Table 1.A.32 – Prefecture outcomes and promotion incentives (Reduced forms):
Placebo tests for lagged cohort size

Panel A: Economic growth						
	GDP growth (nominal)	GDP growth (real)	GDP growth rank	GDP growth Primary sector (nominal)	GDP growth Secondary sector (nominal)	GDP growth Tertiary sector (nominal)
Lagged starting cohort size	0.000916 (0.00235)	0.000520 (0.00111)	-0.00231 (0.00229)	0.0000195 (0.00329)	-0.000484 (0.00264)	-0.0449 (0.0499)
Nb. clusters	281	281	281	281	281	281
Panel B: Employment						
	Employment growth (Total)	Employment growth (Primary sec.)	Employment growth (Secondary sec.)	Employment growth (Tertiary sec.)		
Lagged starting cohort size	0.00516 (0.00489)	-0.0233 (0.0407)	0.00314 (0.00448)	0.00553 (0.00429)		
Nb. clusters	281	281	281	281		
Panel C: Real estate and expropriation						
	Employment Growth Construction sec.	Real Estate investment % GDP	Residential RE investment % GDP	Expropriation rate	Urban expansion rank	
Lagged starting cohort size	-0.0134 (0.0653)	0.00211* (0.00121)	0.000620 (0.000820)	0.000350 (0.00141)	-0.0626 (0.0616)	
Nb. clusters	281	281	280	226	281	
Panel D: Public good provision						
	Average Z-score	# Hospital beds per capita	# Doctors per capita	# Buses per capita	# Teachers per c. (primary)	# Teachers per c. (middle)
Lagged starting cohort size	-0.00427 (0.00559)	-0.110 (0.104)	70.33 (52.35)	-0.0566 (0.0642)	0.0342 (0.140)	-0.120 (0.149)
Nb. clusters	278	279	277	278	280	281

Observations are at the prefecture-year level. Contrary to the main specifications, the sample only includes terms that **start at the earliest in 1997 and finish before 2013** (as this is the subsample for which lagged and lead values of the cohort size are available) and for which we observe the prefecture party secretary's promotion and age, as well as all macroeconomic variables of interest. In column 4 of Panel C, we restrict the sample to the 120 sampling prefectures of the CHARLS survey, where the annual prefecture-level expropriation rate is estimated on at least 20 observations. In column 5 of Panel C, the dependent variable is the prefecture's annual national rank in the growth of total urban area based on satellite data. The regressor of interest is the 1-year lagged value of the starting cohort size (the value it would have taken had the bureaucrat started one year earlier). Controls include the CCP secretary's age in the year he takes office, as well as prefecture and calendar year fixed effects (not reported in table). Standard errors are clustered at the province-start year level with significance at the 1%, 5% and 10% denoted by ***, ** and *, respectively.

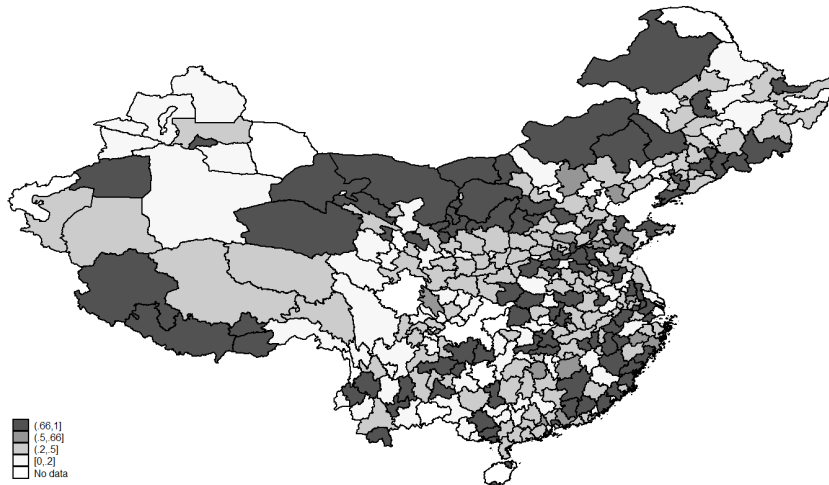
1.B Figures

Figure 1.B.1 – Competitors



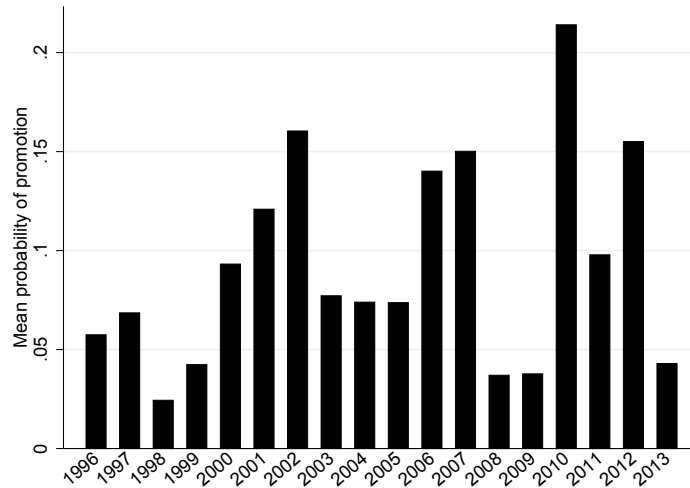
Note: This graph shows the distribution of our measure of the number of competitors in our sample. This measure is defined as the number of other CCP secretaries from the same province as a given CCP secretary, who start their term in the same year. The data are from our database on 1310 terms of CCP prefecture-level secretaries.

Figure 1.B.2 – Average promotion rate, by prefecture



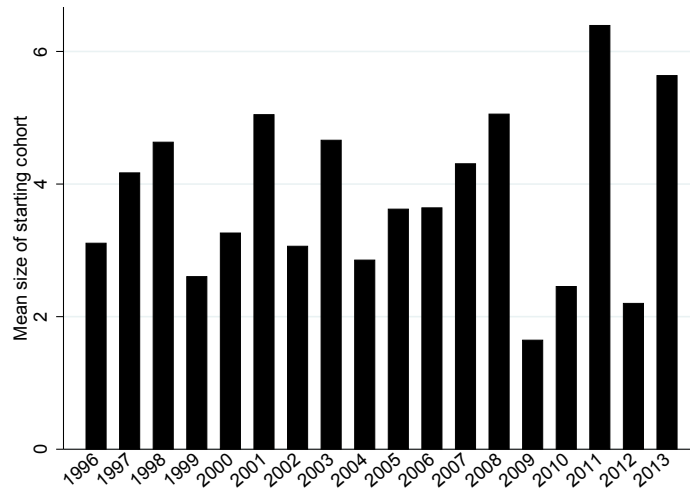
Note: This map shows the average promotion probability of CCP secretaries for each prefecture of China. The color of a prefecture corresponds to the average unconditional promotion rate of the leader in each prefecture. This is driven by the conditional promotion rate (conditional on the term coming to an end), and the average length of term by prefecture. The data are from our database on 1310 terms of CCP prefecture-level secretaries.

Figure 1.B.3 – Average promotion rate, by year



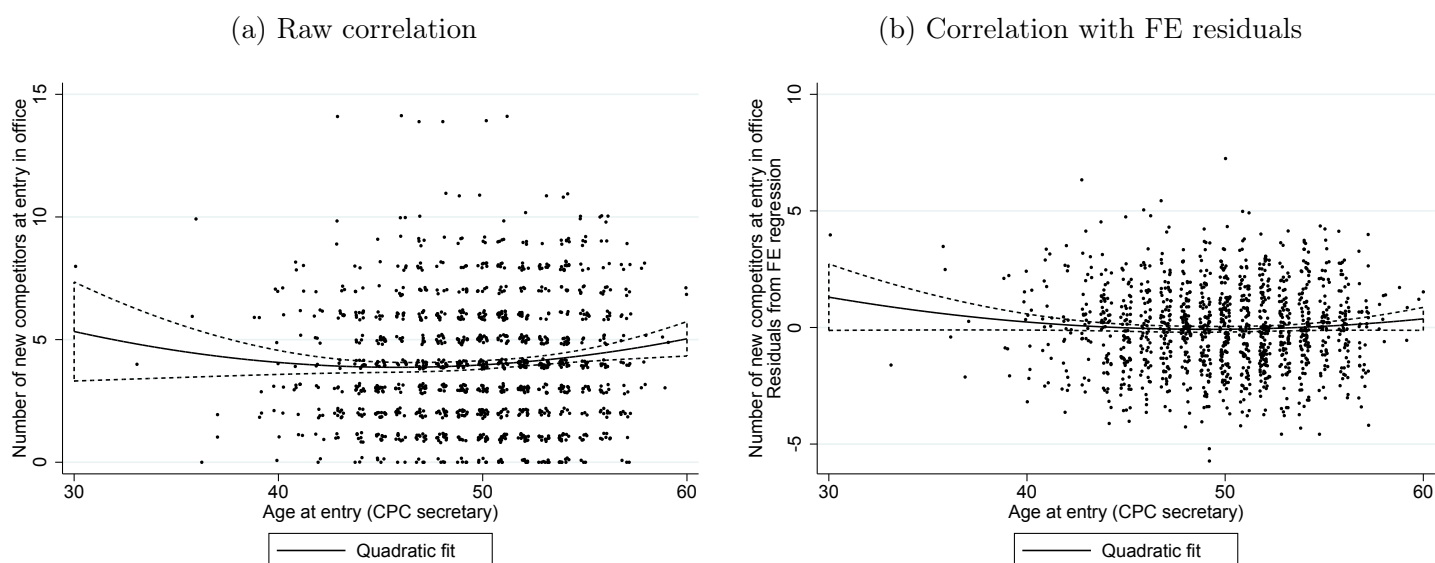
Note: This graph shows the mean probability of being promoted in any year of our sample. This is the unconditional probability of being promoted (the intersection of one's term ending and this term ending by a promotion). The data are from our database on 1310 terms of CCP prefecture-level secretaries.

Figure 1.B.4 – Size of cohorts of competitors



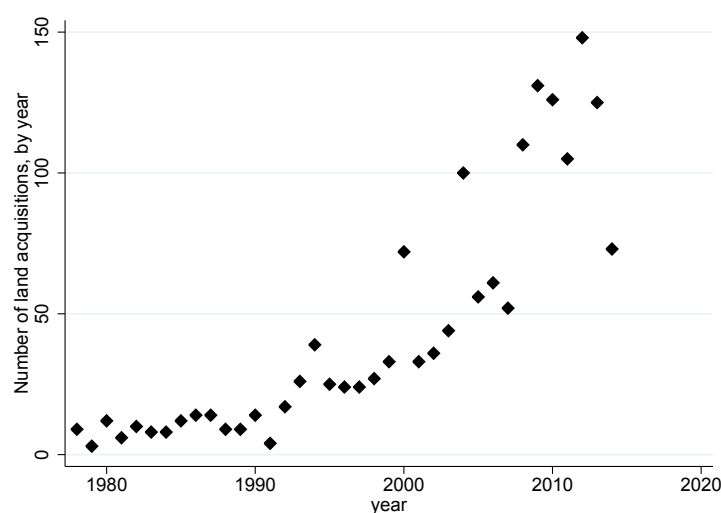
Note: This graph shows the average value of our measure of the number of competitors by starting year, or the mean size of provincial cohorts, by year in which those cohorts start their term. This measure is defined as the number of other CCP secretaries from the same province as a given CCP secretary, who start their term in the same year. The data are from our database on 1310 terms of CCP prefecture-level secretaries.

Figure 1.B.5 – Correlation of the number of competitors with CCP secretary’s age



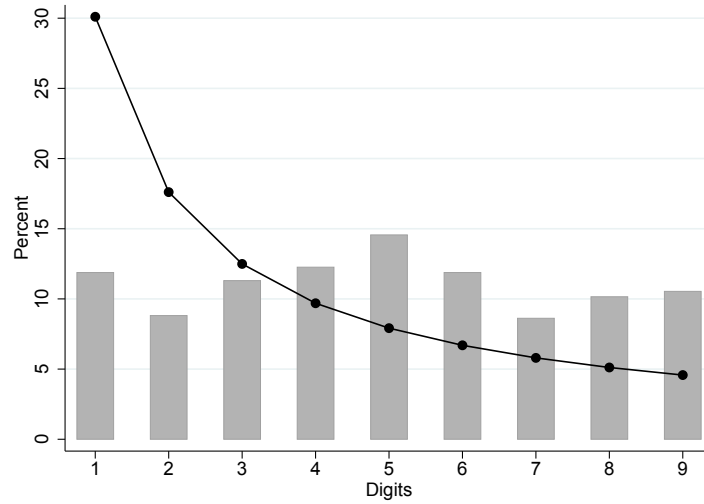
Note: These graphs plot the value of the size of the starting cohort, against the age of the CCP secretary when he takes office. The size of the starting cohort is the number of other prefecture-level CCP secretaries who start their term in the province in which a given prefecture CCP secretary starts his term. Panel (a) plots the raw correlation and a quadratic fit. Panel (b) plots the Residuals of the starting cohort size regressed on prefecture-level and starting-year-level fixed effects. The data are from our database on 1310 terms of CCP prefecture-level secretaries.

Figure 1.B.6 – Land seizures, CHARLS survey



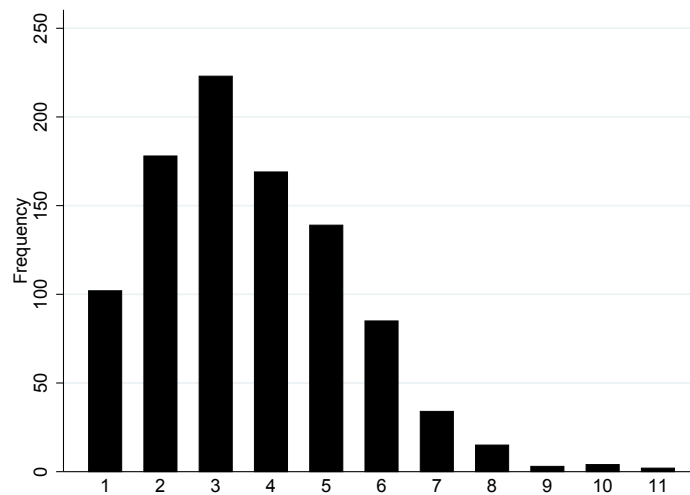
Note: This graph plots the absolute number of expropriation events that are recorded for each calendar year. In our analysis, we restrict the data to the 1996-2014 period, which is the period where most expropriations occur, and for which we collect CCP secretaries career data. The data come from the 2014 Life History wave of the CHARLS survey.

Figure 1.B.7 – Second decimal digit distribution



Note: This graph plots the actual distribution (grey bars) of the second decimal digits in reported real GDP growth rates; and the theoretical distribution (black connected dots) one should observe if said distribution followed Benford's law. The underlying data come from the NBS City Statistical Yearbooks, 1996-2013.

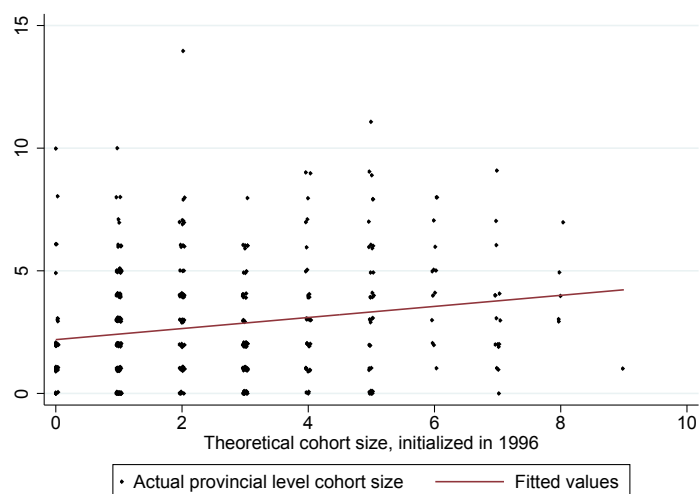
Figure 1.B.8 – Distribution of term lengths



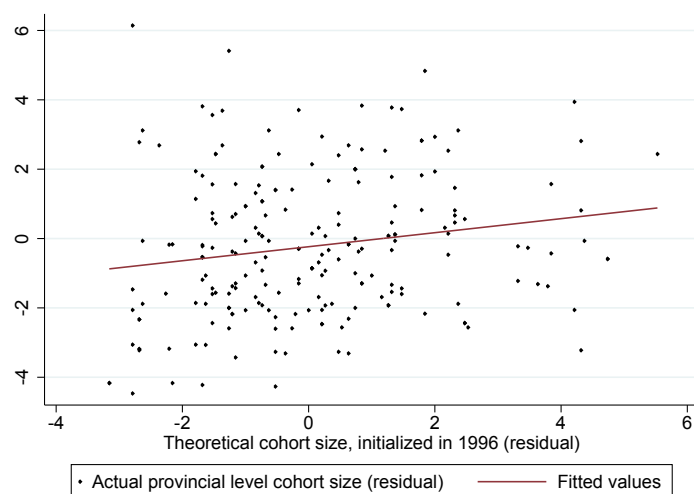
Note: This graph shows the distribution of term lengths in our sample. The length of a term is equal to the number of consecutive years at the end of which a bureaucrat is observed as being in office in a given prefecture. The theoretical length of a term is therefore 5 in this graph. The data are from our database on 1310 terms of CCP prefecture-level secretaries.

Figure 1.B.9 – Correlation between theoretical and actual starting cohort size

(a) Raw correlation



(b) Correlation of residuals



Note: These graphs plot the value of the actual size of the starting cohort (an observation is thus a province \times year), against the theoretical value of the starting cohort size, if starting 1996, all terms had been exactly 5 years long (as per the theoretical administrative rule). The size of the starting cohort is the number of other prefecture-level CCP secretaries who start their term in a given province in a given year. Panel (a) plots the raw correlation and a linear fit. Panel (b) plots the Residuals of the (actual and theoretical) starting cohort sizes regressed on province-level and starting-year-level fixed effects. The data are from our database on 1310 terms of CCP prefecture-level secretaries.

1.C Model

In this appendix we propose a simple formal model to rationalize the CPC secretaries' promotion incentives.

Suppose there are N bureaucrats competing for a given position. Obtaining the position yields a utility S to the bureaucrat i who does so. Each bureaucrat spends his total available effort e in, $e_{1,i}$ and $e_{2,i}$, with associated costs $c_1(e_{1,i})$ and $c_2(e_{2,i})$. e_1 enters the determination of the contest for the position, while e_2 does not.

The likelihood that bureaucrat i obtains the position is given by the following contest success function (CSF):

$$p_i = \frac{e_{1,i}}{\sum_{k \in [0, N]} e_{1,k}} \quad (1.6)$$

This is similar to the framework initially proposed by (Tullock, 1980) and further generalized by (Pérez-Castrillo and Verdier, 1992). In a principal-agent setting like ours, where higher-ranked bureaucrats evaluate lower-ranked ones and decide upon their careers, this functional form translates the fact the principal values the effort of the agent, but that said effort is only imperfectly measured by $e_{1,i}$, for instance.

Meanwhile, $e_{2,i}$ yields a private benefit to the bureaucrat, $B(e_{2,i})$. Importantly, effort e_2 does not come into consideration in the contest. In a Bengtstrom-Milgrom type of framework, this would be an extreme case where by design, one type of effort is not observed by the principal, here not because there is asymmetry of information or verifiability of effort e_2 , but because the principal only cares for e_1 .

Bureaucrats are risk-neutral.

The bureaucrat maximizes:

$$S * p_i(e_{1,i}, E_{1,-i}) + B(e_{2,i}) - c_1(e_{1,i}) - c_2(e_{2,i})$$

with regards to $e_{1,i}$ and $e_{2,i}$ and under the constraint that:

$$e_{1,i} + e_{2,i} \leq e_i = e$$

this program simplifies to maximizing:

$$S * p_i(e_{1,i}, E_{1,-i}) + B(e - e_{1,i}) - c_1(e_{1,i}) - c_2(e - e_{1,i})$$

The first order conditions yield

$$\frac{\partial p_i(e_{1,i}, E_{1,-i})}{\partial e_{1,i}} = \frac{\partial c_1(e_{1,i})}{\partial e_{1,i}} + \frac{\partial B(e - e_{1,i})}{\partial e_{1,i}} - \frac{\partial c_2(e - e_{1,i})}{\partial e_{1,i}}$$

which we simplify to:

$$\frac{\partial p_i(e_{1,i}, E_{1,-i})}{\partial e_{1,i}} = \frac{\partial \phi(e_{1,i})}{\partial e_{1,i}} \quad (1.7)$$

where ϕ represents the costs of effort e_1 net of the opportunity cost of not providing $e_2 = e - e_1$. Equation (1.7) defines a “best response curve” to the vector of $N - 1$ strategies of the other bureaucrats, $E_{1,-i}$.

A symmetric equilibrium ($e_{1,j} = e_{1,k} = e_1, \quad \forall j \neq k$) exists for this game, whereby:

$$\begin{aligned} \frac{\partial p_i(e_1, E_1)}{\partial e_1} &= \frac{\partial \phi(e_1)}{\partial e_1} \\ \frac{(e_1 + E_1) - e_1}{(e_{1,i} + E_1)^2} &= \frac{\phi'(e_1)}{S} \\ \frac{(N - 1)e_1}{N^2 e_1^2} &= \frac{\phi'(e_1)}{S} \\ e_1 \phi'(e_1) &= \frac{N - 1}{N^2} S \end{aligned} \quad (1.8)$$

One can show (as in Pérez-Castrillo and Verdier, 1992) that non-symmetric equilibria

do not exist if all agents have the same costs and valuations.

Equation (1.8) calls for a few comments. The right side of the equation is decreasing in N the number of competitors. This means that the equilibrium level of contest-related effort will be a decreasing or increasing function of N depending on the shape of $f(e) = e \times \phi'(e)$

Recall that $\phi = c_1 - (c_2 - B)$. Thus ϕ' is increasing when the marginal costs of contest-relevant effort increases faster than the marginal costs of private costs, net of marginal private benefits. This is likely to be the case when the levels of contest-relevant effort are high. This is the case when the benefits expected from the contest, i.e. the utility associated to promotion S , is large with regards to private utility. In our context, where bureaucrats typically spend their entire career in the Party or in the administration, we think of S as being large.

As a result, the equilibrium level of effort e_1 is a decreasing function of N : bureaucrats when facing many competitors are discouraged from providing promotion-relevant effort, and substitute for private effort. Note that how much substitution is actually taking place may be quite limited, depending on the marginal net private benefit $B - c_2$.

Finally, since equilibria are symmetrical, equilibrium promotion probabilities are equal to:

$$\begin{aligned} p_i &= \frac{e_{1,i}}{\sum_{k \in [0, N]} e_{1,k}} \\ &= \frac{1}{N} \end{aligned} \tag{1.9}$$

Thus promotion probabilities are also a decreasing function of the number competitors. This is a prediction that we test empirically.

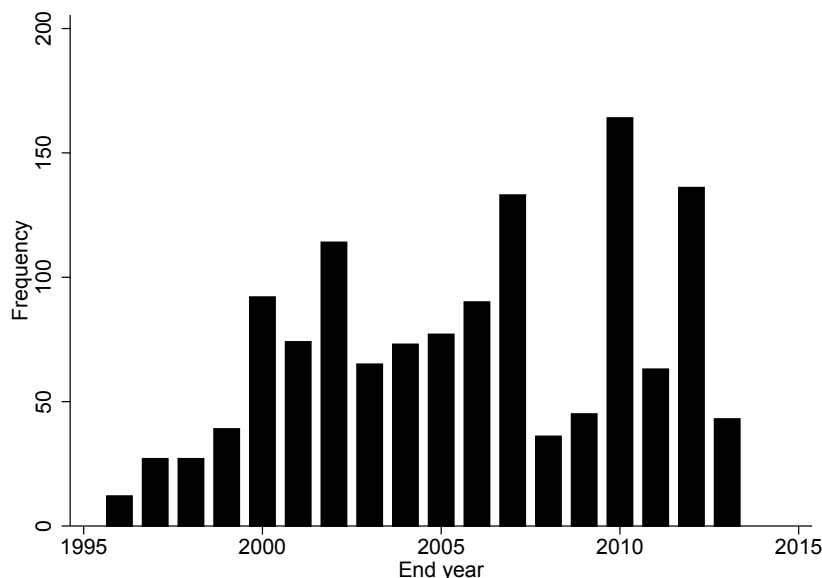
1.D Promotions and movements within the CCP hierarchy

We give in this appendix more details on how we treat data on the career of CCP officials. An important factor to consider is the length of terms. Part of the variation in term length comes from spatial variation. Some provinces have on average statistically significantly longer terms than others. This could be due to several province-specific characteristics, such as the number of sub-provincial units, or the political weight of provinces. These are likely to be correlated with the population of provinces, and with their level of economic development. Table 1.A.2, column 1, displays the average length terms of prefecture CCP secretaries in our sample, by province. Furthermore, some years are likely to see more career moves than others, due to country-wide political events. Figure 1.D.1 displays the distribution of end years for the terms considered in our sample. Years 1997, 2002, 2007 and 2012 correspond to the plenary sessions of the People's Assembly, which explains why greater exit rates should be observed. Remarkably, there is both numerous exits in-between, and substantial variations across years.

We measure promotions in the following way: for each bureaucrat's term, we gather data on his career path in the years following his term. We define a time window relative to the end of the term, and collect information on the positions in which the bureaucrat takes office within this time frame. We then attribute ranks to these positions based on official rules. We assign to the bureaucrat the highest rank he reaches within two years of the end of his term as CCP secretary. Importantly, we make a distinction between sub-provincial and regular prefecture-level units. Sub-provincial cities rank one level higher than regular units, so that a 'transfer' from a regular prefecture-level unit to a subprovincial city is coded as a promotion. While subprovincial status is time-varying, all subprovincial cities reach that status prior to 1996, the beginning of our data on secretaries' careers.³¹

³¹Because of the diversity of positions, it is difficult to assign a rank to all, but for almost all secretaries where we collect career information, we are able to correctly rank at least one position.

Figure 1.D.1 – End years of terms



Note: This graph shows the distribution of term ends in our sample. The end date of a term is equal to the last year at the end of which a bureaucrat is observed in office. The data are from our database on 1310 terms of CCP prefecture-level secretaries.

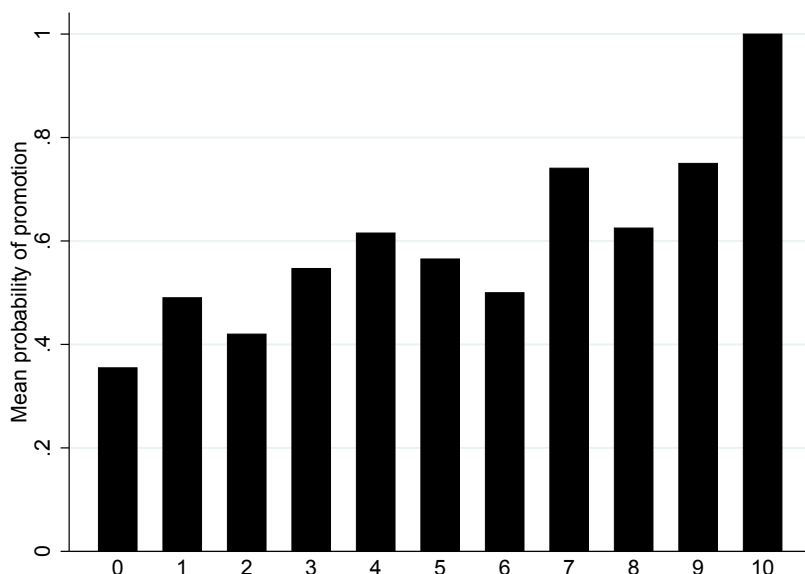
Many of the transfers to positions at the end of terms are in fact transfers to ‘retirement’ positions, that are official positions that wield little to no political power, that are still remunerated, and, in the case of many, officially ranked higher than prefecture-level party secretaries. In order to deal with this problem, we use qualitative data from media sources to identify such positions, and re-code transfers to such positions as transfers or retirement, rather than promotions.³² Note that we follow a similar path to Li and Zhou (2005) in making this distinction, although at a different level of the bureaucratic hierarchy. (Li and Zhou (2005) focus on provincial-level secretaries, while we focus on prefectural-level secretaries.)³³

³²The positions that we disregard as being promotions are, at the national-level, provincial-level and prefectural-level, (i) positions pertaining to the Chinese People’s Political Consultative Conference, often abbreviated as CPPCC, or in Chinese, 政治协商会议委员会, abbreviated 政协, and (ii) positions pertaining to the Standing Committee of the People’s Congress, or in Chinese, 人民代表大会常务委员会, abbreviated 人大.

³³Typically, provincial secretaries do not retire in the full sense immediately after leaving office unless there are exceptional circumstances, such as poor health. Before retiring officially, they are often assigned, as a transition, to an honorary yet virtually powerless position, such as director of the provincial People’s Congress or chairman of the provincial People’s Political Consultative Conference. Some are given honorary positions on the National People’s Congress or the National People’s Political Consultative Conference. Whatever gloss may be put on it, the loss of power is what marks the end of an official’s

The consequences of such a re-coding strategy are dramatic. Figure 1.D.2 shows the average promotion rate of CCP secretaries as a function of the length of their term. The promotion rate seems to be a monotonic increasing function of a term's length, which suggests that seniority unequivocally pays off.

Figure 1.D.2 – Promotions and term length



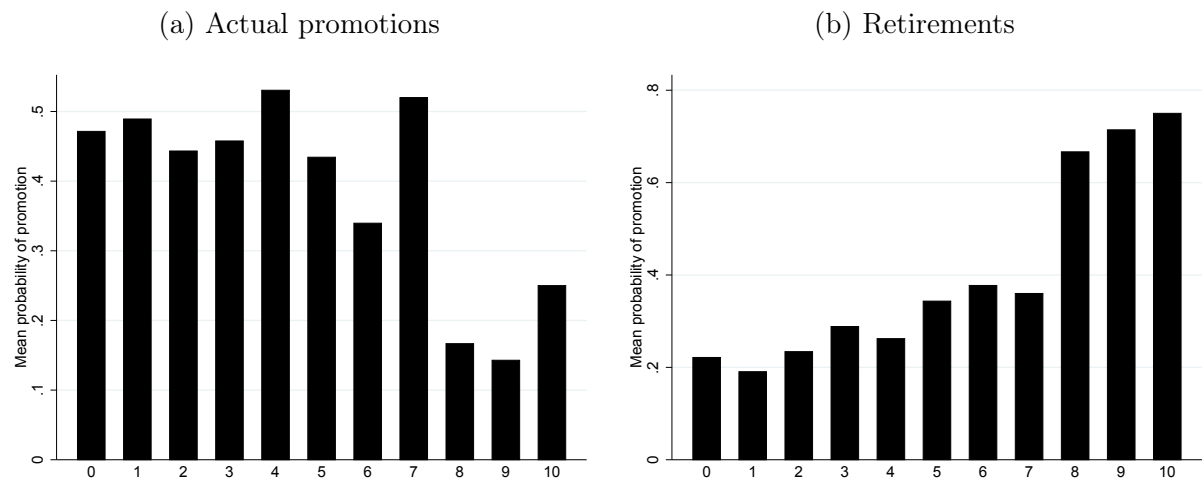
Note: This graph shows the mean promotion probability by values of term length in our sample. Promotions in this graph are based on official rankings from the CCP hierarchy, and mix movements to retirement positions that are officially higher ranked, and actual promotions. The data are from our database on 958 terms of CCP prefecture-level secretaries for which we have promotion information.

This is at odds with prior knowledge regarding the optimal career path of a CCP official: ‘sprinting with small steps’, that is, a succession of relatively short terms at each level of the CCP hierarchy, is often seen as the only way to reach the highest public offices. Figures 1.D.3a and 1.D.3b decompose Figure 1.D.2 according to the retirement-non-retirement criterion. Actual promotion probability increases slightly over the first years of a term and decreases sharply after the 5th year, while retirement probability steadily increases over the years of a term. This highlights the potential pitfalls from

political career. Therefore, in this paper, we define retirement as any departure from secretaryship or governorship that is not followed by a lateral move or promotion. Although demotion is different from retirement, we have not been able to identify many demotions in the data. Publicly announced demotions were rare and a seemingly routine retirement may well disguise a dismissal. Because of the subtle difference between retirement and demotion, we group them together and call both terminations.”

considering any transfer to an officially higher rank as a promotion. As a consequence, in the remainder of our analysis, we will disregard ‘retirement’ promotions and focus entirely on ‘actual’ promotions.³⁴

Figure 1.D.3 – Actual promotions, retirements, and term length



Note: This graph shows the mean promotion probability by values of term length in our sample. Promotions in this graph are based on official rankings from the CCP hierarchy, and distinguish across movements to retirement positions that are officially higher ranked, and actual promotions. The data are from our database on 958 terms of CCP prefecture-level secretaries for which we have promotion information.

³⁴Numerous instances of this expression -小步快跑, xiao bu kuai pao- can be found in media sources; see for instance *Nanfang ribao*, 15 July 2011.

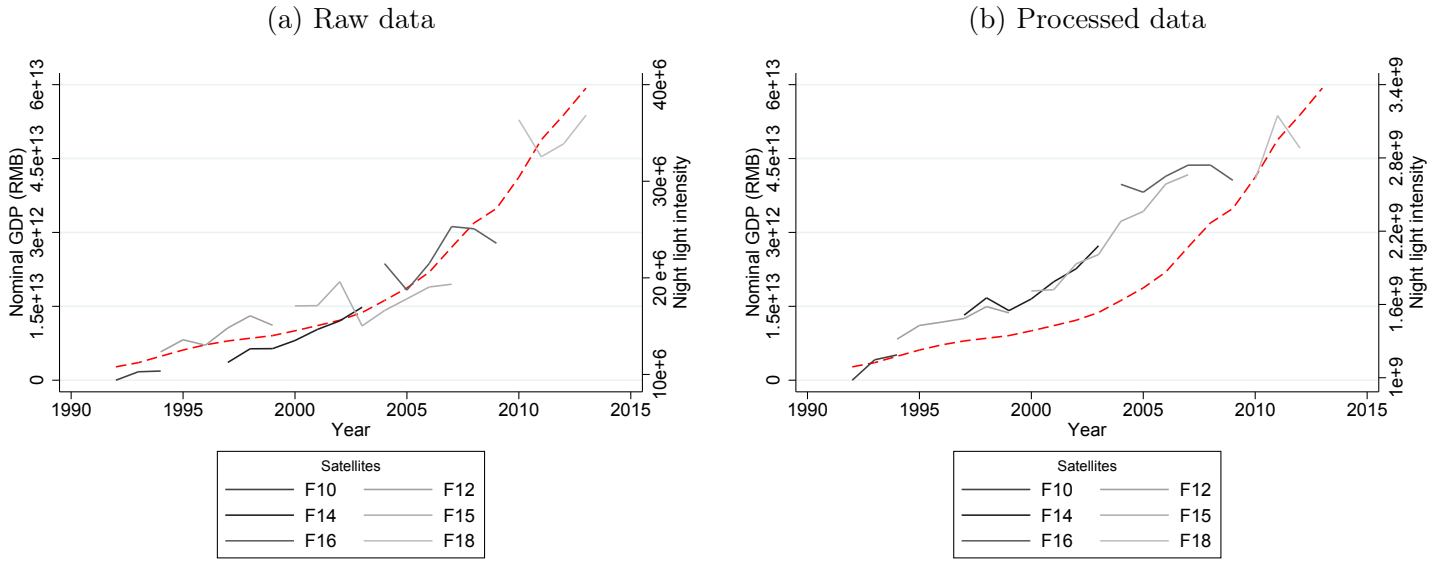
1.E Measurement of GDP growth using satellite data

The measurement of economic development via remote sensing imagery has become a standard toolbox of applied studies since the seminal work of Henderson, Storeygard and Weil (2012). Satellite imagery has features that considerably widens the scope of possibilities in many research areas, as discussed by Donaldson and Storeygard (2016).

Using measures based on satellite data as a dependent variable raises however issues that are often particular to a specific source. This is the case for nightlight data as a proxy for economic development, as highlighted in Zhang and Seto (2013). In this appendix we present a simple comparison based on raw data of satellite intensity from the DMSP/OLS, and nominal GDP figures from the NBS. This reproduces for China the exercise of Zhang, Pandey and Seto (2016). As shown in Panel (a) of Figure 1.E.1, the DMSP/OLS data comes in different series of data that partially overlap. The absolute levels of night light intensity (on the left axis) for a given year are sometimes substantially different. This in itself is not necessarily problematic, but inevitably induces breaks in the data that could be wrongly interpreted for episodes of largely negative or largely positive growth -for instance in 1999-2000 (for the negative case) and 2000-2001 (for the positive case). Zhang, Pandey and Seto (2016) use machine learning algorithms that smooth the overall global data of nightlights on aggregate GDP, and obtain processed night light data, displayed in Panel (b). The interest of this procedure is less in the overall fit of the night light intensity data to the GDP data than in smoothing out differences across series of satellite data.

Aside from discrepancies in the levels of night light intensities recorded by the different satellite systems, the most problematic aspect of nightlight data in our context is that two satellite systems of different sensitivity yield year-on-year variations that sometimes go in opposite directions. In Panel (a) of Figure 1.E.2 this is the case for the F14 and F15 systems for the years 2002 and 2003, or the the F15 and F16 systems in 2005. The overall consistency of the processed data -over time and compared to aggregate GDP data- in

Figure 1.E.1 – Night lights and GDP levels

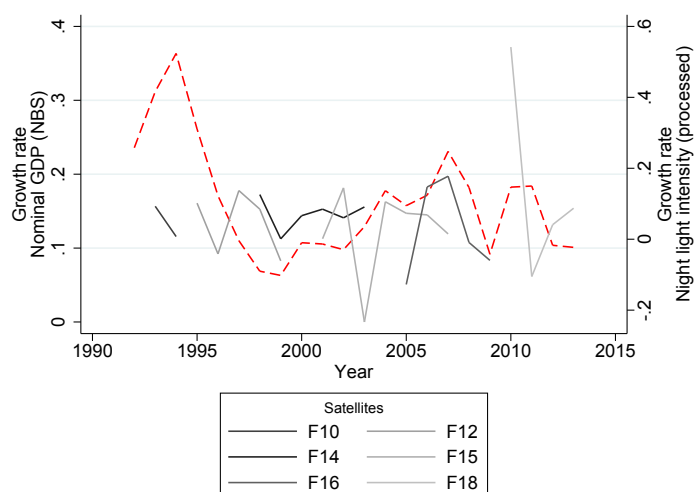


Note: These figures show the aggregate GDP of China in nominal terms (dashed red line, left axis), and the total night time light intensity from satellite data over China's territory (grey solid lines, right axis). GDP data are from the NBS, and night time light intensity data are from the DMSP/OLS (Panel (a)) or Zhang, Pandey and Seto (2016) (Panel (b)). The different solid grey lines correspond to different satellite systems.

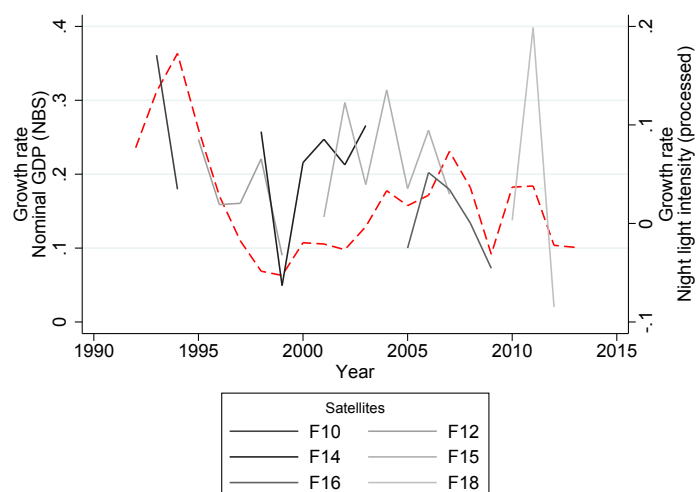
terms of growth rate (Panel (b) of Figure 1.E.2) seems better than that of the raw data, but worse than in levels. This invites us to caution when using night light growth as a proxy of GDP, and emphasized the need for location-specific and year-specific fixed effects, given the large temporal swings in the error term of the proxy.

Figure 1.E.2 – Growth of Night light intensity and Growth of GDP

(a) Raw data



(b) Processed data



Note: These figures show the nominal growth of aggregate Chinese GDP (dashed red line, left axis), and the yearly growth of total night time light intensity from satellite data over China's territory (grey solid lines, right axis). GDP data are from the NBS, and night time light intensity data are from the DMSP/OLS (Panel (a)) or (Zhang, Pandey and Seto, 2016) (Panel (b)). The different solid grey lines correspond to different satellite systems.

Chapter 2

Internal migration in China and the 2008 economic crisis

I study the impact of the 2008/2009 slowdown in the Chinese urban economy on the temporary internal migration patterns of rural households. I leverage the exposition of destination prefectures of rural migrants in terms of exports in the manufacturing sector as a source of variation, and focus on disentangling the income and substitution effects that the economic shock at destination has on migrant households. I find that individuals who were temporary migrant workers in 2008 actually migrate more in 2009 as a consequence of the shock, a fact that is compatible with an important negative income effect. This effect is heterogeneous, as marginal migrants react strongly to the shock, while the migration probability of typical, young male migrants, stays stable. Among the other members of the rural households of these 2008 migrant workers, the ex-ante most likely candidates to migration migrate less, and the least likely to migrate migrate more, a pattern that is also compatible with heterogeneous substitution effects. The shock spills over to non-migrant households, where the most likely candidates to migration are deterred from migrating by the magnitude of the shock in the primary destination of migrants from their village.

2.1 Introduction

Migrations, among many things, reallocate labour force across space. They alternatively transmit shocks to labour markets, and weather shocks that affect those markets. By moving from one place to another, migrants also convey information about markets, the one from which they come and the one to which they go. In China, where migration flows are numerous and repeated, these channels play a significant role in the livelihood of many. Shocks affecting the areas where migrants go are felt throughout the societies of sending areas.

In this paper, I address two questions: I first examine the sensitivity of the temporary migration decisions of Chinese internal migrants to a large adverse economic shock in their destination area. I investigate which, of the income effect or the substitution effect, is stronger for households of migrant workers that are directly faced with the shock. I distinguish between the reaction of household members who were migrant workers themselves, and other members of households in which a migrant worker faced an adverse shock. In a second step, I examine whether such shocks also impact the migration decisions of households who were not directly affected by them, because they were not migrating themselves: other households in the same location of origin, whose members did not migrate during the shock.

I use a panel survey dataset of rural households that spans the time period between 2007 and 2009, and I examine the impact of the 2008 financial crisis as a shock that hit urban labour markets. I match this detailed dataset to information on the destination prefectures of migrants in 2008. I leverage the ex-ante exposition of prefectures to foreign exports as a source of identifying variation.

I find that, despite the shock to urban areas, aggregate migration rates remain stable between 2008 and 2009. Migrant workers whose migration destinations were hit harder in the 2008 crisis, were actually more likely to migrate again in 2009, which is compatible with a strong income effect from the negative shock. This effect is driven by women

and migrants older than 30, or “marginal migrants”, who reacted to the shock by migrating more; while young male migrants did not. Shocks to migrants’ destinations also affected the subsequent migration decisions of individuals in their household, who had not migrated in 2008. Among the members in the households of migrants who were not migrating in 2008, the reaction in terms of migration also show substantial heterogeneity: more likely candidates to migration are deterred from migrating by the shock, while less likely candidates are driven to migrate more by the shock. This suggests a substitution effect that is stronger for individuals with typical characteristics of migrants, while the income effect is also felt by previously non-migrating members of migrant households. The substitution effect also affects households that are not directly concerned by the shock: adverse shocks in the destinations of the migrants from a given village deterred migrations from other individuals of the same village in previously non-migrating households; this effect is entirely concentrated on individuals who have migrant-like characteristics.

Thus, both in migrant households, who are affected by a negative income effect, and in non-migrant households, who are not, the individuals who are the most deterred from migrating by the negative shock at their migration destination are the ones whose characteristics are the closest to those of migrants (young males with intermediate education). This is in line with their potential earnings from migration being more detrimentally affected by the shock. In migrant households, in addition, the income effect drives the migration probability of all individuals to increase, which goes in the opposite direction to the substitution effect: marginal migrants are led to migrate more and most likely candidates to migration are left unaffected.

This paper speaks to several branches of the literature on migrations. How migration flows react to shocks is an empirical issue that has been shown to depend on various factors. Seminal work by (McKenzie, Theoharides and Yang, 2014) shows that international migration positively react to GDP fluctuations in destination areas. A growing literature has looked at the mobility of migrants, and their responsiveness to shocks at destination; most notably, (Cadena and Kovak, 2016) show that migrants, once at desti-

nation, remain more mobile than natives, and thus help to weather shocks. However these studies are specific to international migrations, often of entire households, which are key differences to our context. Closer to our setting, (Gröger, 2017) shows, using Vietnamese data, that substitution and income effects may play in opposite directions following an adverse economic shock in the destination countries of migrants: lower earnings at destination -whether because of lower wages or fewer jobs- make migration a less attractive option relative to the origin country of migrants. But the shock itself also decreases the total income of a household, so that if migration still entails higher expected earnings than working at home, albeit by a lesser margin than before the shock, then migration of other household members may increase to compensate for the loss in income. In the case of Vietnamese international migrants, income effect from an adverse economic shock at destination dominates, and increases the international migrations of household members, while decreasing domestic, short-distance migrations. This paper by contrast looks at differential shocks at destination *within* a country, in a context where internal migrations are ubiquitous and repeated.

This study also speaks to the small literature dealing with the intra-household dynamics of migration decision-making. Empirically, few studies have looked at the selection of who migrates within a household. (Chort and Senne, 2015) show that who migrates within the household depends on the relative gains from migration across household members, but also on the remittance potential of each household member. Using a lottery for migration in Tonga, (Gibson, McKenzie and Stillman, 2011) show evidence of positive selection into migration of adults within a household; whereby individual with higher earnings potential within the household are the ones who migrate. This also emphasizes how difficult the identification of the effect of migration on households in the presence of within-household selection into migration can be, as has been discussed and formalized in (Murard, 2019). The identification of the effect of the migration of one household member on the rest of the household requires strong assumptions on the constraints to migration of non-migrating household members -in addition to exogenous variation of the migration

probability of migrants. In this paper, I take the migration event and selection of 2008 as given and focus on the effect of shocks at destination both on migrant members and on the migration behaviour of household members in origin areas.

The role of networks in the economics of migration has been the object of a growing literature. Closest to our context is (Giulietti, Wahba and Zenou, 2018), who emphasize the role of weak ties -that is, outside of the family circle- in the migration behaviour of Chinese rural migrants, as opposed to strong ties, or family networks. This study takes the networks of migrants and their direct effects as given, and looks at how economic shocks may transit through them. In this regard, this paper also speaks to the literature on the effect of emigration on stayers. This literature has typically looked at the effect of out-migration flows on the economic outcomes of natives, mostly in an international context. The focus of these studies is generally on wages or employment: (Dustmann, Frattini and Rosso, 2015) for instance estimates the effect of emigration from Poland on Polish wages, by leveraging detailed information on emigrants by region of origin, and finds that negative labour supply shocks have positive effects on wages, concentrated on intermediate-skill stayers; and (Elsner, 2013) find, for Lithuanian natives, that emigration following EU enlargement has had positive impacts on domestic wages. Inasmuch as the 2008 economic crisis in the manufacturing sector of destination areas induced even greater migrations from sending areas, this could have an impact on local markets and thus impact non-migrant households. The focus of the present study is the reaction of said households in terms of their own migration behaviour.

Finally, Chinese migrations have also been extensively studied. Rather than the permanent reallocation of households across the territory, migrant flows in China are characterized by rural households sending one or several individuals to urban labour markets for a period of time. These individuals are employed in the industrial, labour-intensive sector of urban economies, and accumulate or send back their labour earnings to their households, who remain in the country and are engaged in relatively low productivity agricultural production, while non-agricultural self-employment in rural areas acts as a

buffer for individuals coming back from migration (Démurger and Xu (2011)). The consequences of the global crisis have drawn the attention of (Huang et al., 2011), who study the consequences of the crisis in terms of off-farm employment; and find that despite massive layoffs between October 2008 and Spring 2009, off-farm employment recovered quickly. One interpretation they suggest for this recovery is the shift towards the construction and services sectors, away from the traditional manufacturing sector, driven by a government-led macroeconomic stimulus in 2009. A major difference with this study is the focus on geographical reallocation and migration pattern of the individuals who were migrant workers in 2008, while (Huang et al., 2011) focuses on off-farm employment dynamics. An estimate of the total impact of the crisis in terms of internal migration rate has been suggested in (Kong, Meng and Zhang, 2010); in this paper I analyze the drivers behind this overall evolution, and I look at the aftermath of the 2008-2009 crisis on migrations during 2009 rather than 2008.

This paper is structured as follows. Section 2.2 presents the migration data and highlights both the resilience of migration rates through the crisis, and the important features of internal rural-to-urban migration in China; it then describes the macroeconomic data that is used to capture the 2008-2009 crisis. Section 2.3 details the empirical strategies that I use to look at the effect of the 2008-2009 crisis on different subsamples of the rural population. Sections 2.4 and 2.5 presents the results and some robustness checks, and Section 2.6 concludes.

2.2 Data and descriptive statistics

2.2.1 Migration in the rural RUMiC dataset

I use survey data from the Rural-Urban Migrant in China (RUMiC) dataset. This is a panel dataset following households and implemented in 2008, 2009 and 2010. I focus on the 2009 and 2010 waves of the survey, that ask retrospective questions on the years 2008 and 2009. The RUMiC survey is implemented shortly after the Chinese Spring Festival,

which takes place in January or February of each calendar year, and includes questions concerning the preceding calendar year.

The data are from the rural arm of the RUMiC survey. The rural part of the RUMiC survey follows 8000 households, or about 33000 individuals for 3 years; drawn from 69 prefectures and 82 villages, of 9 provinces in China. Household-level attrition is limited to 4 out of 8000 households, individual-level attrition is somewhat more pronounced. It is largely accounted for by marriages, adult children leaving the household to settle and deaths. Importantly, the sampling frame does not account for long-term migrant households who may have moved permanently and entirely to urban areas. Long-term migrants whose household is still living at their place of registration in rural areas should be accounted for. A migration rate of 19.2 percent in the rural working age population for the year 2008, as measured in the sample, corresponds to a total migrant population of 90 million individuals based on the 2010 Census figure of 469 million working age rural individuals. This is a substantial share of the 153 million migrant rural workers officially reported by the National Bureau of Statistics of China (NBS, 2011), which uses a wider definition of migrant workers.

Migrants are defined in this paper as working age adults who declare spending three months or more outside of their township, for work reasons, over a period of one year.¹ The migration events that I consider are also restricted to movements to urban areas, whether within their province of registration or outside. Short-distance migrations to the local county seat are excluded. Because detailed data on the destination are only available for migration spells that last longer than 3 months, shorter-term spells are not considered as migrations.²³

¹This corresponds to the period between two Chinese Spring festivals - in this case, February 18th, 2007, February 7th, 2008 and January 26th, 2009.

²In comparison, official numbers of the National Bureau of Statistics consider since the 2010 Census a period of 6 months or more, and aggregate numbers generally include migrants who stay within their township; including these very-short distance in migration numbers increases the number of migrants from 153 to 221 million in 2010. The NBS definition also relies on an administrative difference between the registered residence of individuals and where they actually live, and this aggregate number also does not distinguish rural-to-urban movements from others, such as rural-to-rural and urban-to-urban.

³The precise wording of the questions, translated into English, reads 'How many months did you live outside local countryside in 2008?', followed by 'If you have lived outside the household more than 3

The migration rates for the years 2008 and 2009 for all adults aged 16 to 64 are 19.2 and 19.6 percent, but this difference is not statistically significant at conventional levels (see Table 2.A.1). It is possible for all years to distinguish between individuals migrating within their province, and those crossing provincial boundaries to migrate. The within-province migration rate increased from 7.6 percent to 8.4 percent from 2008 to 2009, and this increase is statistically significant at the 1% level. The across-provincial migration rate was more stable and decreased slightly, from 11.5 percent to 11.3 percent from 2008 to 2009. This pattern is also true of the part of the population who exhibit the highest yearly migration rates. Men between 20 and 30 are the most numerous of Chinese rural-to-urban migrants. Their yearly migration rate is 45.6 percent in 2008 and 47.3 percent in 2009, and all of this increase comes from within province migrations (see Table 2.A.2).

This overall stability or even increase of migration rates between 2008 and 2009 is surprising given the magnitude of the slowdown that hit urban labour markets at the end of 2008. Classical models of migration decisions, since (Sjaastad, 1962) emphasize that the choice to migrate is driven by positive expected monetary gains from migration. A deterioration of economic conditions at destination would intuitively reduce the relative expected gains from migration and therefore decrease migration rates on aggregate. This however does not account for the income effect induced by such a drop in earnings from migration. If the negative income effect is strong for migrant households faced with an economic shock, then the migration probability of the members of these households can increase. This would be the case if the relative expected earnings from migrating are still higher than those from staying home, despite being lower than before the shock.⁴ In contrast, for households that did not have a migrant in 2008, their choice not to migrate in 2008 reveals that their relative expected gains from migrating are already low before the

months in 2008 [2007/2009], where did you mainly live?', where the possible answers are 'Rural area in local county', 'Rural area in other county in the same province', 'Rural area in other province', 'In the local county seat', 'In city/other county of local province', 'In city/other county of other province'. For the 2009 wave of the survey, regarding migrations during 2008, two additional answer items were required for this question, regarding the province and the prefecture or county of destination. These items are not available in the 2008 nor in the 2010 questionnaire. The surveys were carried out starting February and into early April.

⁴This is the mechanism described by Gröger (2017).

crisis (if not negative). For these households we would expect that the substitution effect would be stronger than the income effect.⁵ This is why we should expect different reactions to economic shocks for these two types of households. These differences across households are likely to be partly a reflection of differences in their composition: differences across individuals in terms of individual characteristics are likely to drive the magnitude of the macroeconomic shock's effect on each individual's relative gains to migration. This is why it is particularly relevant to analyze the reaction to the macroeconomic shock of individuals with migrant-like characteristics, who yet were not migrant workers in 2008.

This stability in migration rates is also at odds with the findings of (Kong, Meng and Zhang, 2010). By comparing migration rates from the 2009 and 2008 waves of the RUMiC survey, that are retrospective for the years 2007 and 2008, the authors conclude to a decrease of the migration rate as a consequence of the economic downturn of the crisis of late 2008. While this decrease is puzzling, I show in the Section 2.4.1 that this decrease is unlikely to be related to the export-driven crisis of the end of 2008, since the length of migrations of migrants is not affected by the shock. Since the questionnaire items on migrations also include very short-distance migrations (such as to the local county seat, or to other rural areas), it is possible that this is driving the discrepancy in the estimation of the impact in terms of migration rates. It is likely that focusing on the 2007-2008 time frame is unable to capture the full effect of the crisis, whose onset started in the autumn of 2008. In contrast, this paper's findings of stable migration rates between 2008 and 2009 are in line with finding by (Huang et al., 2011), who find that the recovery in terms of off-farm employment was quick, and that half of all the off-farm workers who had been laid off between October 2008 and March 2009 had found a job back by April, and nearly all of them by August of the same year.

Finally, a caveat must be made regarding how representative this increase in migration rates is with respect to the entire population of migrants in China. Indeed, the rural arm of the RUMiC survey does not account for rural households that have moved altogether

⁵Note that for non-migrant households, the income effect from a negative shock at potential destination is not realized but only counterfactual.

to urban areas, and only focusses on households of which part of the members may spend some time outside of the household’s registration area during the year. In comparison, migrant surveys, collected in cities, have been used to examine the impact of the shock on migrant workers (see, for instance Kong, Meng and Zhang, 2010). These data show that the migrant workers surveyed in their urban residence tend to stay for a long time in their destination area.⁶ By contrast, data on migrants emerging from rural household surveys shows that 23 percent of people declaring to have migrated for work for a period of more than 3 months in 2009 among the rural households of the RUMiC survey had not migrated the year before; and of those, 40 percent had migrated the year before that.⁷ This suggests a substantial turnover of workers in and out of migration on a yearly basis; and that on the extensive margin, migratory flows that react in potentially different ways to economic shocks than those of longer-term migrant households.

Rural-to-urban migrant flows in China warrant three important observations that are relevant for the empirical strategy of this paper. First, national-level dynamics in migration rates hide large variation at the provincial and prefecture level. Migration rates out of the 69 survey prefectures of the sample range from 1 percent and 60 percent.⁸ This is compatible with substantial heterogeneity in the economic conditions at the location of origin of rural households. Push-factors have been shown to matter a lot for migration, including in the Chinese context (for a recent example of a strategy exploiting these push-factors, see Imbert et al., 2018). This advocates for the inclusion of location-of-origin-specific fixed effects.

Second, migration rates are highly correlated across years. Yet individual trajectories show that there are large gross flows into and out of the migrant status. Table 2.A.1

⁶If only as a lower bound, only 15 percent of migrants interrogated in the migrant arm of the CHIP survey in early 2008 declare having been back in their home village for a continuous duration of at least three months ever since their first departure from home.

⁷The *hukou* status of the individuals in the rural part of the RUMiC dataset is at 95 percent local and rural, 4.5 percent local and urban and the remaining non-local hukous being largely the consequences of familial re-compositions. The presence of urban *hukou* holders in rural areas can be due to familial re-compositions such as weddings.

⁸This figure is based on all working-age, valid and non-enrolled in school adults in 2007, 2008 and 2009.

shows that 24 percent of individuals migrating within their province and 23 percent of individuals migrating outside their province of origin in 2008 were not migrating the next year. Similarly, 27 percent of 2009 within-province migrants and 24 percent of 2009 out-of-province migrants were not migrating in 2008.⁹ These substantial gross flows (of new individuals starting to migrate and migrant workers putting an end to their migration spells) spur the question of the drivers of these entries and exits. In my empirical strategy, I look at what drives migrant workers to continue being migrant workers, and I look at what drives individuals who were previously not migrating to start migrating.

Third, the distribution of destinations is often skewed towards one particular destination. Figure 2.B.6 represents the share of a village's migrants' primary destination in the total migrant worker body of the village, as a function of the village's migration rate. The primary destination accounts for 40% of total village migration on average. Even in villages with total migration rates close to 50 percent, the primary destination generally accounts for at least 20 percent of migrants. Figure 2.B.5 represents the number of different destinations chosen by migrants of a same village as a function of the migration rate of the working age population; there is diversification in the destination choices as the migrant body becomes larger. This suggest to use as source of information on urban markets for candidates to migration the average shock at destination of the same migrants of one origin, weighted by the migration pattern from that origin.

2.2.2 Macroeconomic data

I complement the RUMiC survey data with prefecture-level data and firm-level censuses from the National Bureau of Statistics of China. The prefecture-level City Statistical

⁹One issue with these observations is that they are based on a migration status defined over the year preceding the survey, from one Chinese Spring Festival to another. It is possible for instance that an individual start her migration bout in November 2008 and comes back in June 2009; this would be observed as a transition from non-migrant to migrant, despite there being a period of time in 2008 where the individual is actually migrant. However, the duration limit for an individual to be considered a migrant is quite low, at 3 months, so that misclassifications would mostly obfuscate very short periods of overlap. There remains a risk of not measuring migrations of up to 6 months and equally split across the survey years' divide, against which there seems to be no relevant strategy. Given the general pattern of migrations in China, this risk is limited.

Yearbooks of the NBS follows a panel of 287 prefectures every year and collects information on production, industry, employment, and prefecture-level assets.

I show descriptive statistics for all prefectures in China and for the prefectures chosen as destinations in the RUMiC sample in Table 2.A.3. The average prefecture in China is about 4.3 million people large, with a 28,000 RMB nominal GDP per capita in 2008. The nominal GDP growth rate is stable in 2007 and in 2008 at 20 percent and 21 percent, and substantially drops in 2009 at 12 percent. Figure 2.B.4 shows that this drop in the growth rate of GDP per capita exhibits substantial variation across provinces.

Additionally, the NBS conducts a yearly census of all state-owned manufacturing enterprises and all non-state manufacturing firms with sales exceeding 5 million RMB or about \$600,000, that contains information on the firm's location, accounts, and exporting activity. While smaller firms are not represented in the census, these firms represent 90 percent of total gross output in the manufacturing sector. I use the 2007 wave of this census to construct a measure of prefectures' exposure to the downturn in global demand for Chinese goods in 2008-2009. This measure is equal to the share of exports in the total of sales of all manufacturing firms in the prefecture.

I show the distribution of this exposure measure in Figure 2.B.3. Most prefectures have at least some exporting activity in manufacturing (all the ones chosen as 2008 destinations by the migrant workers in the sample do). There is substantial variation in the exposure of prefectures to foreign trade across China, with an unweighted mean at 9 percent and a standard deviation of 0.11.

The export-driven shock that affects a prefecture is computed as the product of the exposure of a prefecture and the growth rate of exports in the province of the prefecture. Figure 2.B.2 displays the evolution of exports in a few provinces of China. There is substantial variation across provinces. In the analysis, some specifications allow for province-level fixed effects; in which case the identification relies on the sole within-province variation of the exposure measure. This variable relates closely to the spirit of shift-share instruments, as first introduced by (Bartik, 1991) and popularized by (Blanchard and

Katz, 1992) in the context of regional flows of populations. (Goldsmith-Pinkham, Sorkin and Swift, 2018) shows that in many cases, the assumptions necessary to identify the effect of such shock measures indeed refer to the ex-ante structure, rather than the multiplying growth factor.

A concern for the empirical strategy in this paper is the possibility to associate migration spells of migrants in the sample and the economic situation they faced. The survey identifies the prefecture or county to which each individual has moved. Among 4948 adult individuals who declare spending 3 months or more outside of their township in 2008, for work reasons, 14 individuals migrated to an unidentified destination, and 421 (or 8.5 percent) migration destinations are non-specified. The province of destination is known in 91 percent of cases, and the prefecture in 79 percent of cases. In cases where information is only available about the destination province (and not the prefecture), I assign the average value for the province to these observations.¹⁰ Some migrants also report their county of destination, rather than prefecture; I match the county declared to the corresponding prefecture. When this county does not correspond to any prefecture in the NBS data because of changes in the limits of prefectures, I match these counties to the nearest prefecture, using great-circle distance.

Importantly, the migration destination of migrant workers is not known with the same level of precision for the migration spells of either 2007 or 2009.¹¹ For these years, the only information available is whether the migration spell was directed towards destinations that are within the province of the migrant worker’s household residence, or outside of it. This does not allow for the analysis of more refined reallocation mechanisms, as I discuss in Section 2.4.3.

¹⁰This average is weighted mean, where the weights are the size of each prefecture in terms of nominal GDP.

¹¹The reason for this discrepancy is that the 2008 and 2010 RUMiC survey questionnaires differed from that of 2009.

2.3 Empirical strategy

We are interested in the effect of the 2008-2009 economic crisis on the migration decisions of rural households. I distinguish between two types of households, and three types of individuals. The first type of households are those within which at least one member was a migrant workers in 2008, which I call “migrant households”. Within migrant households, I distinguish members who were migrant workers in 2008 -and were thus directly exposed to the crisis- and members who were not. The second type of households are “non-migrant households”, that is, households in which no member was a migrant worker in 2008.

In order to capture the effect of the economic 2008/2009 downturn on migrant workers’ migration pattern, I estimate the following equation:

$$Y_i = \delta_{p_o} + \beta \mathbf{X}_i + \gamma_1 Z_{p(i,2008)} + \epsilon_i \quad (2.1)$$

Where Y_i is an outcome variable measured for individual i (in 2009), δ_{p_o} are fixed effects for the village of origin of migrant worker i (that is, the village where her household is answering the survey), X_i is a set of individual-level controls, and $Z_{p(i,2008)}$ are variables that are specific to the destination p of i in 2008. The variable of interest among those included in $Z_{p(i,2008)}$ is the measure of the export crisis at destination detailed previously, which is the product of the exposure to foreign trade of the manufacturing sector in prefecture p , by the 2009 growth rate of exports in the province of prefecture p . It is explicit here that p is a function of individual i : it is the prefecture in which i spent most of her time in 2008.

The identification of the coefficient γ_1 relies on the assumption that the error term ϵ_i has mean 0 conditional on the observable variables that are included in Equation (2.1). While reverse causality is unlikely here, since the shock was mostly driven by

a decrease in global demand, the variable $Z_{p(i,2008)}$ could capture the effect of omitted variables. To address this issue, I first include a relatively rich set of pre-determined individual characteristics as control, that are likely to explain migration decisions. I also include destination-specific macroeconomic controls, such as the 2008 level of GDP per capita, to account for the migration income of migrant i in 2008.¹² and 2009 growth of GDP per capita in destination $p(i,2008)$ to capture time variation in amenities level. Additionally, the overlap between the prefectures chosen as destination by migrant workers of different origins also allows for the inclusion of province of destination fixed effects (or even prefecture of destination fixed effects). These would control for destination-specific factors that could explain the propensity of individuals, who have migrated to these locations in 2008 to continue migrating in 2009, such as amenities, cultural proximity, etc. Since $Z_{p(i,2008)}$ is computed as the product of the 2009 growth of exports *at the province level* by the exposition of the *prefecture* to exports, controlling for province-level fixed effects essentially makes the identification rely entirely on the 2007 exposure of prefectures to foreign trade.

For individuals who have not migrated, I compute the economic shock to which they are exposed as the weighted average of the shock that migrant workers in their household have been exposed to. Each 2008 migrant worker in the household receives an equal weight of 1. Indeed, about half of migrant households have more than one migrant worker. However, there are relatively few cases where the migrant workers of the same household do not migrate to the same destination. The estimation equation becomes:

$$Y_i = \delta_{p_o} + \beta \mathbf{X}_i + \gamma_2 \bar{Z}_{p(hh,2008)} + \epsilon_i \quad (2.2)$$

¹²The income data that is available in the RUMiC does not account for within-household transfers, and does not include transfers from individuals outside of the household, so that data on remittances do not exist.

The identification of γ_2 comes from slightly assumptions than that of γ_1 . Z is this time computed at the level of households rather than individuals.¹³ The measure of the 2008-2009 shock that a given household member is exposed to must be exogenous to that household member's migration decision, conditional. Since I control for a relatively rich set of individual controls, for finely-grained origin fixed effects, the main risk here would come from correlations between the ex-ante exposure of the manufacturing industry of a give destination with other characteristics, for instance with other destination characteristics. It is possible to control for the province of destination of the 2008 migrants, which would capture important geographical factors, such as cultural proximity, distance and accessibility, as well as general amenities levels.

Finally, for households where no individual was a migrant worker in 2008, I investigate the effect of the shock in the destinations of migrants in the village on the 2009 migration decision of individuals. I use as a measure of exposure to the shock, the weighted average of the shock measure for all recorded destinations from migrants of the same origin, weighted by their respective shares in the migration destination distribution of migrants of said origin.¹⁴

The estimating equation becomes:

$$Y_i = \alpha_{\text{Prov}_0} + \beta \mathbf{X}_i + \gamma_3 Z_{p,2008} + \epsilon_i \quad (2.3)$$

Here, the identification of γ_3 cannot come from variations in the destination of migrants within the same origin. The vector Z is not a function of the individual (or of the household) anymore, but of her prefecture of origin: $Z_{p,2008}$ refers to the measure of the

¹³Note that in Eq. (2.1), migrants are only affected by the shock at their own migration destination rather than the household level shock. Since there are only few households for whom there are several destinations, $\bar{Z}_{p(hh),2008}$ and $Z_{p(i(hh),2008)}$ are equal for most non-migrant and migrant members of households

¹⁴Other destination-specific variables are handled in the same way.

export shock in the destinations of migrants from origin p ; this by definition is equal for all the non-migrant households of the same origin p . This implies that fixed effects for the prefecture of origin of non-migrants would capture the full variation in Z . This is an important caveat, as local push factors -such as agriculture-related shocks- are known to be important for the migration decision of rural migrants (see for instance Imbert et al., 2018). However it is difficult, without precise information on the connections between migrant households and non-migrant households from the same origin, to assign different shocks to the households of a same origin.

I address this issue in two ways. First, Equation (2.3) includes province-of-origin fixed effects, and includes as controls other variables that capture the attractiveness of the prefecture of origin, such as the 2009 per capita GDP, in level and growth rates. Second, I implement a different specification, putting the RUMiC sampling frame to my advantage. Several villages are sampled within each prefecture of the sampling frame. I replicate the analysis by allocating to each village its primary destination (or destinations). By doing this, prefecture-of-origin fixed effects can be added to the regression equation, and within-prefecture across-villages variability of the most important destination of migration identifies the relevant effect. This is a demanding strategy, as it only leverages information from sample prefectures where several villages are surveyed. This is unfortunately not the case for all prefectures in the RUMiC sample. Out of the 69 prefectures in the sample, only 12 have several villages surveyed. This second strategy therefore comes at the cost of a lot of statistical power, and results in a highly selected sample; I use it primarily for checking the robustness of the main strategy.

As shown in Figure 2.B.6, the share of migrant workers who go to the village's primary destination varies substantially. There are several ways in which this heterogeneity could play a role. First, the more important the primary destination is in the migration destination distribution of migrants, the stronger the informational effect of a shock affecting this destination, for the non-migrant households of the village. Second, a large share of the primary destination could be a sign of fewer outside options for the potential (and

actual) migrants of a given origin. This is captured by including, in Equation (2.3), the share of the primary destination in all migrations from the village, and an interaction term with the measure of the export shock at destination: the more migrants come from the primary destination of the village migrants, the stronger the expected effect of a shock at destination on other village households.

Characteristics of individuals such as age, education, gender, drive their relative potential earnings from migration: it is also likely to drive their reaction to economic shocks at destination. Examining the overall reaction of migrants (or non-migrants) to an economic shock at destination is likely to obfuscate important heterogeneity with respect to characteristics that determine how much an economic shock affects relative expected gains to migration. In order to summarize these characteristics in a single index, I regress information on the migrant status of individuals in the sample in 2007 and 2008 (prior to the crisis) on individual pre-determined and time-invariant characteristics. The results of this estimation are shown in Table 2.A.4. In China, typical rural migrants are young men aged 20 to 30, with intermediate education levels. I then use this estimation to predict, for each individual in the 2009 sample how close to a typical migrant this individual is. I take the predicted probability of migration in 2007 and 2008 to represent information on how large the expected relative gains from migrations are for a given individual. I classify individuals according to the quartiles of the distribution of the predicted migration probability of 2008 migrants. Individuals in ‘Q4’ are in the individuals with characteristics that predict the highest migration probabilities before the crisis, and individuals in ‘Q1’ the ones with characteristics that are the furthest from those of typical pre-crisis migrants.

2.4 Results

2.4.1 Migrants in migrant households

The economic downturn in China occurred at the end of the year 2008. Figure 2.B.1 shows that the onset of the period of negative growth in exports is November 2008. This confirms that the growth of exports from 2008 to 2009 is an appropriate proxy for the shocks migrants' destinations faced at the end of the 2008 migration spells. It also means that migrations in 2008 could have been affected by the crisis. Unfortunately, the yearly data does not contain information on the precise dates of departure or return of migrants, only migration lengths. One effect of the economic shock could have been to shorten migration spells in 2008. I therefore look at the effect of the shock at destination on the duration of migration in 2008 for 2008 migrants.

Table 2.A.5 displays the results. The economic shock does not seem to have had a precise nor substantial impact on the duration of migrations in 2008. Individual characteristics seem to be significantly correlated with the length of one's migration spell's length, but the difference in 2008 GDP per capita between the destination prefecture and the prefecture of origin of the migrant is not. One explanation for this, that is supported by anecdotal evidence from newspaper reports, is that migrant workers started coming back to their home in January-February 2009, but not before. This could be due to a delay between the time of the sudden decrease in foreign demand and the time production adjusted to this decrease, with the accumulation of stocks perhaps playing the role of buffer. Unfortunately, the RUMiC data on the job history of respondents is too partial to check whether layoffs took place between October 2008 and February 2009, as (Huang et al., 2011) are suggesting. It is possible however that laid-off migrant workers tried to find another job before going back home for the Spring Festival. In Table 2.A.6, I examine the effect of the export shock at destination on migrant and non-migrant income, in order to check whether the shock had indeed hit migrant households by the end of the year

2008. I aggregate data on non-agricultural income at the level of the household, because remittances between migrants and their families are known to depend on many aspects that are not the focus here, and would potentially make the analysis more difficult. The economic shock of 2008-2009 seems to have had a negative impact on the income of migrant households in 2008, although this effect is only marginally statistically significant. Its effect in 2009, however, is strongly negative, and statistically significant at the 5% level: a 1 standard deviation decrease in the exposure-weighted export growth decrease household income by 2,000 RMB, or 7 percent. I shall return to the effect of the shock on non-migrant households in Section 2.4.3.

I next turn to the 2009 migration decision of 2008 migrants. It is unclear a priori in which direction we should expect a negative shock at destination to impact migrants' decisions with regards to their future migrations. An economic shock decreases the income of the household, and affects the relative attractiveness of the destination of a migrant. If expected earnings from migrations are larger than the ones from non-migrating, the income effect of the shock to expected migration earnings could lead to *even more* migrations (Gröger, 2017). Conversely, if the substitution effect dominates, the economic downturn reduces the attractiveness of migration, thereby decreasing migration probability. One could argue that the substitution effect would require that one takes into account all of the shocks to all the potential destinations of migrants. This is especially the case if migrants can reallocate to another destination from one year to another.

Table 2.A.7 presents the results. On average, there is a small, and imprecisely estimated negative effect of the export shock on the 2009 migration decision of 2008 migrants.¹⁵ A decrease by 1 standard deviation in the growth of exports in the 2008 destination of a migrant increases her probability of migrating in 2009 by 2.1 percentage points on average. In columns 2 and 3 the effect of the shock is allowed to vary by whether the

¹⁵Note that the shock variable is expressed in negative values: the more negative the independent variable, the larger the shock.

2008 migration spell was directed towards a prefecture within the province of origin of the migrant worker, or outside of it. The overall effect seems to be driven by migrants who migrated towards close-by destinations in 2008. This observation is line with the aggregate observation that shorter-distance migrations increase from 2008 to 2009, while overall migration rates are stable or slightly increasing.

This suggests that the shock of 2008/2009, while making migrations relatively less attractive, pushed migrant workers of 2008 to stay in migration (or to be migrant workers again in 2009) more than they would have otherwise. This can be interpreted as an income effect of the shock on migrant households. I next examine whether the effect of the economic shock varies with pre-determined observable characteristics of migrant workers. Table 2.A.8 shows that the reaction of 2008 migrant workers to the shock is not homogeneous. Individuals with characteristics that predict high migration rates do not react to the economic shock (column 4) or react by migrating less (column 3). Individuals with characteristics that predict lower migration rates (column 1 and 2) react to the shock by increasing their migration rate in 2009. These are individuals whose sample average of migration probability in 2009 are also lower (71 percent) than the rest of the 2008 migrant workers (80 percent). They are typically above 30 years of age, or women (see Table 2.A.4). For the individuals in Q1 and Q2 the effect of the shock is substantial: a decrease by 1 standard deviation in the export growth at destination increases their migration probability in 2009 by 7.2 and 7.4 percentage points respectively, or 10 and 9 percent. These effects are statistically significant at the 5 percent level. I verify in Table 2.A.9 that these results are not sensitive to the exclusion of fixed effects for the province of destination of migrants.

Another concern regarding this strategy is that the heterogeneous reaction of different types of household members could be a reflection of heterogeneously composed households sending members to areas that are differentially affected by the shock. This would arise for instance if households with different member types send migrants to urban markets with different sectoral distribution, who could be differentially affected by the economic

downturn. I address this concern in the next section by showing balance tests of household composition with regards to the shock measure.

I also verify in column 4 of Table 2.A.7 whether yearly reallocations of migrant workers to a different destination are affected by the economic shock. Indeed, among migrant workers of a given year, some continue being migrant workers to the same destination the next year, some stop migrating, and some migrate again, but to a different destination. Yearly reallocations of migrants across provincial borders are small yet not negligible.¹⁶ Approximately 5.2% of within-province migrants in 2008 migrate outside of the province in 2009, and 5.3% of outside-province migrants migrate within their province in 2009; these probabilities are similar when looking only at young men. Column 4 of 2.A.7 shows that education is an important predictor of whether 2008 migrant worker migrate to a different destination in 2009 as compared to 2008; but it also shows that the magnitude of the 2008/2009 shock does not affect this probability.¹⁷

Taken together, the results from Tables 2.A.7 and 2.A.8 show that marginal migrants reacted to the economic shock with which they were confronted at destination by increasing their 2009 migration probability, or to put it differently, by putting an end less frequently to their migration spells. The migration probability of migrant workers that have more typical “migrant-like” characteristics either decreased or stayed stable as a result of the shock. This is compatible with the latter being confronted with larger substitution effects, because the crisis primarily hit the manufacturing exporting sector, where young male migrant workers are employed. Marginal migrant workers by contrast may have been employed in different, less-affected industries, and may therefore face a smaller substitution effect, and increase their migration pattern as a reaction to the income effect. Another possible explanation for this set of results would be that more typical migrants also have higher chances of finding jobs when staying at home. Thus, the fact that these

¹⁶A limitation of this analysis is that 2009 destination prefectures are not reported in the data. This means that migrants who migrated out-of-province in 2008 and 2009, may still be switching their migration destinations (which cannot be inferred from the data available). The propensity to change destination here is thus a lower bound of true reallocations.

¹⁷This is true of all subsamples Q1, Q2, Q3 and Q4, for which the results are not displayed for the sake of concision. This is probability being very small, there is also little variation to exploit.

typical migrants do not increase their migration in 2009 as a response to the shock, in the same way as more marginal migrants do, could be due to the fact that they have already taken up a job at home as a consequence of the crisis. They would thus be unable to follow the rebound of the export sector starting summer 2009. Unfortunately, questions pertaining to the reasons why individuals do not migrate in 2009, especially why former migrants do not migrate, are not asked in the RUMiC survey. Absent this information, it is difficult to disentangle both explanations. However, identifying the type of work for which migration is substituted may help understanding the consequences of the shock in 2009.

Part of the literature on migrant workers in China has shown that off-farm employment at origin location acts as a substitution activity for migrants who come back, notably out of hardship in cities (Démurger and Xu, 2011; Huang et al., 2011). The RUMiC data for 2009 contains information about the number of days spent in each of four activities over the course of one year (agricultural work, household work, non agricultural employment in the origin township and away). Tables 2.A.10 shows that migrant work is substituted for non-agricultural work, rather than for agricultural work: a decrease by 1 standard deviation in the export shock increases migrant non-agricultural work by 9 days (or 4 percent), and decreases non-migrant non-agricultural work by 7 days (or 29 percent). Table 2.A.11 verifies that these effects are driven by the individuals in Q1 and Q2, who are the ones for whom the migration probability reacts to the economic shock.

2.4.2 Non-migrants in migrant households

I next turn to the reaction in 2009 of the members of the migrant households who had not migrated in 2008. Given the results of the previous section, we should expect the same type of heterogeneity across the observable characteristics of these individuals, which would imply that the substitution effect is weaker for individuals who are “more different” from typical migrants. However, given that these individuals are also part of households whose income was negatively affected by the economic downturn of 2008/2009, we might also

expect that the income effect could push some individuals into migration to compensate for the loss of income. This would be facilitated by the presence of already-migrating households members.

Table 2.A.12 supports these predictions. Overall, the effect of the export shock at a migrant worker's destination on the other household members who previously were not migrants is 0. However, this precisely estimated 0 hides substantial heterogeneity. I estimate Eq. 2.2 separately for individuals in Q1, Q2 and Q3 and Q4, using the quartiles of the distribution of predicted migration probability from the migrant subsample (studied in the previous subsection). Because individuals who are not migrating are systematically different from individuals who migrate, the sizes of the subsamples Q1, Q2, Q3 and Q4 are not equal, and unsurprisingly decreasing. I pool the individuals in Q3 and Q4 for statistical power. Individuals in Q1, Q2, and Q3 and Q4 display very different baseline probabilities of migrating, from 3 percent for Q1, to 15 percent for Q2 and 28 percent for Q3 and Q4. The regression results are in line with those of the previous section, and in line with a strong substitution effect for migrant-like individuals and an income effect for others. Individuals in Q1 are actually pushed to migrate more: a decrease by 1 standard deviation in the export shock increases their probability to migrate in 2009 by 1 percentage point on average, or 25 percent. At the other end of the spectrum, the same decrease by 1 standard deviation in the export shock decreases the probability to migrate of individuals in Q3 and Q4 by 9 percentage points, or 31 percent.

2.4.3 Non-migrant households

Non-migrant households are not directly affected by the shock at the destination of other households. In theory, economic shocks in the migration destinations of migrants in their villages could have several effects that are difficult to disentangle.

Large adverse income effects that affect an important share of households in a community could have general equilibrium effects on other households: a decrease in local demand for goods in services can for instance decrease the income of local businesses, and

make migrations more attractive. If migrant households react by sending more household members away on migration, this could also make local labour markets less constrained and more attractive to other households, who would therefore be less likely to start migrating.

Negative shocks at destination could also inform other households about the relative gains to migration. The direction in which this effect is likely to play is unambiguously negative for the propensity to migrate towards that or those particular destinations, but less unequivocal for migration in itself. Indeed, migrations towards relatively more-affected destinations could reallocate towards less-affected destinations; or it could turn people away from migration altogether. Networks have been shown to matter in the diffusion of information regarding jobs (Ioannides and Loury, 2004); and migration networks in particular have been shown to influence the destination choice of migrants (Epstein, 2008). If network effects are large (thereby preventing reallocation to different destinations), then we could expect large economic shocks at some destination to turn people in the prefectures of origin away from migrating altogether. Unfortunately, the 2009 wave of the RUMiC survey does not allow us to know with precision the destination of migrants during 2009, so that we cannot identify whether migrant workers in 2009 went to the primary destination of 2008, or elsewhere.

However, data on income are informative on the potential channels through which shocks for migrants of the village transmit to other households. Table 2.A.6 shows that the household-level income of non-migrant households is not affected by the magnitude of the economic shock at the primary destination of migrants of the same location. Any effect on migration decisions is thus more likely to reveal informational spillovers rather than general equilibrium effects via local demand or local labour markets, since the latter would presumably have an impact on household-level income. One important caveat of this strategy is that the RUMiC survey does not include data on other individuals, who are not household members, but could be sending remittances to the household. Former household members who would not be considered as formally part of the household

anymore, and who would be long-term migrants in urban areas, are thus not accounted for.

Table 2.A.13 displays the results of estimating Equation (2.3) with province of origin fixed effects. Among individuals in non-migrant households of 2008, the magnitude of the shock at village-specific destinations has a sizeable effect on their probability to migrate in 2009. A decrease by 1 standard deviation in the measure of the export shock decreases migration probability by 1.9 pp on average, down from a 7 percent baseline. This effect is statistically significant at the 5% level. In line with the results of the last two subsections, this effect exhibits a lot of heterogeneity by the characteristics of individuals. The probability of migration in 2009 of individuals in Q3 and Q4 is strongly impacted by the magnitude of the shock: a decrease by 1 standard deviation in the export shock decreases their migration probability by 5.9 percentage points, or 26 percent. The effect for the individuals in Q1 is a relatively precisely estimated 0, while the effect on individuals in Q2 is positive, much smaller in magnitude than that of individuals in Q3 and Q4, and unprecisely estimated. Note that the 2008 within-village level migration rate is also an important predictor of the 2009 migration rate of individuals who were not migrating in 2008. Its effect is also strongly heterogeneous, and strongest for individuals who have migrant-like characteristics. This suggest important network effects.

These results are consistent with the fact that the economic shock mostly affected the sectors where the typical young male migrant workers worked in 2008. These are the individuals whose marginal gains from migration are most affected by the economic shock. It is also these individuals who have the highest baseline probability of migrating in 2009 after not having migrated in 2008. The channel through which this shock is transmitted to individuals who were not migrating in 2008 however is interesting however, inasmuch as these individuals or their households did not, a priori experience the shock themselves. Given the small and statistically imprecise effect of the same shock on the income of households, I interpret this effect as channeled by the transmission of information from 2008 migrant households to 2008 non-migrant households.

In order to test this intuition, I include as independent variables the share that the primary destination represents in all migrations from the origin village, and its interaction with our macroeconomic variables of interest. Table 2.A.14 displays the result of these estimations. The coefficient on the interaction term is positive and statistically significant at the 5% level. This shows that the more the distribution of destinations of migrants in a village is skewed towards one unique destination, the larger the effect of an economic shock in that destination on the probability that other households migrate. This is compatible with information on the shock spreading through local networks at origin: the larger the network that is directly affected by the shock, the more likely is it that this shock impacts other households.

Finally, Table 2.A.15 displays the results of a more demanding strategy, that leverages within-prefecture-of-origin variation in the primary destination. The results are globally in line with those of Tables 2.A.13 and 2.A.14: the negative export shock in the destinations of migrants of a given origin decreases the likelihood of migration in 2009 for non-migrant households of 2008 from the same origin. This effect is still heterogeneous across the predetermined characteristics of individuals, although it is only precisely estimated for individuals in Q1 and Q2. Given the support of the variation on which these estimations are based (see section 2.3), the lack of statistical precision is hardly surprising.

2.5 Robustness checks

2.5.1 Balancing tests

One concern about the nature of the results and particularly the patterns of heterogeneity that can be observed is that they may be the reflect of heterogeneous households being differently affected by the shock. This would be the case for instance if households with different compositions in terms of the observable characteristics of their members were to send their migrating members to different destinations, or to send different types of members to different of destinations, and if the economic shock were not orthogonal to

these decisions. This could be the case for instance if households of different composition were sending members to different sectors of the industry.

One way to alleviate these concerns is to perform a balancing test on the characteristics of households, with regards to the intensity of the shock they are confronted with. In order to do this, I run regressions on each of a series of variables representing the structure of the household (size, gender distribution, age distribution, education distribution) that are supposed to be time invariant and determined before the shock.¹⁸ Table 2.A.23 displays the results. Reassuringly, the magnitude of the shock seems to be uncorrelated with the composition of migrant households prior to the shock.

2.5.2 Questionnaire respondents' presence

Because of the prevalence in China of households that are split across different locations, one characteristic of the RUMiC survey is that it was designed to capture all household members, including those who were away at the time of the survey. The timing of the survey itself was set at a time where it was expected to be possible for a maximum of household members to be respondents for themselves, but notably because of the presence of many migrant members, a substantial part of information is collected by interrogating other household members.

This raises a concern as to the quality of the data that regards migration and migrant members. The RUMiC survey includes a question as to whether each member is present during the survey and can answer the questions that are pertaining to him or her. I run the main analysis on the subsample of individuals who are answering for themselves. Unfortunately, this massively decreases sample size in the case of 2008 migrants; indeed, their re-migration rate is high (around 80%), and even if they have not migrated at the time of the survey, they may not be present because they are the most likely to be engaged on the labor market. Results on individuals who weren't migrants in 2008,

¹⁸I measure these characteristics in the year before the shock, but they are also remarkably stable over time.

when restricted to individuals who are present at the time of the survey, are less precisely estimated, but qualitatively not different from the results estimated on the full sample.

2.6 Conclusion

In this paper, I study the consequences of the large adverse shock of 2008/2009 on the migration patterns of rural households in China. I leverage the pre-crisis exposition of destination areas to foreign trade, and variation across provinces in the magnitude of the shock, as a source of identification. I find that the economic shock had strongly heterogeneous effects on different populations. Female and older migrants, or marginal migrant workers, reacted to the shock by increasing their propensity to migrate in 2009; young male migrants, that are the bulk of the migrant population, did not react. The economic downturn in the export manufacturing sector at destination also had spillover effects to other household members within migrant households. Among previously-non-migrating household members the response was heterogeneous: young adults' propensity to migrate declined in response to the shock while adults over 30 were more likely to migrate. Households who had no migrants in 2008 reacted to the shock at the primary destination of the migrants of their village by sending less migrants. These effects are most pronounced for the demographic with the highest ex-ante likelihood of migration (young males). Overall, this very heterogeneous pattern explains why migration rates have remained stable and even slightly increased between 2008 and 2009.

These results are compatible with large income effects for migrant households, and very heterogeneous substitution effects across the characteristics of individuals. These heterogeneous substitution effects are not limited to the boundaries of the household, since migration also appears less attractive to households without migrants when their likely destination is hit with a larger shock. For these households, I show evidence of strong informational effects of migrations by their neighbouring households, who transmit information about the shocks they face to their location of origin.

2.A Tables

Table 2.A.1 – Migration patterns, 2008 and 2009

2009	2008			
	No migration %	Within prov. %	Outside prov. %	Total %
No migration	75.9	1.8	2.7	80.4
Within prov.	2.3	5.4	0.6	8.4
Outside prov.	2.7	0.4	8.2	11.3
Total	80.9	7.6	11.5	100.0

The data in this table are from RUMiC 2008 and 2009. The sample considered is all working age individuals (aged 18 to 64) who are not retired, nor students, nor in the military.

Table 2.A.2 – Migration patterns, 2008 and 2009: Young men

2009	2008			
	No migration %	Within prov. %	Outside prov. %	Total %
No migration	43.3	4.6	4.9	52.8
Within prov.	5.5	14.4	1.6	21.5
Outside prov.	5.6	1.0	19.1	25.7
Total	54.4	20.0	25.5	100.0

The data in this table are from RUMiC 2008 and 2009. The sample considered is all young men (aged 18 to 30) who are not students nor in the military.

Table 2.A.3 – Macroeconomic statistics of Chinese prefectures, 2007-2008

	Mean	Std. Deviation	Min	Max
2007				
Population (million)	4.22	2.98	0.18	32.4
Nominal GDP (million RMB)	7.77	12.2	0.070	121.9
Nominal GDP growth	0.20	0.059	0.021	0.54
Real GDP growth	0.15	0.030	0.036	0.33
GDP per capita	23571.1	28003.6	3380.5	320254.8
Nominal per capita GDP growth	0.19	0.060	-0.0045	0.52
2008				
Population (million)	4.27	3.02	0.19	32.6
Nominal GDP (million RMB)	9.28	14.1	0.080	137.0
Nominal GDP growth	0.21	0.072	-0.28	0.63
Real GDP growth	0.13	0.042	-0.36	0.26
GDP per capita	27882.6	31402.4	3547.2	342287.0
Nominal per capita GDP growth	0.20	0.064	-0.088	0.52
2009				
Population (million)	4.30	3.04	0.21	32.8
Nominal GDP (million RMB)	10.4	15.8	0.091	150.5
Nominal GDP growth	0.12	0.081	-0.27	0.45
Real GDP growth	0.13	0.037	-0.040	0.36
GDP per capita	30464.7	31932.0	4406.4	333441.1
Nominal per capita GDP growth	0.11	0.078	-0.29	0.35
Observations	1083			

Data in this table are from the City Statistical Yearbooks of the National Bureau of Statistics.

Table 2.A.4 – 2009 migration choice: 2008 non-migrants in non migrant-households

	$\mathbb{1}\{\text{Migrant worker}\}$ (1)
Male	0.092*** (0.004)
Education level(ref: no education)	ref.
Elementary school	-0.051*** (0.013)
Junior middle school	-0.040** (0.017)
Senior middle school	-0.085*** (0.018)
Higher education	-0.123*** (0.019)
Years of education	0.012*** (0.003)
Years of education, sq.	-0.001*** (0.000)
Age category (ref: 16-19 years old)	ref.
20-29 years old	0.026*** (0.008)
30-39 years old	-0.180*** (0.010)
40-49 years old	-0.323*** (0.010)
50-59 years old	-0.388*** (0.010)
60-64 years old	-0.410*** (0.012)
Ethnicity (ref: Han)	ref.
Zhuang	0.050 (0.047)
Hui	0.013 (0.089)
Uyghur	-0.162 (0.099)
Yi	-0.132 (0.146)
Miao	0.420*** (0.146)
Manchu	-0.075*** (0.020)
Other	-0.005 (0.039)
Marital status (ref: single)	ref.
Married	-0.115*** (0.007)
Remarried	-0.103*** (0.013)
Cohabit	-0.105*** (0.025)
Divorced	-0.037 (0.027)
Widowed	-0.124*** (0.018)
Hukou type (ref: Local urban)	ref.
Non-local urban hukou	0.233*** (0.030)
Local rural hukou	0.100*** (0.008)
Non-local rural hukou	0.129*** (0.038)
Height	-0.000*** (0.000)
Constant	0.468*** (0.033)
Nb. Obs.	41776
Mean of dep. var.	0.21
Adjusted R square	0.24

Observations are at the individual level. The outcome variable is a dummy variable indicating if the individual was a work migrant in 2007 or 2008. The regressors of interest refer to the main destination of migrants in the respondent's village. Other individual controls not reported in the table include dummy variables for age categories, and marital status. The reference of the education-level categorical variable is 'No education'. Standard errors are clustered at the level of prefectures of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.5 – Determinants of the length of 2008 migration duration: 2008 migrants

	All (1)	Q1 (2)	Q2 (3)	Q3 (4)	Q4 (5)
2009 Export shock (normalized)	-0.025 (0.073)	0.076 (0.144)	0.052 (0.131)	-0.093 (0.118)	-0.118 (0.133)
2008 GDP per c. differential	0.000 (0.000)	0.000 (0.000)	0.000** (0.000)	-0.000 (0.000)	-0.000 (0.000)
Dist to 2008 dest	0.062 (0.069)	0.182** (0.087)	0.061 (0.142)	0.053 (0.142)	-0.056 (0.127)
Male	-0.099 (0.060)	-0.454** (0.175)	0.707* (0.399)	-0.509** (0.221)	-0.060 (0.152)
Elementary school or literacy class	0.598 (0.374)	0.298 (0.433)	1.919 (1.930)	1.650* (0.953)	-1.671*** (0.626)
Junior middle school	0.777** (0.385)	0.526 (0.497)	1.905 (1.929)	1.885** (0.876)	-1.345** (0.566)
Senior middle school	0.829* (0.418)	0.907* (0.490)	1.626 (2.026)	1.801** (0.870)	-1.316** (0.575)
Higher education (college, university)	0.489 (0.462)	0.751 (0.651)	1.458 (2.118)	1.171 (0.948)	-1.636*** (0.588)
Nb. Obs.	3609	847	851	868	877
Nb. clusters	77	62	65	66	62
Mean of dep. var.	9.20	8.91	9.14	9.21	9.49
Adjusted R square	0.22	0.29	0.15	0.27	0.17

Observations are at the individual level. The sample consists of all individuals aged between 18 and 64 who have migrated to a known destination in 2008. The outcome variable in all columns is the length of the 2008 migration spell. The macroeconomic regressors of interest refer to the main destination of migrants in the respondent's village. Other individual-level controls not reported in the table include: dummy variables for age categories, marital status and ethnicity. Other controls include counties of origin fixed effects. Standard errors are clustered at the level of counties of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.6 – 2008 and 2009 household incomes

	Migrant households		Non-migrant households	
	2008	2009	2008	2009
2009 Export shock (normalized)	1178.255 (784.479)	2003.529** (912.667)	-532.452 (907.729)	-286.269 (1364.578)
2008 origin-destination GDP per capita diff.	0.014 (0.009)	-0.018 (0.074)	0.001 (0.012)	0.019 (0.074)
Household size	2785.929** (1166.137)	2309.822* (1238.818)	5112.448*** (877.269)	6827.631*** (1090.436)
County of origin FE	Yes	Yes	No	No
Province of origin FE	No	No	Yes	Yes
Province of destination FE	Yes	Yes	Yes	Yes
Nb. Obs.	2354	2332	4739	4702
Nb. clusters	77	77	73	73
Mean of dep. var.	25208.03	28878.88	16510.64	19422.44
Adjusted R square	0.39	0.35	0.21	0.21

Observations are at the household level. The outcome variable is the household's income in 2008 and in 2009. The regressors of interest refer to the main destination of migrants in the household (columns 1 and 2) or in the household's village (columns 3 and 4). Other household level controls not reported in the table include household structure variables: number of children, number of young adults, number of men, number of young adult men, average level of education of adults, education level of the household head, number of hh members of ethnic minority; in columns 1 and 2 additional control variables describe the characteristics of migrants from the household: number of migrants, average age, gender, education level and ethnicity of migrants. In columns 2 and 4, the 2009 per capita GDP differential between the prefecture of origin and the prefecture of destination (migrant households) or the primary destination of migrants (non-migrant households) are included. In columns 3 (resp. 4), additional controls for the origin prefecture are included: the values of the 2007 and 2008 (resp. 2008 and 2009) GDP per capita in the prefecture, the 2008 (resp 2009) yearly growth rate of GDP per capita in the prefecture, and the 2008 (resp. 2009) prefecture-level migration rate of working age adults. Standard errors are clustered at the level of counties of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.7 – Probability of migrating in 2009: 2008 migrants

	Probability to migrate in 2009			Probability to migrate to a different destination in 2009
	All migrants (1)	Migrated within prov. in 08 (2)	Migrated outside prov. in 08 (3)	All migrants (4)
2009 Export shock (normalized)	-0.021* (0.012)	-0.036* (0.019)	-0.011 (0.015)	-0.004 (0.005)
Dist to 2008 dest	0.011 (0.010)	-0.013 (0.012)	0.021 (0.016)	-0.010 (0.009)
Male	0.037*** (0.014)	0.037** (0.018)	0.033* (0.018)	0.003 (0.006)
Elementary school or literacy class	0.180** (0.086)	0.367** (0.142)	0.059 (0.088)	0.059*** (0.018)
Junior middle school	0.188** (0.084)	0.387*** (0.142)	0.063 (0.078)	0.053*** (0.013)
Senior middle school	0.184** (0.084)	0.339** (0.141)	0.089 (0.082)	0.070*** (0.018)
Higher education (college, university)	0.196** (0.082)	0.388** (0.147)	0.054 (0.077)	0.074*** (0.024)
County of origin FE	Yes	Yes	Yes	Yes
Province of 2008 dest FE	Yes	Yes	Yes	Yes
Nb. Obs.	3532	1371	2145	3526
Nb. clusters	76	71	63	76
Mean of dep. var.	0.77	0.77	0.78	0.05
Adjusted R square	0.21	0.23	0.23	0.05

Observations are at the individual level. The sample consists of all individuals aged between 18 and 64 who have migrated to a known destination in 2008. The outcome variable in all columns is a dummy variable indicating if the individual was a work migrant in 2009. The macroeconomic regressors of interest refer to the main destination of migrants in the respondent's village. Other individual-level controls not reported in the table include: dummy variables for age categories, marital status and ethnicity. Other controls include counties of origin fixed effects. Standard errors are clustered at the level of counties of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.8 – Probability of migrating in 2009: 2008 migrants

	All (1)	Q1 (2)	Q2 (3)	Q3 (4)	Q4 (5)
2009 Export shock (normalized)	-0.021* (0.012)	-0.072** (0.027)	-0.074*** (0.023)	0.043** (0.021)	0.005 (0.023)
2008 GDP per c. differential	-0.000 (0.000)	-0.000** (0.000)	-0.000* (0.000)	0.000** (0.000)	0.000 (0.000)
Dist to 2008 dest	0.014 (0.010)	0.002 (0.024)	-0.018 (0.022)	0.063*** (0.019)	-0.015 (0.021)
Male	0.039*** (0.014)	0.042 (0.036)	0.105 (0.064)	0.014 (0.041)	0.040 (0.029)
Elementary school or literacy class	0.203** (0.094)	0.130 (0.115)	0.165 (0.221)	0.522** (0.231)	0.283 (0.256)
Junior middle school	0.210** (0.092)	0.165 (0.115)	0.151 (0.214)	0.532** (0.221)	0.223 (0.244)
Senior middle school	0.205** (0.092)	0.136 (0.122)	0.246 (0.216)	0.493** (0.223)	0.243 (0.243)
Higher education (college, university)	0.212** (0.090)	0.064 (0.141)	0.169 (0.218)	0.499** (0.226)	0.229 (0.242)
Province of destination FE	Yes	Yes	Yes	Yes	Yes
Nb. Obs.	3486	841	843	864	873
Nb. clusters	75	62	65	66	62
Mean of dep. var.	0.77	0.71	0.80	0.78	0.80
Adjusted R square	0.21	0.26	0.20	0.22	0.21

Observations are at the individual level. The outcome variable in all columns is a dummy variable indicating if the individual was a work migrant in 2009. The macroeconomic regressors of interest refer to the 2008 destination of the respondent. Other individual-level controls not reported in the table include: dummy variables for age categories, marital status and ethnicity. Other controls include counties of origin fixed effects. Standard errors are clustered at the level of counties of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.9 – Probability of migrating in 2009: 2008 migrants

	All (1)	Q1 (2)	Q2 (3)	Q3 (4)	Q4 (5)
2009 Export shock (normalized)	-0.005 (0.009)	-0.043** (0.020)	-0.036** (0.018)	0.022 (0.020)	0.020 (0.021)
2008 GDP per c. differential	-0.000 (0.000)	-0.000*** (0.000)	-0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Dist to 2008 dest	0.015 (0.010)	0.001 (0.024)	-0.006 (0.022)	0.052*** (0.018)	-0.000 (0.021)
Male	0.043*** (0.014)	0.035 (0.033)	0.081 (0.061)	0.017 (0.041)	0.043 (0.029)
Elementary school or literacy class	0.213** (0.089)	0.142 (0.109)	0.243 (0.179)	0.524** (0.230)	0.229 (0.259)
Junior middle school	0.222** (0.087)	0.172 (0.109)	0.249 (0.164)	0.540** (0.222)	0.183 (0.251)
Senior middle school	0.216** (0.086)	0.132 (0.114)	0.331** (0.162)	0.505** (0.222)	0.220 (0.249)
Higher education (college, university)	0.228*** (0.085)	0.086 (0.139)	0.298* (0.170)	0.521** (0.225)	0.194 (0.249)
Province of destination FE	No	No	No	No	No
Nb. Obs.	3504	847	850	870	883
Nb. clusters	75	62	65	66	62
Mean of dep. var.	0.77	0.70	0.80	0.78	0.80
Adjusted R square	0.20	0.26	0.19	0.21	0.21

Observations are at the individual level. The outcome variable in all columns is a dummy variable indicating if the individual was a work migrant in 2009. The macroeconomic regressors of interest refer to the 2008 destination of the respondent. Other individual-level controls not reported in the table include: dummy variables for age categories, marital status and ethnicity. Other controls include counties of origin fixed effects. Standard errors are clustered at the level of counties of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.10 – Time use in 2009: 2008 migrants

	All 2008 migrant workers: work days by type of activity			
	Agricultural work	Household work	Non-ag. work origin prefecture	Non-ag. work , outside prefecture
2009 Export shock (normalized)	-0.638 (1.338)	0.131 (0.121)	6.892** (2.915)	-8.741** (3.525)
2008 GDP per c. differential	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
Dist to 2008 dest	-2.337* (1.312)	-0.082 (0.104)	-5.740** (2.223)	6.782** (3.290)
Elementary school or literacy class	2.430 (9.542)	0.339 (0.437)	-2.521 (14.220)	32.447 (22.865)
Junior middle school	0.191 (9.936)	0.579 (0.419)	-7.353 (13.551)	40.602* (21.901)
Senior middle school	5.987 (9.512)	0.627 (0.451)	-4.213 (14.366)	32.062 (22.050)
Higher education (college, university)	3.133 (10.455)	0.703* (0.421)	-5.048 (14.415)	40.406* (23.578)
Nb. Obs.	3278	3210	3275	3412
Nb. clusters	74	74	75	75
Mean of dep. var.	21.35	0.79	24.35	241.16
Adjusted R square	0.20	0.06	0.10	0.23

Observations are at the individual level. The outcome variable is specified for each column and refer to the number of days each respondent allocated to different activities in 2009. The macroeconomic regressors of interest refer to the 2008 destination of the respondent. Other individual-level controls not reported in the table include: dummy variables for age categories, marital status and ethnicity. Other controls include counties of origin fixed effects. Standard errors are clustered at the level of counties of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.11 – Time use in 2009: 2008 migrants

	Q1 & Q2 2008 migrant workers: work days by type of activity			
	Agricultural work	Household work	Non-ag. work origin prefecture	Non-ag. work , outside prefecture
2009 Export shock (normalized)	0.725 (2.323)	0.211 (0.195)	10.465** (4.143)	-12.799*** (4.797)
2008 GDP per c. differential	0.000 (0.000)	0.000 (0.000)	0.000* (0.000)	-0.000 (0.000)
Dist to 2008 dest	-3.866* (2.134)	-0.214 (0.130)	-2.864 (2.540)	7.682 (5.398)
Elementary school or literacy class	12.987 (9.253)	0.672 (0.516)	-5.372 (16.702)	6.891 (25.818)
Junior middle school	5.778 (10.138)	0.754 (0.529)	-9.376 (15.402)	24.296 (25.201)
Senior middle school	12.704 (10.260)	0.670 (0.565)	-14.536 (16.203)	14.220 (24.963)
Higher education (college, university)	7.723 (12.332)	0.986* (0.553)	-7.645 (16.651)	38.446 (29.921)
Nb. Obs.	1620	1572	1616	1673
Nb. clusters	71	71	71	71
Mean of dep. var.	28.02	1.10	23.42	230.62
Adjusted R square	0.22	0.07	0.09	0.26

Observations are at the individual level. The outcome variable is specified for each column and refer to the number of days each respondent allocated to different activities in 2009. The macroeconomic regressors of interest refer to the 2008 destination of the respondent. Other individual-level controls not reported in the table include: dummy variables for age categories, marital status and ethnicity. Other controls include counties of origin fixed effects. Standard errors are clustered at the level of counties of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.12 – 2008 non-migrants in migrant households: Probability of migration in 2009

	All (1)	Q1 (2)	Q2 (3)	Q3 & Q4 (4)
2009 Export shock (normalized)	-0.000 (0.007)	-0.010* (0.005)	-0.014 (0.053)	0.088** (0.043)
Dist to 2008 dest	-0.004 (0.006)	-0.002 (0.004)	-0.031 (0.041)	-0.037 (0.039)
HH members are migrating outside province	-0.004 (0.014)	0.002 (0.011)	-0.050 (0.072)	-0.016 (0.099)
County of origin FE	Yes	Yes	Yes	Yes
Province of 2008 dest FE	Yes	Yes	Yes	Yes
Nb. obs.	4540	3616	352	479
Nb. clusters	77	77	55	61
Mean of dep. var.	0.07	0.03	0.15	0.28
Adjusted R ²	0.18	0.05	0.07	0.15

Observations are at the individual level. The outcome variable in all columns is a dummy variable indicating whether the individual was a work migrant in 2009. The macroeconomic regressors of interest refer to the 2008 destination of migrant workers in the respondent's household. The sample consists in all individuals who have not migrated in 2008 but are part of households in which there was at least one migrant in 2008. Other individual-level controls not reported in the table include: dummy variables for age categories, marital status and ethnicity. Standard errors are clustered at the level of counties of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.13 – 2009 migration choice: 2008 non-migrants in non migrant-households

	All (1)	Q1 (2)	Q2 (3)	Q3 & Q4 (4)
2009 Export shock (normalized)	0.019** (0.009)	0.006 (0.005)	0.018 (0.020)	0.059** (0.024)
Dist to 2008 dest	-0.000 (0.007)	-0.000 (0.005)	-0.020 (0.020)	0.030 (0.027)
Within-village migration rate	0.320*** (0.076)	0.091** (0.039)	0.525** (0.212)	1.071*** (0.215)
Province of origin FE	Yes	Yes	Yes	Yes
Nb. Obs.	11455	7815	1519	1519
Nb. clusters	72	72	72	72
Mean of dep. var.	0.07	0.02	0.13	0.23
Adjusted R square	0.14	0.03	0.09	0.18

Observations are at the individual level. The outcome variable in all columns is a dummy variable indicating if the individual was a work migrant in 2009. The regressors of interest refer to the main destination of migrants in the respondent's village. Other individual controls not reported in the table include dummy variables for age categories, and marital status. The reference of the education-level categorical variable is 'No education'. Standard errors are clustered at the level of prefectures of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.14 – 2009 migration choice: 2008 non-migrants in non migrant-households

	All (1)	Q1 (2)	Q2 (3)	Q3 & Q4 (4)
2009 Export shock (normalized)	-0.027 (0.020)	-0.009 (0.010)	-0.082 (0.066)	-0.050 (0.051)
Share of primary destination	0.100* (0.056)	0.030 (0.023)	0.260* (0.147)	0.159 (0.151)
Share of primary destination \times 2009 Export shock (normalized)	0.103** (0.042)	0.035* (0.020)	0.220 (0.148)	0.250** (0.105)
Dist to 2008 dest	0.006 (0.007)	0.001 (0.005)	-0.002 (0.016)	0.045 (0.031)
Within-village migration rate	0.360*** (0.079)	0.102** (0.040)	0.623*** (0.215)	1.118*** (0.230)
Province of origin FE	Yes	Yes	Yes	Yes
Nb. Obs.	11455	7815	1519	1519
Nb. clusters	72	72	72	72
Mean of dep. var.	0.07	0.02	0.13	0.23
Adjusted R square	0.14	0.03	0.10	0.19

Observations are at the individual level. The outcome variable in all columns is a dummy variable indicating if the individual was a work migrant in 2009. The regressors of interest refer to the main destination of migrants in the respondent's village. Other individual controls not reported in the table include dummy variables for age categories, and marital status. The reference of the education-level categorical variable is 'No education'. Standard errors are clustered at the level of prefectures of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.15 – 2009 migration choice: 2008 non-migrants in non migrant-households
(prefecture FE specification)

	All (1)	Q1 & Q2 (2)	Q3 & Q4 (3)
2009 Export shock (normalized)	0.019 (0.013)	0.016** (0.006)	0.042 (0.041)
Dist to 2008 dest	0.003 (0.032)	0.028** (0.012)	-0.153 (0.126)
Within-village migration rate	0.398** (0.191)	0.176* (0.101)	1.645** (0.625)
Prefecture of origin FE	Yes	Yes	Yes
Nb. Obs.	11455	9334	1518
Nb. clusters	72	72	71
Mean of dep. var.	0.07	0.04	0.23
D1 of Share of primary destination	0.19	0.19	0.19
Adjusted R square	0.16	0.09	0.24

Observations are at the individual level. The outcome variable in all columns is a dummy variable indicating if the individual was a work migrant in 2009. The regressors of interest refer to the main destination of migrants in the respondent's village. Standard errors are clustered at the level of prefectures of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.16 – Probability of migrating in 2009: 2008 migrants
Respondents present only

	Probability to migrate in 2009			Probability to migrate to a different destination in 2009
	All migrants (1)	Migrated within prov. in 08 (2)	Migrated outside prov. in 08 (3)	All migrants (4)
2009 Export shock (normalized)	0.030 (0.045)	0.301 (0.260)	0.019 (0.054)	0.004 (0.023)
Dist to 2008 dest	-0.032 (0.052)	0.479** (0.206)	-0.005 (0.072)	-0.010 (0.020)
Male	0.067 (0.061)	-0.169 (0.149)	0.080 (0.075)	-0.015 (0.024)
Never been to school	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
Elementary school or literacy class	0.050 (0.216)	1.139*** (0.327)	0.050 (0.227)	-0.006 (0.046)
Junior middle school	0.148 (0.218)	1.327*** (0.220)	0.216 (0.234)	0.029 (0.031)
Senior middle school	0.184 (0.203)	1.296*** (0.341)	0.240 (0.214)	0.046 (0.040)
Higher education (college, university)	0.111 (0.236)	1.239*** (0.366)	0.074 (0.238)	0.053 (0.041)
Constant	0.525 (0.478)	-3.910*** (1.183)	0.388 (0.621)	0.082 (0.114)
County of origin FE	Yes	Yes	Yes	Yes
Province of 2008 dest FE	Yes	Yes	Yes	Yes
Nb. Obs.	308	79	213	308
Nb. clusters	47	21	36	47
Mean of dep. var.	0.36	0.34	0.38	0.02
Adjusted R square	0.12	0.17	0.20	-0.04

Observations are at the individual level. The sample consists of all individuals aged between 18 and 64 who have migrated to a known destination in 2008. The outcome variable in all columns is a dummy variable indicating if the individual was a work migrant in 2009. The macroeconomic regressors of interest refer to the main destination of migrants in the respondent's village. Other individual-level controls not reported in the table include: dummy variables for age categories, marital status and ethnicity. Other controls include counties of origin fixed effects. Standard errors are clustered at the level of counties of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.17 – Probability of migrating in 2009: 2008 migrants
Respondents present only

	All (1)	Q1 (2)	Q2 (3)	Q3 (4)	Q4 (5)
2009 Export shock (normalized)	0.022 (0.045)	-0.005 (0.081)	-0.069 (0.328)	1.265 (.)	0.504*** (0.000)
2008 GDP per c. differential	-0.000 (0.000)	-0.000** (0.000)	-0.000 (0.000)	-0.000 (.)	0.000*** (0.000)
Dist to 2008 dest	-0.030 (0.052)	-0.003 (0.079)	-0.187 (0.301)	0.000 (.)	0.110*** (0.000)
Male	0.067 (0.061)	-0.068 (0.108)	0.417 (0.651)	0.000 (.)	0.562*** (0.000)
Never been to school	0.000 (.)	0.000 (.)	0.000 (.)		
Elementary school or literacy class	0.045 (0.214)	0.247 (0.279)	-1.009 (1.967)	0.000 (.)	0.000 (.)
Junior middle school	0.143 (0.217)	0.330 (0.267)	0.500 (1.313)	0.000 (.)	-0.438*** (0.000)
Senior middle school	0.181 (0.202)	0.381 (0.271)	0.630 (1.346)	0.000 (.)	0.000 (.)
Higher education (college, university)	0.107 (0.235)	0.451 (0.296)	-0.599 (1.919)	0.000 (.)	0.000 (.)
Constant	0.514 (0.474)	0.416 (0.587)	0.814 (2.906)	14.004 (.)	-0.286*** (0.000)
Nb. Obs.	308	158	47	8	17
Nb. clusters	47	35	14	4	8
Mean of dep. var.	0.36	0.37	0.32	0.12	0.29
Adjusted R square	0.13	0.12	-0.42	1.00	1.00

Observations are at the individual level. The outcome variable in all columns is a dummy variable indicating if the individual was a work migrant in 2009. The macroeconomic regressors of interest refer to the 2008 destination of the respondent. Other individual-level controls not reported in the table include: dummy variables for age categories, marital status and ethnicity. Other controls include counties of origin fixed effects. Standard errors are clustered at the level of counties of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.18 – Probability of migrating in 2009: 2008 migrants
Respondents present only

	All (1)	Q1 (2)	Q2 (3)	Q3 (4)	Q4 (5)
2009 Export shock (normalized)	0.040 (0.040)	-0.009 (0.066)	-0.056 (0.199)	-1.111*** (0.000)	0.022 (0.055)
2008 GDP per c. differential	-0.000 (0.000)	-0.000** (0.000)	-0.000 (0.000)	0.000*** (0.000)	0.000 (0.000)
Dist to 2008 dest	0.019 (0.052)	0.034 (0.097)	-0.074 (0.303)	-1.868*** (0.000)	-0.252 (0.254)
Male	0.094 (0.056)	-0.001 (0.109)	-0.147 (0.170)	1.914*** (0.000)	0.972*** (0.076)
Never been to school	0.000 (.)	0.000 (.)	0.000 (.)		
Elementary school or literacy class	-0.030 (0.244)	0.080 (0.249)	-0.098 (0.454)	0.000 (.)	0.000 (.)
Junior middle school	0.070 (0.241)	0.191 (0.246)	0.404 (0.474)	-0.845*** (0.000)	0.877*** (0.145)
Senior middle school	0.103 (0.241)	0.142 (0.248)	0.547 (0.519)	2.152*** (0.000)	0.808*** (0.198)
Higher education (college, university)	-0.007 (0.252)	0.183 (0.279)	0.180 (0.422)	-1.718*** (0.000)	0.066 (0.607)
Constant	0.201 (0.433)	0.144 (0.624)	0.240 (2.435)	8.986*** (0.000)	1.010 (1.985)
Nb. Obs.	314	168	54	18	21
Nb. clusters	47	36	17	7	9
Mean of dep. var.	0.36	0.38	0.33	0.22	0.29
Adjusted R square	0.15	0.14	-0.02	1.00	0.69

Observations are at the individual level. The outcome variable in all columns is a dummy variable indicating if the individual was a work migrant in 2009. The macroeconomic regressors of interest refer to the 2008 destination of the respondent. Other individual-level controls not reported in the table include: dummy variables for age categories, marital status and ethnicity. Other controls include counties of origin fixed effects. Standard errors are clustered at the level of counties of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.19 – Time use in 2009: 2008 migrants
Respondents present only

	All 2008 migrant workers: work days by type of activity			
	Agricultural work	Household work	Non-ag. work origin prefecture	Non-ag. work , outside prefecture
2009 Export shock (normalized)	-11.403* (6.703)	0.892* (0.476)	24.110* (13.407)	-2.881 (12.369)
2008 GDP per c. differential	-0.000 (0.000)	0.000 (0.000)	0.000* (0.000)	-0.000** (0.000)
Dist to 2008 dest	-3.253 (7.565)	-0.345 (0.495)	-1.666 (9.527)	5.900 (13.555)
Never been to school	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
Elementary school or literacy class	50.270 (36.449)	1.234 (1.538)	-67.425* (35.520)	58.707 (39.366)
Junior middle school	38.996 (36.222)	2.034 (1.485)	-46.128 (33.092)	74.259** (32.897)
Senior middle school	57.513 (35.303)	1.796 (1.561)	-59.363* (31.070)	46.201 (31.728)
Higher education (college, university)	108.157** (46.857)	1.463 (3.399)	-61.414 (44.637)	6.304 (49.581)
Constant	21.083 (51.777)	3.670 (3.535)	42.920 (82.641)	140.288 (108.378)
Nb. Obs.	295	289	294	309
Nb. clusters	47	47	47	47
Mean of dep. var.	70.92	2.57	47.37	115.10
Adjusted R square	0.31	0.19	0.08	0.26

Observations are at the individual level. The outcome variable is specified for each column and refer to the number of days each respondent allocated to different activities in 2009. The macroeconomic regressors of interest refer to the 2008 destination of the respondent. Other individual-level controls not reported in the table include: dummy variables for age categories, marital status and ethnicity. Other controls include counties of origin fixed effects. Standard errors are clustered at the level of counties of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.20 – Time use in 2009: 2008 migrants
Respondents present only

	Q1 & Q2 2008 migrant workers: work days by type of activity			
	Agricultural work	Household work	Non-ag. work origin prefecture	Non-ag. work , outside prefecture
2009 Export shock (normalized)	-15.386 (10.414)	0.940* (0.511)	32.554** (14.682)	-13.824 (14.388)
2008 GDP per c. differential	0.000 (0.000)	0.000 (0.000)	0.000** (0.000)	-0.001*** (0.000)
Dist to 2008 dest	-4.866 (7.777)	-0.612 (0.533)	3.804 (9.876)	12.348 (15.051)
Never been to school	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
Elementary school or literacy class	46.658 (38.285)	1.224 (1.434)	-52.743 (35.834)	51.482 (44.598)
Junior middle school	38.670 (37.701)	1.363 (1.437)	-38.850 (32.893)	64.493 (38.969)
Senior middle school	56.933 (36.624)	1.491 (1.591)	-52.838 (31.441)	67.301* (38.773)
Higher education (college, university)	95.005 (59.338)	2.079 (2.229)	-25.485 (53.359)	8.480 (72.990)
Constant	12.271 (50.530)	1.856 (4.208)	32.979 (85.636)	89.581 (117.543)
Nb. Obs.	224	222	225	232
Nb. clusters	43	43	43	43
Mean of dep. var.	72.30	2.68	48.10	110.75
Adjusted R square	0.30	0.23	0.17	0.32

Observations are at the individual level. The outcome variable is specified for each column and refer to the number of days each respondent allocated to different activities in 2009. The macroeconomic regressors of interest refer to the 2008 destination of the respondent. Other individual-level controls not reported in the table include: dummy variables for age categories, marital status and ethnicity. Other controls include counties of origin fixed effects. Standard errors are clustered at the level of counties of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.21 – 2008 non-migrants in migrant households: Probability of migration in 2009
Respondents present only

	All (1)	Q1 (2)	Q2 (3)	Q3 & Q4 (4)
2009 Export shock (normalized)	-0.002 (0.005)	-0.005 (0.005)	0.000 (.)	0.055 (0.127)
Dist to 2008 dest	-0.001 (0.003)	-0.001 (0.003)	0.000 (.)	-0.050 (0.063)
HH members are migrating outside province	0.005 (0.011)	-0.003 (0.010)	0.000 (.)	-0.019 (0.122)
County of origin FE	Yes	Yes	Yes	Yes
Province of 2008 dest FE	Yes	Yes	Yes	Yes
Nb. obs.	2531	2348	51	60
Nb. clusters	75	75	18	22
Mean of dep. var.	0.01	0.01	0.00	0.03
Adjusted R ²	0.03	0.03	.	-1.03

Observations are at the individual level. The outcome variable in all columns is a dummy variable indicating whether the individual was a work migrant in 2009. The macroeconomic regressors of interest refer to the 2008 destination of migrant workers in the respondent's household. The sample consists in all individuals who have not migrated in 2008 but are part of households in which there was at least one migrant in 2008. Other individual-level controls not reported in the table include: dummy variables for age categories, marital status and ethnicity. Standard errors are clustered at the level of counties of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

Table 2.A.22 – 2009 migration choice: 2008 non-migrants in non migrant-households
Present respondents only

	All (1)	Q1 (2)	Q2 (3)	Q3 & Q4 (4)
2009 Export shock (normalized)	0.003 (0.004)	-0.000 (0.003)	0.014 (0.014)	0.053 (0.032)
Dist to 2008 dest	0.000 (0.003)	-0.001 (0.003)	-0.014 (0.011)	0.068* (0.036)
Within-village migration rate	0.031 (0.027)	0.015 (0.020)	0.041 (0.083)	-0.236 (0.279)
Province of origin FE	Yes	Yes	Yes	Yes
Nb. Obs.	5709	4812	616	158
Nb. clusters	72	72	72	51
Mean of dep. var.	0.01	0.01	0.03	0.04
Adjusted R square	0.02	0.01	0.03	0.02

Observations are at the individual level. The outcome variable in all columns is a dummy variable indicating if the individual was a work migrant in 2009. The regressors of interest refer to the main destination of migrants in the respondent's village. Other individual controls not reported in the table include dummy variables for age categories, and marital status. The reference of the education-level categorical variable is 'No education'. Standard errors are clustered at the level of prefectures of origin, with significance levels of 1%, 5% and 10% indicated by ***, ** and * respectively.

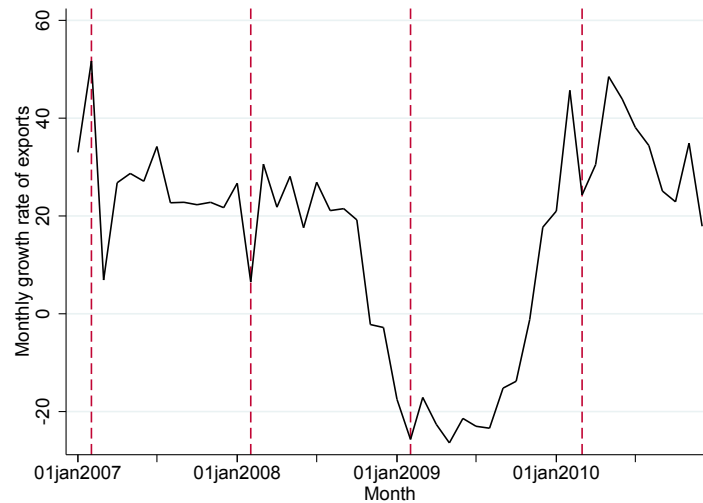
Table 2.A.23 – Balance tests of the characteristics of migrant households

Characteristic of the household	Coefficient of shock measure	Standard Error
Size of household	-0.0247	(0.0296)
Number of members > 60 y.o.	-0.00333	(0.0160)
Number of members < 30 y.o.	-0.0211	(0.0232)
Number of men	0.0141	(0.0180)
Number of men > 60 y.o.	-0.000479	(0.00877)
Number of men < 30 y.o.	0.0131	(0.0152)
Number of children	0.00845	(0.0169)
Number of working age adults	-0.0137	(0.0244)
Number of married individuals	-0.00986	(0.0237)
Nb of members, by education level		
No education	-0.833	(1.107)
Primary	-3.067	(2.296)
Middle (lower)	-0.852	(2.258)
Middle (upper)	2.178	(1.724)
High school and higher education	-1.592	(1.740)
<i>N</i>	2438	

In each row, the coefficient reported is the point estimate of the normalized shock that migrant households are exposed to, in a regression of the variable reported in the first column, on origin county fixed effects, and the magnitude of the shock. The sample consists in all households where at least one member was a migrant in 2008. Standard errors are reported in parentheses in the third column, and are clustered at the prefecture of origin level, with significance at the 1%, 5% and 10% level denoted by ***, **, and * respectively.

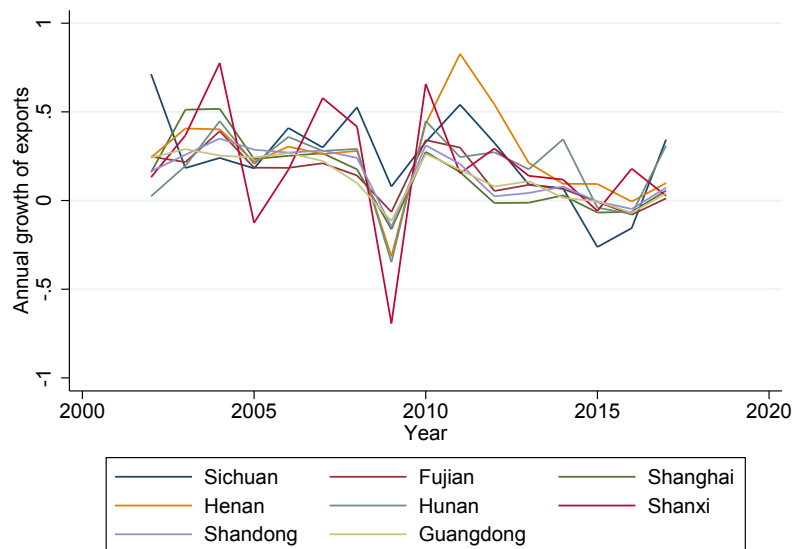
2.B Figures

Figure 2.B.1 – Evolution of exports and timing of the RUMiC survey



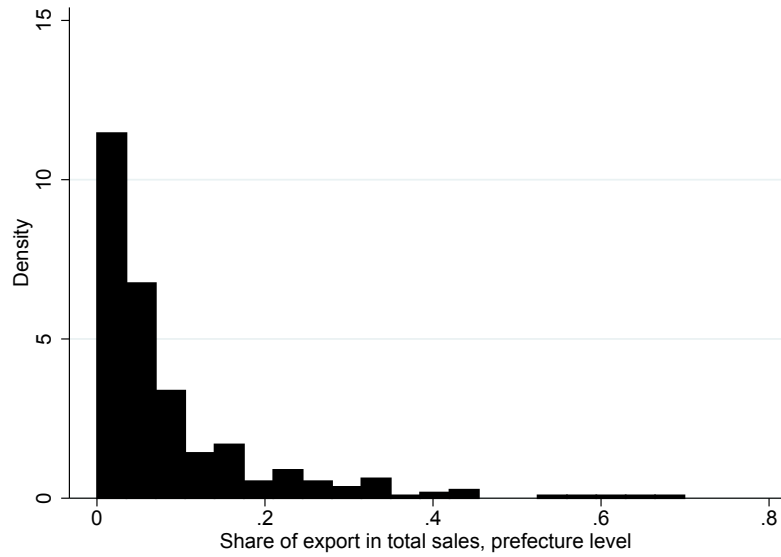
Note: This graph plots the evolution of monthly growth of aggregate exports in China. The red dashed lines materialize the periods of reference of the three survey waves, for 2007, 2008 and 2009 (the survey is conducted around the Spring Festival, which takes place in late February-early March of each calendar year. The data are from the NBS.

Figure 2.B.2 – Evolution of Exports, selected provinces



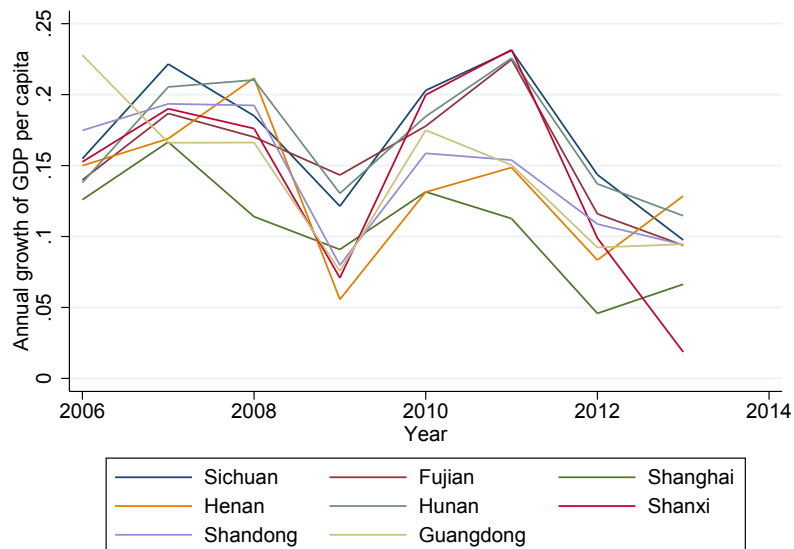
Note: This graph plots the evolution of annual GDP per capita growth for different provinces in China. For readability, the 8 provinces are chosen at 8-tiles of the distribution of provinces. The underlying data are from the NBS Statistical Yearbooks.

Figure 2.B.3 – Exposure to the foreign trade shock, by prefectures



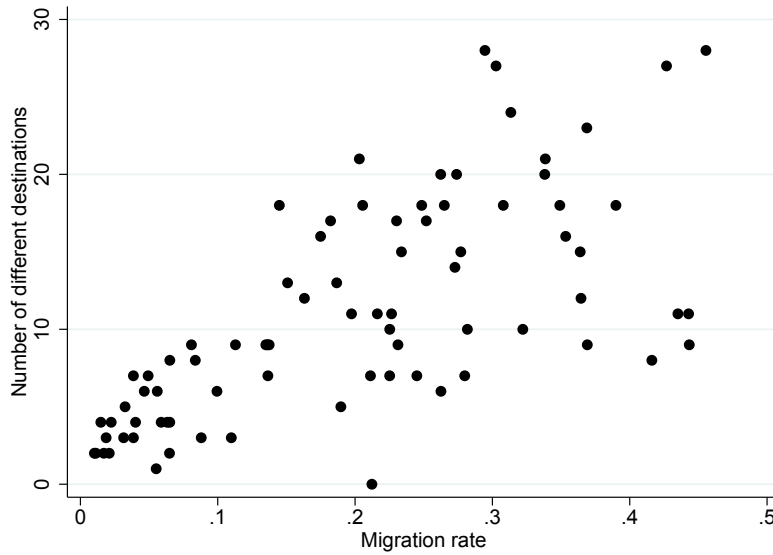
Note: This graph represents the distribution across prefectures of the exposure to foreign trade of manufacturing firms. This exposure measure is defined as the share of exports in total sales by manufacturing firms in a given prefecture. The underlying data are from the NBS Census of manufacturing firms, 2007.

Figure 2.B.4 – Evolution of GDP per capita growth, selected provinces



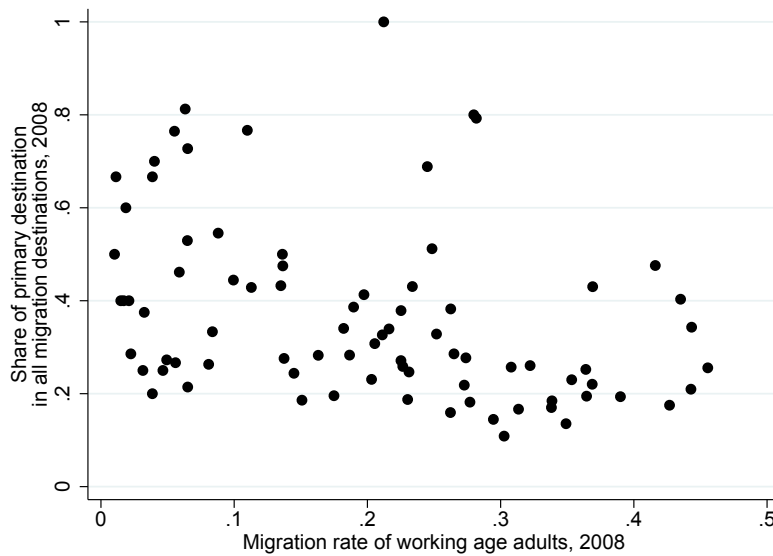
Note: This graph plots the evolution of annual GDP per capita growth for different provinces in China. For readability, the 8 provinces are chosen at 8-tiles of the distribution of provinces (these are the same as in Figure 2.B.2). The underlying data are from the NBS Statistical Yearbooks.

Figure 2.B.5 – Number of destinations by size of the migrant worker body



Note: Each dot in this graph represents a village. This graph represents the number of different identifiable destinations chosen by migrant workers in a village, as a function of the migration rate of the working age population in the village. The underlying data are from the RUMiC 2009 survey.

Figure 2.B.6 – Primary destinations and migration rates



Note: Each dot in this graph represents a village. This graph represents the share that the primary destination of migrant workers from a village represent in the total number of migrant workers from that village. The primary destination of a the migrant workers from a village is defined as the prefecture to which most of the migrant workers from a village elect to go in 2008. The underlying data are from the RUMiC 2009 survey.

Chapter 3

Large Means-Tested Pensions with Informal Labor Markets: Evidence from South Africa

co-written with Alessandro Tondini (PSE, Université Paris I)

We investigate how means-tested public pensions interact with the informal sector, by exploiting a reform in the non-contributory *Old Age Pension* system in South Africa, where the eligibility age was lowered from 65 to 60 for men. By employing a difference-in-discontinuities (“diff-in-disc”) approach, we show that this reform triggered a large drop in elderly male employment. This response at the extensive margin comes from informal workers, who drop out of the labor force, while formal employment is mostly unaffected. This heterogeneity is not due to lower wages in the informal sector; at the same level of wages, informal workers drop out, while formal workers do not. This occurs despite the implicit incentive to draw benefits and simultaneously work informal jobs, and even if the means-test is located where formal and informal wages largely overlap. In total, we estimate that the pension reform has driven about 25,000 elderly individuals away from informal jobs.

3.1 Introduction

Understanding and quantifying labor supply responses to income shocks is a central question for economic research. It is also a key parameter for policy-makers, essential for the “calibration” of social programs, such as those non-contributory, public pensions schemes that are prevalent across developing countries. What is unique and relevant in these contexts is the concern that social assistance might distort incentives due to the segmented nature of the labor market, and create inefficiencies (Gerard and Gonzaga (2016)). Indeed, when a large portion of the workforce escapes labor market regulation, social assistance programs providing large income benefits conditional on means may discourage work, but also incentivize entry into the informal sector.

This paper exploits a reform of the public, non-contributory pension system of South Africa, which significantly lowered the age threshold necessary to access the pension, for men only. The focus of this paper is to document in detail the labor market effects on direct recipients, i.e. the elderly. By making use of this reform, we are able to precisely identify the labor market effects of the public pension, while avoiding potential confounders from other pension schemes, which we show to be sizable. We provide evidence that the disincentive effects of the OAP on its direct recipients are large, but almost exclusively concentrated on informal employment, as informal workers drop out of the labor force regardless of the level of hourly wages. Instead, formal workers are affected only at very low level of hourly wages. We believe these results have implications for both economic research and policy design. These results speak (indirectly) to the potentially large welfare effects of a public pension in a developing country: providing individuals with an external source of income results in large, informal labor supply adjustments at the extensive margin, suggesting that this type of program might relieve subsistence-level constraints. Moreover, in our results, we find no evidence that the pension gives the incentive to combine earnings from informal labor, which do not enter the means-test, with the pension transfer. This has important implication for policy design, as it indicates

that this potentially concerning “perverse” effect is small, despite the means-test being located in an area where the two distributions largely overlap. Lastly, we also argue that, in the aggregate, the jobs freed up by the elderly are unlikely to make a difference for the young. While employment responses of the elderly are relatively large, the affected group is too small to significantly affect the stock of available jobs. Although with limited statistical power, by exploiting differential effects of the reform across industries and occupations, we are able to reject a one-to-one substitution with those workers with most similar characteristics to the affected population.

Non-contributory, old-age pension programs have a long history in developing countries, and have been generally found to have negative, yet relatively small effects on employment of the elderly (Juárez and Pfütze (2015), de Carvalho Filho (2008), Kaushal (2014)), but these programs are usually less universal and provide lower amounts than the South African *Old Age Pension*. Contrary to Jung and Tran (2012), who model the general equilibrium effects of extending social security programs to informal workers, our analysis is focused on the direct effects on recipients.

The results of this paper are informative for the broader literature about informality in the labor market of developing countries. The main debate remains whether workers choose to work informally rather than in the formal sector, or whether informal employment provides subsistence-level jobs when better employment is lacking. These diverging views have led to the development of different hypotheses on the nature of the informal sector, also known as the “comparative advantage” and the “segmentation” hypotheses (summarized in Günther and Launov (2012)). Until recently, less attention has been paid to the interaction between social security programs and informality, more specifically on how different social policies shape the size and composition of the informal sector, and on how the presence of a significant informal sector might distort incentives within the labor market (Azuara and Marinescu (2013) and Bergolo and Cruces (2014) for non-contributory health insurance to workers and/or relatives; Garganta and Gasparini (2015) for cash transfers to those not in formal employment; Gerard and Gonzaga

(2016) for how informality plays a role in unemployment insurance). We contribute to this growing literature by studying the effects of a strong income effect for workers near retirement, interacted with the “perverse” incentive to switch to informal work. Within the context of older workers in the South African labor market, our results support the view of informality as mostly out of necessity.

There is a large literature on the South African *Old Age Pension*, which can be categorized in two main branches. The first branch of the literature relates to the impact of the OAP on children’s outcomes and, more generally, to the intra-household allocation of resources (Duflo (2000, 2003), Jensen (2003), Ambler (2016)).¹ The labor market effects of the OAP have also been at the center of significant empirical research. Ranchhod (2006) estimates the discontinuity in labor supply and employment for individuals at the age cut-off point, finding large disincentive effects for both men and women. We are able to significantly improve on this identification by taking advantage of the latest reforms in age eligibility.² Evidence on the labor market impacts on other household members is mixed and significantly more complex, mostly because of the issue of selection when household composition changes as a result of pension receipt (Hamoudi and Thomas (2014)). Cross-sectional evidence from Bertrand et al. (2003) revealed the presence of disincentive effects for other members of the household who were not the direct recipients, but migration is also impacted by the grant, as members of OAP recipient households are more likely to migrate (Posel, Fairburn and Lund (2006)). To solve this selection issue, Ardington, Case and Hosegood (2009) make use of panel data to show that households that receive the OAP actually experience an increase in employment, which “occurs primarily through labor migration.” However, recent evidence by Abel (2013), who also uses panel data but at the country level, has challenged these results. Thus, evidence on the employment

¹Duflo (2000, 2003) finds that the extension of the OAP to the African population in the early 90s has led to higher health and nutrition outcomes for girls who live in the same household as their grandmother. Ambler (2016) expands on this argument to show that this is the result of a change in bargaining power within the household upon pension receipt. Jensen (2003) shows that the public pension partly crowds out private transfers from other household members, such as remittances.

²To our knowledge, the only other paper to use these reforms for an analysis of the effects of public pension is Matsuda (2016).

effects for member of the same household is still unclear.

Incorporating the reform within our empirical strategy allows us to better identify the effects of the *Old Age Pension*. The main issue with the existing literature is that, in the cross-section, it is difficult to disentangle the effect of the OAP from other private schemes that might have the same age threshold, and that create discontinuities in the age profile regardless of the social pension. Indeed, we show that ignoring other pension scheme with similar age threshold leads to overestimate the employment response of the elderly by a factor of two, which could potentially confound the impact on other outcomes and affect the computation of income elasticities (Berg (2013), Ambler (2016)).

The outline of the paper is the following: Section 3.2 describes the South African *Old Age Pension*, and the latest reforms. Section 3.4 provides a simple conceptual framework to better frame the results of the paper. Section 3.5 presents the empirical strategy, showing the bias from not accounting for other pension schemes, and the results. Section 3.6 discusses the market level results. Section 3.7 concludes.

3.2 The South African *Old Age Pension*

The *Old Age Pension* (OAP) is a non-contributory pension system in South Africa, originally put in place in the 1920s to provide a minimal level of income to those who were not covered by a retirement plan (Duflo (2000), Woolard and Leibbrandt (2010)). During the Apartheid period, Black South Africans were consistently excluded from most social transfers, and, to a large extent, from public pensions. This occurred in several different ways: the means-test was set at different levels for different races, and was significantly lower for Black and Coloured people. Moreover, the benefits paid when actually eligible only made up one tenth of the amount paid to Whites (Duflo (2003)). Several other administrative loopholes were exploited to keep groups other than White to fully access the grant (for a full account of the history of the OAP in Apartheid South Africa refer to Lund (1993) and Woolard and Leibbrandt (2010)). The means-tests were equalised in

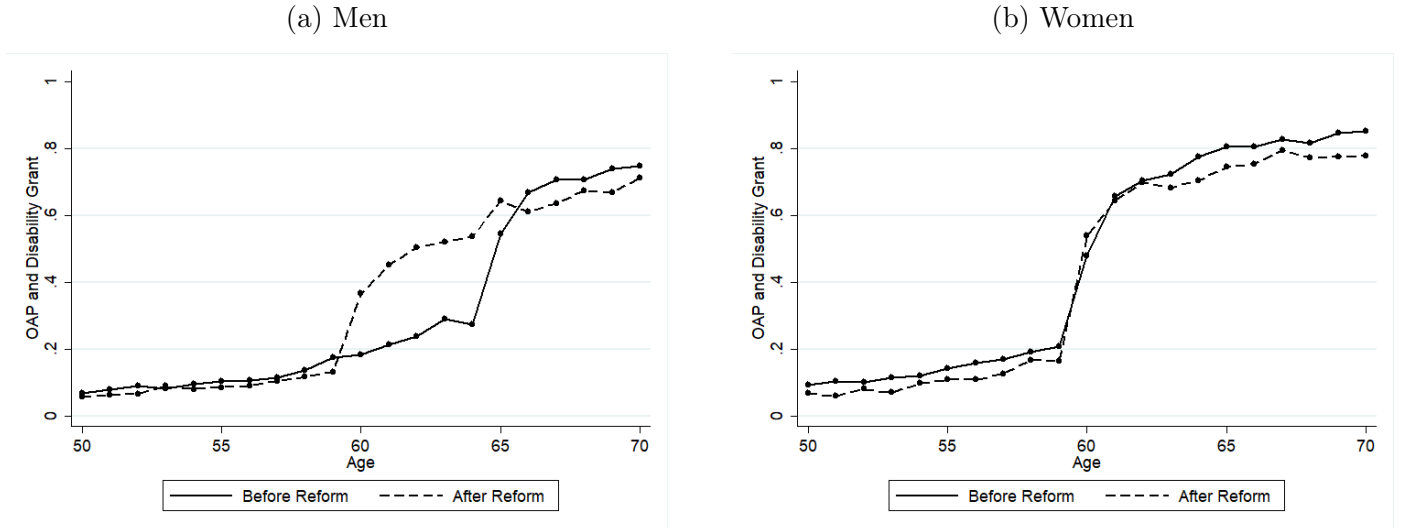
1992, and full “legal” equality was achieved as from 1993, about one year before the first democratic election (Duflo (2003), Woolard and Leibbrandt (2010)).

The *Old age Pension* scheme is the largest social program in South Africa in terms of spending (National Budget Reviews (2007-2013)). Access to the grant is subject to two criteria: (1) an age threshold, where the recipient has to be older than a certain age; and (2) a means-test, where the recipient’s earnings (and overall wealth) have to be lower than a certain threshold. There are no requirements with respect to past contributions or past employment, so it can be characterized as a non-contributory, means-tested pension scheme. Since the extension of the grant to the Black population, the age criterion for women has always remained fixed at 60 years old. Instead, the threshold for men has been set at 65 until July 2008, and then gradually lowered to 60 in the two following years. We present the reforms in the age threshold and the amount paid by the pension over time in Table 3.B.1. The amount of the pension in nominal terms has been constantly increased since 1993 to keep up with inflation. The real amount of the grant is fairly constant, around R 1000 (2010), which is equivalent to approximately 200 \$ PPP *per month*. This is a remarkably high transfer when compared to median income (Woolard and Leibbrandt (2010)), but also to wages, as we will show in the following sections.

The means-test was always set at very high levels. Initially, the proportion of the grant paid was supposed to be a decreasing function of the recipient’s income, which would reach zero at a given threshold. However, it was always understood in a “binary fashion” (i.e. an individual either makes the means-test or he/she does not), hence with a 100% marginal tax rate (Ranchhod (2006)). Since the reform, the means-test has been relaxed further. In the next Section, we show how it compares to monthly earnings in the formal and informal sector. It is not clear to what extent the administration can verify applicants’ income, and probably relies for the most part on what is self-reported at the application stage (Case and Deaton (1998), Ranchhod (2006)). Importantly, private occupational pensions enter the computation for the means-test, and this is likely to be an important discriminating factor. Together with proof of income, individuals have to provide proof of

their private pension (if any) and assets.³ As informal jobs, by definition, do not provide any proof of income, we consider informal labor supply decision (for workers already in the informal sector) to be independent from the means-test.

Figure 3.2.1 – Share of People Receiving the *Old Age Pension* or *Disability Grant* by Age, Before and After Pension Reform



Note: These graphs plot the share of individuals receiving the *Old Age Pension* or the *Disability Grant* within each age bin, for men and women separately. The solid line is for the years before the reform (2002–2007), while the dotted line is for the years after the reform (2010–2015).

Source: Authors' calculations on GHS.

An important, complementary feature of the *Old Age Pension* is the *Disability Grant* (DG). This transfer is received by individuals who have not yet reached the age threshold for the public pension, and carry some sort of physical and/or mental disability.⁴ The *Disability Grant* has exactly the same means-test as the OAP, and pays the same amount. Moreover, it is automatically converted into the pension once the appropriate age is reached (Abel (2013)). The two transfers cannot be received simultaneously, as one condition to get the *Disability Grant* is to be younger than the pension age threshold. Therefore, they have to be thought of as complementary, in that the DG provides some form of income support to people who cannot work before they reach pension age. Figure

³This is clearly stated in the guidelines published by the South African Government: <http://www.gov.za/services/social-benefits-retirement-and-old-age/old-age-pension>

⁴Information about the *Disability Grant* can be found here: <http://www.gov.za/services/social-benefits/disability-grant>

3.B.1 gives the number of beneficiaries of the OAP and *Disability Grant*, and their sum, over time.⁵

In Figure 3.2.1, we plot the share receiving the *Disability Grant* and the *Old Age Pension* by age in the years before and after the reforms in age eligibility (*Source*: General Household Survey). The percentage of men aged 60 to 64 receiving the DG before 2008 was already fairly high before the reform. However, we do observe a large spike between 60 and 64 for the post-reform years. Consistently, this only occurs for men and not for women, for whom everything remains virtually unchanged. Overall, the share of people receiving the OAP is very high, reaching peaks of over 80% for women older than 65, and around 70% for men of a similar age. These rates are even higher when excluding the White and Indian population, who are generally less covered by social grants (Figure 3.B.2.) We also observe that, in proportion, men receive the pension less than for women, which is true both before and after the equalization of the age threshold. The explanation presumably lies in the higher prevalence of formal employment among men, who are therefore more likely to be covered by occupational pension schemes rather than the public pension.

3.3 Data and Descriptive Statistics

3.3.1 Data

Our analysis relies on the use of two datasets: the *General Household Survey* (GHS) and the *Post-Apartheid Labour Market Series* (PALMS). The GHS is an annual, nationally representative survey that covers the period 2002 to 2015, run by the South African statistical agency (STATSA). It includes information on the *Old Age Pension*, the *Disability grant*, but only some basic information about employment and salary. Its labor market

⁵This figure is calculated on administrative data from the Social Security administration (SOCPEN database) found in the yearly National Budget Reviews. As from 2008 when retirement age is lowered, there is a large increase in the number of OAP recipients. This positive trend is partly offset by a decrease in the number of disability grants, because the two transfers are not cumulative. The total number of beneficiaries increases during the period of the reform but not at a faster (nor slower) pace than the previous years, because the faster growth in the number of pensions paid is offset by a decline in the number of disability grants.

section is not detailed enough for the purpose of our analysis, so we only use this dataset to document the effect of the reforms on the share of people receiving the *Old Age Pension*.

The PALMS consists of several appended cross-sections from 1994 to 2015. Over our period of interest (2002–2015), the PALMS is made up of two similar surveys: the *Labour Force Survey* (2002–2007) and the *Quarterly Labour Force Survey* (2008–2015). These surveys are generally considered to be of high quality, and are the main source of labor market information in South Africa. They are also run by STATSA, but the PALMS dataset, which combines them in a coherent way, is put together by researchers at *Data First* of the University of Cape Town.⁶ From 2000 to 2007, the LFS was bi-annual, i.e. interviews were carried out in March and September. As from 2008 to now, together with a change in the sampling framework, the QLFS began to be run each quarter. Overall, both the LFS and the QLFS have much more detailed information on employment, informality and wages (except for the period 2008-2009, where salary information was not asked). The sampling frame is the same as the GHS. The disadvantage of the PALMS is that information about social transfers (including the public pension) is asked only to individuals who are either inactive or unemployed. This makes it impossible to calculate accurately the share of people receiving the OAP on the PALMS, given that employed people can in theory also access it. The advantage of using the PALMS, rather than the original waves from the LFS and the QLFS, is that variables are coded consistently over the whole period, and sampling weights are adjusted to be coherent over time, but the underlying data is the same. To ensure the maximal comparability across the GHS and the PALMS, we exclude the initial waves of the PALMS and focus only on the period 2002–2015.⁷

⁶Detailed information about how the PALMS 3.2 was put together can be found here: <https://www.datafirst.uct.ac.za/dataportal/index.php/catalog/434>

⁷Moreover, it has been reported that the initial waves of the LFS significantly over-measured informal employment. See Kerr and Wittenberg (2015) for discussion.

3.3.2 Informality Definition

Given its detailed labor market section, PALMS data allows for an accurate measurement of informal employment. Throughout the paper, we employ the most objective, and most conservative, measurement of informality. For employees, we consider informal workers those who work without a written contract. The advantage of this measure is its lack of ambiguity, as it is easily known to the worker who answers the survey questions.⁸ For self-employed workers, who, by definition, do not have a work contract, we use information about business registration. These are both standard ways to measure informality in the literature. Throughout the paper, we use the terms informal employment and informal sector interchangeably, but the definition is always based on the presence/absence of work contracts (employees) and business registration (self-employed). Importantly, earnings are measured in gross amounts, before tax and deductions. This is irrelevant for informal workers (for whom net wage=gross wage), and low-paid formal workers, but it certainly enhances the differences between formal and informal wages, in particular at the top of the wage distribution where taxes and social contributions are more relevant.

3.3.3 Descriptive Statistics

Men aged 60–64 make-up between 1.5%-2% of the working-age population (15–65)⁹, and account for a similar share of working-age employment. Around half of employed elderly male were in the informal sector between 2002 and 2007, while this share decreased to one fourth after the reform. The importance of private pension schemes is shown in Figure 3.B.3, where we plot the joint probability to be employed and have an occupational pension in the years before the reform. A significant share of people contributes to some type of pension fund (through the employer). More importantly, this probability drops discontinuously at age 60, suggesting that workers tend to retire at that age. In the

⁸Questions about whether the employer pays social contributions may be more difficult to re-collect, or might not be known to the worker.

⁹This share grows slightly during the period of our study (2002–2015), as life expectancy, which which was well below 60 over the period in South Africa, increased.

next sections, we show that not accounting for these private pension schemes leads to a significant over-estimation of the effects of the *Old Age Pension*, and to drastically different results about the heterogeneous impacts on formal and informal employment.

Table 3.B.2 gives average characteristics by labor market status. The median monthly salary for men aged 60–64 before the reform was R 4462 (R 23 per hour) in the formal sector, and R 1373 (R 7 per hour) in the informal sector. After the reform, it was R 5613 (R 30 per hour) in the formal sector, and R 2004 (R 12 per hour) in the informal sector (in 2010 Rand). The amount of the OAP is around R 1000, and roughly stable over time in real terms (Table 3.B.1). Thus, it can be considered a large amount when compared to median monthly salaries in the informal sector (slightly below the median in the pre-reform period). These orders of magnitude are important to interpret the results of the next sections.

Working hours, on average, are similar between the formal and informal sector, where people usually work around 45 hours, with a slight decline over the period. However, this masks a large heterogeneity in the informal sector, where many workers are employed part-time (15-20%) at less than 30 hours a week. On the contrary, part-time is very rare in the formal sector, where less than 5% work less than 30 hours. This suggests that one of the advantages of informal work might be its higher flexibility. In Figure 3.B.4, we show the distribution of occupations in informal employment by categories of age, for men and women respectively. Elementary occupations make up the most of the informal employment of elderly men. Overall, informality among the elderly appears to be relatively similar to that of their younger counterparts, in the sense that is similarly distributed across occupations and sectors.

The position of the means-test, after the reform, with respect to earnings in the formal and informal sectors is given in Figure 3.B.5. We focus on the period after the reform as this is the relevant one to our analysis, and for which we have better information. As we can see from Figure 3.B.5, the monthly value of the means-test is located around the

median of the distribution of formal monthly salaries, and this remains true across years, despite changes in the nominal level. Importantly, the means-test is located in an area where there is a large overlap between formal and informal wages, suggesting that, at that level of wages, there are (potentially) corresponding jobs in the informal sector that pay a similar amount.

3.4 Conceptual Framework

We use a simple model of sectoral choice and labor supply to help formalize the potential effects of an unconditional pension reform on labor market outcomes. The purpose of this simple framework is to elaborate a few predictions on the behavior of individuals faced with an exogenous non-labor income shock. The set-up is a classic leisure-consumption trade-off, where individual i chooses how much to consume (C), how much to work ($l = T - L$) and in which sector to work ($k \in \{F, I\}$). For simplicity, utility is given by $u_i(C, L, k) = \alpha_i \ln(C) + (1 - \alpha_i) \ln(L) - \mu_i(k)$; people are all endowed with the same amount of time, T , and are faced with wages that may differ for each individual and across sectors, $w_{i,k}$. These wages are fixed, which at least in the short to medium term, is a reasonable assumption. In the absence of pension, non-labor income, m , is zero. The reform consists in offering $m > 0$ to all individuals whose labor earnings ($w(T - L(w, m))$) are below a certain threshold \bar{W} . We further assume working in the formal sector implies a negative utility component ($\mu_i(F) = \mu_i$) which is not encountered when working in the informal sector ($\mu_i(I) = 0$). This is meant to reflect some type of fixed cost to enter the formal sector, while we model the informal sector as “free entry”. The introduction of this parameter does not qualitatively change the predictions of interest of this simple framework, and it is an analytically simple way to model the constraints vs. choice debate in the literature on informality. A more detailed treatment of this framework is given in Appendix 3.A.

Individual labor supply Prior to $m > 0$ being implemented, agents allocate in the formal or in the informal sector. These are people for whom, respectively, $\mu_i < \alpha_i \ln \frac{w_{i,F}}{w_{i,I}}$ (formal-sector workers), and $\mu_i > \alpha_i \ln \frac{w_{i,F}}{w_{i,I}}$ (informal-sector workers). In words, people chose to work in the formal sector when the relative monetary benefits of doing so (as opposed to working in the informal sector), weighted by their preference for consumption (the utility associated with this gap in expected earnings), is larger than the (utility) costs associated with it. The difference in the choice of sector is driven by (i) differences in $\frac{w_{i,F}}{w_{i,I}}$, (ii) differences in μ_i , or (iii) differences in α_i (of course, several mechanisms might be simultaneously at play).

As a result of the pension reform, the non-labor income m is given to everyone choosing to work in the informal sector I , and to people earning less than \bar{W} in the formal sector. The condition for choosing to work in the formal sector rather than the informal one becomes more demanding as the pension reform makes the informal sector more attractive as compared to the formal one (since the means-test does not concern revenue from informal work).

Individuals who would have chosen to work in the informal sector absent the pension either keep working in the informal sector and decrease their hours, or drop out of the labor force when receiving the pension. In other words, the effect on the labor supply of those individuals will be that of an income effect. The magnitude of the drop in the labor supply, relative to its baseline level, is $\Delta_\alpha l(m) = -\frac{1-\alpha_i}{\alpha_i} \frac{m}{w_{i,I}T}$. This expression implies that workers with lower hourly wages will react more to the income shock.

Individuals who would have chosen to work in the formal sector absent the pension split in two subcategories that might react differently to the reform. The first are individuals whose optimal labor income in the absence of the pension, $w_{i,F}l^*(0)$, is smaller than the means-test threshold. Because m is alleviating the budget constraint, it is making the formal sector's wage premium relatively less important, thereby potentially inducing switching from the formal sector to the informal sector; in other words, the magnitude of $\frac{w_{i,F}}{w_{i,I}}$ might not be enough to overturn the costs of working in the formal sector anymore. If

individuals do not switch, their formal labor supply will nonetheless decrease, as a result of the income effect created by the pension. Second, individuals whose optimal labor supply in the absence of the pension is located above the means-test will either (i) be left unaffected by the reform, or (ii) decrease their formal labor supply to locate below the means-test, or (iii) switch to the informal sector (and decrease their labor supply). High levels of $w_{i,F}$ will make m negligible enough that labor supply (and sector choice) is unaffected, with no take-up of the pension. For lower levels of $w_{i,F}$, it is the ratio between $w_{i,F}$ and $w_{i,I}$ that determines whether individuals will decrease their formal labor supply, or switch to the informal sector and decrease their labor supply. This implies that the share of individuals who would switch to the informal sector will largely depend on the counterfactual wage that formal workers would have in the informal sector.

Aggregate labor supply In summary, we expect that the formal labor supply curve should move unambiguously downward. Whether it should be affected at all, remains to be investigated. Even with no change at the extensive margin, we would still expect a response in terms of hours worked. Because of the means-test threshold, it could be that some individuals work a few hours less, so as to qualify for the means-test. For workers at the lower-end of the formal wage distribution, where agents are anyway eligible to the pension as they are under the means-test, we would also expect a decrease in the number of hours worked. Failure to observe either response could be because the pension amount is not sufficient to make people drop out of the labor force, and because hours would not be able to adjust, say if contracts cannot easily be changed.

Instead, the direction in which the aggregate informal supply curve will react is ambiguous. If there is no switching from formal employment, then any drop in the informal employment rate is entirely attributable to the income effect for informal workers. If there is switching from formal employment, any drop in the informal employment rate observed at 60 would be the sum of a drop in the (informal) labor supply of individuals who would have worked informally in the absence of the pension, and of an increase in the informal

labor supply of individuals who would have worked formally in the absence of the pension, but switch to informal work as a consequence of it.

Disentangling the effect of wages The distributions of wages in formal employment and in informal employment are very different (see Figures 3.B.6 and 3.B.7). Absent any substitution from formal to informal employment, differences between the labor supply reactions of both sectors are likely to be driven by wage differences: for smaller expected hourly wages, the non-labor income m that the pension represents has a relatively larger income effect. However, to control for these differences, we can compare the formal and informal labor supply responses at similar level of hourly wages. The labor supply response of informal workers (A), and formal workers (B) are respectively equal to $\Delta_{\alpha_A} l(m) = -\frac{1-\alpha_A}{\alpha_A} \frac{m}{w_{A,I}}$ and $\Delta_{\alpha_B} l(m) = -\frac{1-\alpha_B}{\alpha_B} \frac{m}{w_{B,F}}$. At the same level of wages (i.e. for $w_{A,I} = w_{B,F}$), the differences in labor supply responses are thus attributable to differences between informal and formal labor markets; either with regards to the characteristics of workers who select into them (the preference parameter α and μ in our framework), or with regards to intrinsic characteristics of the jobs in question. We are able to take this prediction to the data as the distribution of hourly wages in the informal and formal employment overlap, although only partly. We go back to the empirical challenges of this heterogeneity analysis in the next section.

3.5 Empirical Analysis

3.5.1 Identification Strategy

In order to capture the labor market effects of the *Old Age Pension*, we make use of the latest reform in eligibility, which only directly affected men. In our estimations, we employ a “modified” Regression Discontinuity Design (RDD), conceptually similar to the “diff-in-disc” estimator proposed in Grembi, Nannicini and Troiano (2016). In this context, the main concern is to avoid bias from other pension schemes with a similar age threshold,

which we show to be sizable. With this in mind, we extend the RDD framework to incorporate the time variation in the age threshold introduced by the reform. Therefore, rather than simply estimating the jump at a given threshold as in a traditional RDD setting, we estimate the difference in the jump before and after the reform. This relaxes considerably the “classic” RDD assumption that, in the absence of the treatment, there should be no discontinuity at the cut-off point. Instead, by performing this estimation, the requirement for identification is that the discontinuity at the threshold would have remained the same in the absence of the reform. This design can be thought of as combining both a “Difference-in-Differences” (DID) and an RDD, but requires significantly weaker assumptions than any of the two methodologies applied independently.¹⁰ All our estimations are run on the subsample of Black and Coloured individuals, during the years 2002 to 2007 and 2010 to 2015. We exclude the reform years (2008–2009), and only focus on the before/after period.

As we also want to derive the magnitude of the bias in the simpler, cross-sectional RD estimator, we also compare the estimates in both strategies. This also allows us to have some results for women, for whom the threshold is not reformed. Formally, we begin by estimating the following equations, separately for men and women, before and after the reform:

$$Y_{i,t} = \delta_t + f(\text{age}_i) + f(\text{age}_i) \times \text{Age}_{(60+)} + \beta_{RD} \text{Age}_{(60+)} + \varepsilon_{i,t} \quad (3.1)$$

where $Y_{i,t}$ is the outcome of interest. δ_t indicates year effects. $f(\text{Age}_i)$ is a function of age, and we test the sensitivity of our results to both a linear and quadratic function. Following Gelman and Imbens (2017), we avoid the use of higher-order polynomials. $\text{Age}_{(60+)}$ is a dummy variable indicating whether the individual is older than 60, which is the cut-off point after the reform. β_{RD} is the discontinuity estimated with the classic RD framework, of which we obtain four values: before and after the reform, for men and for women.

To incorporate the reform in the age threshold into the RD strategy, we modify Equa-

¹⁰The assumption of the DID is that the affected and unaffected age-groups would have evolved in the same way over the period in the absence of the reform.

tion 3.1 in the following form:

$$Y_{i,t} = \delta_t + f(\text{age}_i) + f(\text{age}_i) \times \text{Age}_{(60+)} + f(\text{age}_i) \times \text{Post}_t + f(\text{age}_i) \times \text{Age}_{(60+)} \times \text{Post}_t \\ + \beta_1 \text{Age}_{(60+)} + \beta_{DiDRD} \text{Post}_t \times \text{Age}_{(60+)} + \varepsilon_{i,t} \quad (3.2)$$

where the difference is that we interact the function of age with the Post_t and $\text{Age}_{(60+)}$ variables. Post_t is a binary variable equal to 1 for the years after the reform (2010–2015) and equal to 0 for the years before (2002–2007). In this way, we allow for four different functions of age, on both sides of the threshold and before/after. We also allow for two different discontinuities: $\text{Age}_{(60+)}$, and $\text{Post}_t \times \text{Age}_{(60+)}$. β_{DiDRD} captures the before/after difference in the discontinuity at age 60. It is important to underline that we observe age as a discrete variable, so our design suffers from the limitations of an RD with a discrete forcing variable in terms of inference (Lee and Card (2008), Lee and Lemieux (2010), Kolesár and Rothe (2018)). As suggested by Kolesár and Rothe (2018), we do not cluster by the running variable (i.e. age). Instead, when running Equation 3.2, we obtain robust standard errors by clustering at the race-cohort level. In this way, we want to account for the serial correlation arising from observing some of the same cohorts over time at different points of the age profile.

One of the advantages of this estimation is that it offers a practical solution to “age heaping”, meaning the tendency among survey respondents to round age to the closest multiple of 5 or 10, as already pointed out by Ranchhod (2006). In our setting, this does not pose a problem unless heaping is more or less severe before or after the reform, which can be checked easily by looking at the change in density. More generally, under the assumptions stated before, comparing our estimate for β_{RD} and β_{DiDRD} will give us the effect of the pension “purged” of the bias from other pension schemes, and age heaping. In the results Section, we discuss the sign and magnitude of this bias.

Another concern is the possible presence of anticipation effects. This is a common problem when dealing with age as a forcing variable. Individuals are aware of the age

threshold, and can anticipate or postpone their retirement decision before reaching the age threshold. Given that we focus on the discontinuity at the threshold, our estimation ignores changes that may occur before the threshold as a response to the pension reform. With this in mind, our estimates have to be understood as capturing the anticipated income effect of receiving the pension (relative to not receiving it) rather than as the “absolute” effect of the pension reform. In theory, the effect of anticipation could go either way: if individuals younger than 60 anticipate their retirement decision because of the lower age threshold, then our estimates will be a lower bound. Instead, if individuals postpone their retirement to reach the public pension age, then we would overestimate the effect of the pension. However, the gender dimension of the reform allows to tackle this openly. As women are not affected by the change, we can compare men and women at unaffected age values close to the threshold to measure anticipation. Overall, we do not find any evidence of employment responses occurring before eligibility kicks in.

3.5.2 Results

The results of the RD estimation are presented in Table 3.5.1, and those of the “diff-in-disc” in Table 3.5.2. Graphical evidence of the drop in total employment and by sector is presented in Figure 3.5.1. Before the reform, we estimate a significant and large drop in formal employment at age 60, while informal employment is smooth around the threshold. As men were only receiving the pension from age 65 before the reform, these estimates cannot be the result of the *Old Age Pension*. After the reform, when men are “treated” as from age 60, the drop in employment is more than double that of the pre-reform period, coming in equal parts from formal and informal employment. We observe qualitatively similar evidence for women, who also experience a sizable drop in employment at 60, but constant over the period, again split in roughly equal parts from formal and informal employment. This is in line with the fact that they are not affected by the reform, and are always eligible to the pension from the same age.

Importantly, these results show that the “simple” RD results suffer from a significant

Table 3.5.1 – Old Age Pension and Employment, RDD Results, Quadratic Fit 50–70, PALMS

	Before Reform			After Reform		
	(1) Employed	(2) Informal	(3) Formal	(4) Employed	(5) Informal	(6) Formal
a. Men						
<i>Age</i> ₍₆₀₊₎	-0.0278 (0.0195)	0.0083 (0.0151)	-0.0361** (0.0170)	-0.0907*** (0.0115)	-0.0427*** (0.0084)	-0.0480*** (0.0108)
Mean <i>Y</i> at Age 59	0.41	0.18	0.23	0.46	0.15	0.31
Observations	47,034	47,034	47,034	93,030	93,030	93,030
R-squared	0.1037	0.0214	0.0668	0.1521	0.0242	0.1009
b. Women						
<i>Age</i> ₍₆₀₊₎	-0.0720*** (0.0136)	-0.0427*** (0.0114)	-0.0293*** (0.0096)	-0.0536*** (0.0090)	-0.0360*** (0.0067)	-0.0176** (0.0074)
Mean <i>Y</i> at Age 59	0.25	0.15	0.09	0.31	0.14	0.17
Observations	67,803	67,803	67,803	137,631	137,631	137,631
R-squared	0.1071	0.0539	0.0460	0.1251	0.0419	0.0689

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table gives the results of Equation 3.1 with a **quadratic function**, for men (upper panel), and women (lower panel), on the age window 50–70. The sample is limited to Black and Coloured men and women. We only report the coefficient of interest, β_{RD} . The dependent variables are binary variables for: (1) employed, (2) informally employed, (3) formally employed, such that (1)=(2)+(3), and (4)=(5)+(6). *Mean Y at Age 59* refers to the value of the dependent variable at age 59 in the years after the reform. Robust standard errors in parentheses.

Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Table 3.5.2 – Old Age Pension and Employment, Quadratic Fit 50–70, Diff-in-Disc, PALMS

	Employed	Informal	Formal	
	(1)	(2)	Extensive (3)	Intensive (4)
a. Men				
$Post \times Age_{(60+)}$	-0.0639** (0.0260)	-0.0511** (0.0195)	-0.0128 (0.0261)	-0.4621 (1.2691)
Mean Y at Age 59	0.52	0.14	0.38	13.9
Observations	140,064	140,064	140,064	140,064
R-squared	0.1365	0.0264	0.0956	0.0843
b. Women				
$Post \times Age_{(60+)}$	0.0183 (0.0131)	0.0067 (0.0134)	0.0116 (0.0137)	0.4686 (0.5951)
Mean Y at Age 59	0.34	0.12	0.21	6.5
Observations	205,434	205,434	205,434	205,434
R-squared	0.1242	0.0463	0.0743	0.0656

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table gives the results of Equation 3.2 with a **quadratic function**, for men (upper panel) and women (lower panel) on the age window 50–70. The sample is limited to Black and Coloured men and women. We only report the coefficient of interest, β_{DiDRD} . The dependent variables are binary variables for: (1) employed, (2) informally employed, (3) formally employed, such that (1)=(2)+(3), and (4) hours in formal employment. *Mean Y at Age 59* refers to the value of the dependent variable at age 59 in the years after the reform. Robust standard errors clustered by race-cohort group.

Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

bias. This bias is concentrated on formal employment because this is where the jobs with private pension schemes are. Ignoring the confounding effects from other pension schemes leads to mistakenly attribute a negative effect of the *Old Age Pension* on formal employment. Overall, this brings to an over-estimation of the total employment effect of the pension by a factor between 1.5 and 2.¹¹ Intuitively, the coefficients suggest that the size of the bias is roughly the same for women, but this cannot be tested directly as the age threshold for women is left unchanged.¹²

When we correct for this bias, by employing the estimator of Equation 3.2, the point estimate on total employment decreases significantly (Table 3.5.2). Not surprisingly, our estimate of the employment effects of the pension on direct recipients is more conservative than previous studies, which found effects around twice as large (Ranchhod (2006), Ambler (2016)). The effect on informal employment is unchanged from the previous estimation, between -4 and -5pp., which is relatively large drop: more than a third of informal workers leave their jobs as they become age-eligible to the pension. Looking at Table 3.5.1, we can speculate that the effect is of similar magnitude for women, although the lack of a reform does not allow us to control for potential confounders.

The effect on formal employment is much smaller, and insignificant, but the point estimate is not zero. We show in the next section that some formal workers at the very bottom of the wage distribution stop working. Formal workers also do not seem to significantly respond at the intensive margin, as average working hours in the formal sector stay stable when men become eligible to the *Old Age Pension* (Table 3.5.2, Column 4).¹³ This partly contradicts the prediction of our conceptual framework in Section 4, which suggested some kind of adjustment by formal workers. However, this prediction was based on the assumption that formal workers can freely adjust their hours, which is unlikely to

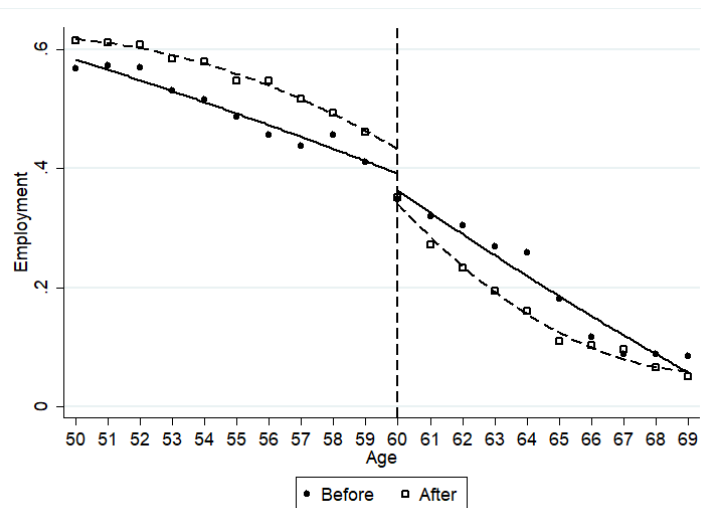
¹¹The RD overestimates the employment effect by a factor of 2 when using a linear function on the 55-64 window, see Table 3.B.3.

¹²The drop in total employment is roughly of the same magnitude as for men, and split in equal parts between formal and informal. If, as for men, there is little or no effect of the pension on formal employment, we can also speculate that the RD estimation over-estimate the effect on employed by a similar magnitude.

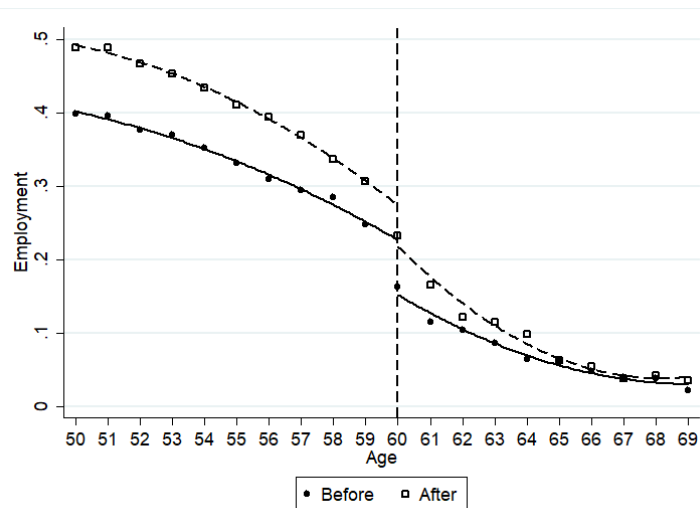
¹³Estimating an effect on the intensive margin of informal employment is more complicated because of the large extensive margin response.

Figure 3.5.1 – Total, Formal and Informal Employment, Before and After, Men & Women

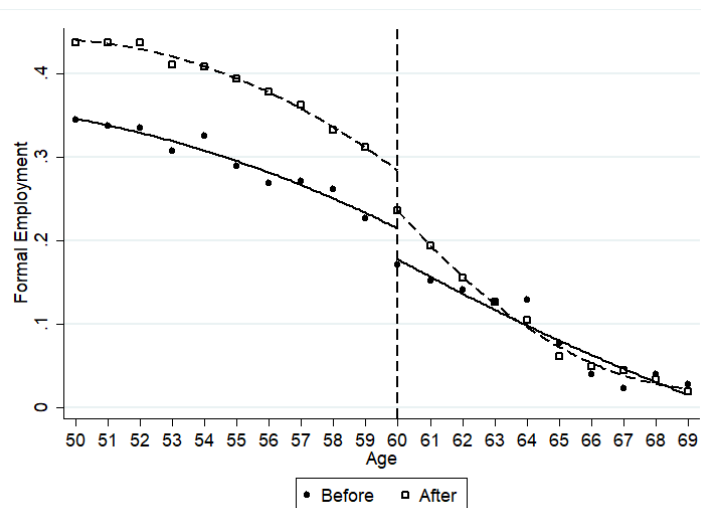
(a) Men: Employment



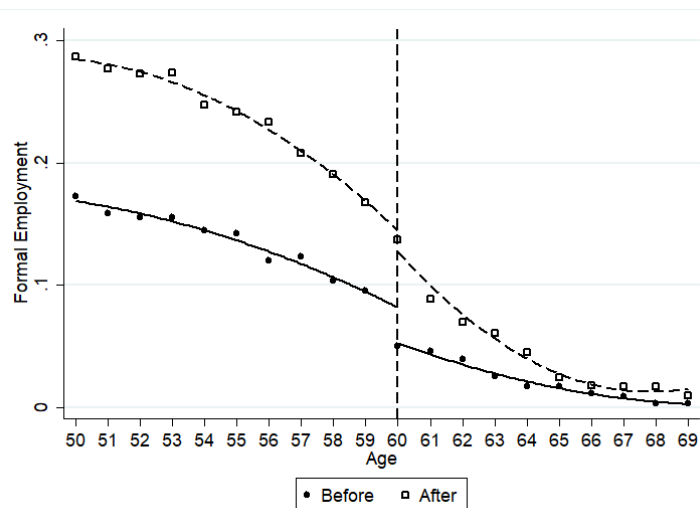
(b) Women: Employment



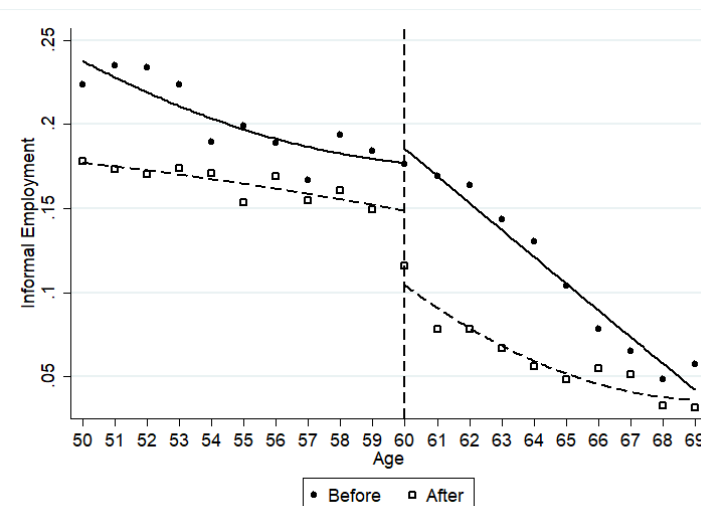
(a) Men: Formal Employment



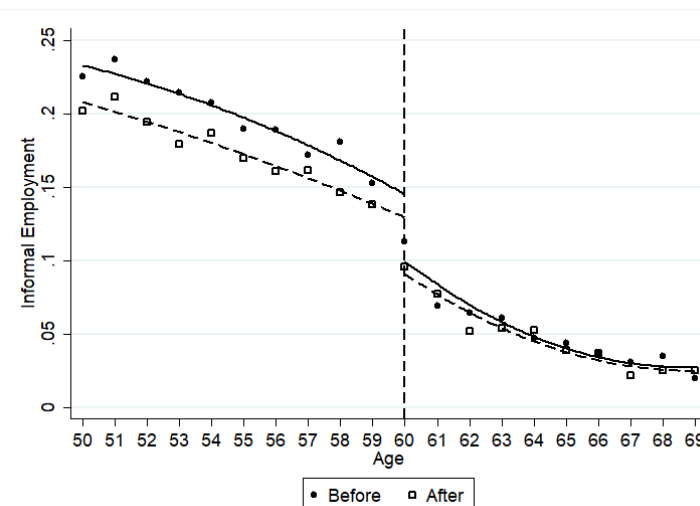
(b) Women: Formal Employment



(a) Men: Informal Employment



(b) Women: Informal Employment



Note: These graphs plot the total employment rate, formal employment rate, and informal employment rate by age for men, in panel (a), and women, in panel (b). Formal and informal employment add up to total employment. The black dots give the mean at each age value before the reform, while the hollow squares after the reform that made men eligible as from age 60. Women have always been eligible as

be the case. If formal jobs are not flexible in terms of working hours, as the absence of part-time jobs would suggest, this could explain the lack of an adjustment at both the extensive and intensive margin.

Overall, the results match the prediction that there should be a net decrease in employment. This decrease comes almost entirely from informal employment, while formal workers do not significantly respond. Importantly, we do not observe a “perverse” effect at play where workers reallocate from formal to informal employment. This would suggest that those in formal employment strictly prefer it to informal work, for example because their counterfactual wages in informal jobs would be significantly lower, so that this change in payoffs across sectors is not large enough to generate any switching. Alternatively, one could posit that there may be barriers that impede this re-allocation across sector. We have assumed that workers can simply enter the informal sector, by modelling it as a “free-entry” sector as is standard in the literature, but this might not be the case.

Heterogeneous Effects by Wage

We now look at how eligibility to the pension has affected workers at different points of the wage distribution. The goal is to test the prediction that the labor supply response of workers should be larger at lower levels of hourly wages. Moreover, we also want to examine differential responses in formal and informal employment *at the same level of wages*. As wages in the informal sector are, on average, lower than in the formal sector, this observed heterogeneity may simply derive from the fact that the OAP is a relatively larger income shock for informal workers.

Ideally, we would estimate the discontinuity at 60 of the *conditional* labor supply at various levels of potential wages. However, only realized wages are observed, i.e. for people who are employed. Alternatively, as outcome variables, we construct different indicator variables equal to 1 if the individual is observed in employment at given levels of wages, and 0 otherwise. The “diff-in-disc” estimator is not well suited for the purpose

of this analysis, as the distribution of real wages changes over time.¹⁴ Differences in the wage distribution over time, weighted by the discontinuity at baseline, would bias our estimates. However, under the assumption that the distribution of wages around the threshold is continuous, and in the absence of any discontinuity prior to the reform, the RD estimator causally identifies the discontinuity in the conditional labor supply (scaled by the density of wages). We show this formally in Appendix 3.C.¹⁵ As there was no discontinuity pre-reform in informal employment, informal labor supply responses by wage can be interpreted directly; with some caveats, we argue one can also interpret the effects on formal employment by wage.

To obtain a counter-factual of the distribution of wages absent the pension reform, we focus on the closest unaffected age group. The wage distribution of 55–59 years old is virtually identical to that of 60–64 years old before the reform (Figures 3.B.6 and 3.B.7). Therefore, we use the distribution of *informal wages* for 55–59 years old to construct quartiles, separately before and after the reform, and estimate heterogeneous effects by wages.

The extensive margin response of informal employment by wage quartile is plotted in Figure 3.5.2 for men, and in Figure 3.5.3 for women. The intuitive prediction of our simple framework is that the effect should be stronger for lower levels of wages, but this is not what we observe. For men, the negative effect on informal employment is constant up to the third quartile of the distribution. Only in the top quartile of the informal wage distribution the effect seems to converge to zero. We observe very similar patterns for women, keeping in mind that they are treated at 60 in both periods. Indeed, extensive margin response by wage is quantitatively and qualitatively similar, with an effect of similar size across the wage distribution, both before and after.

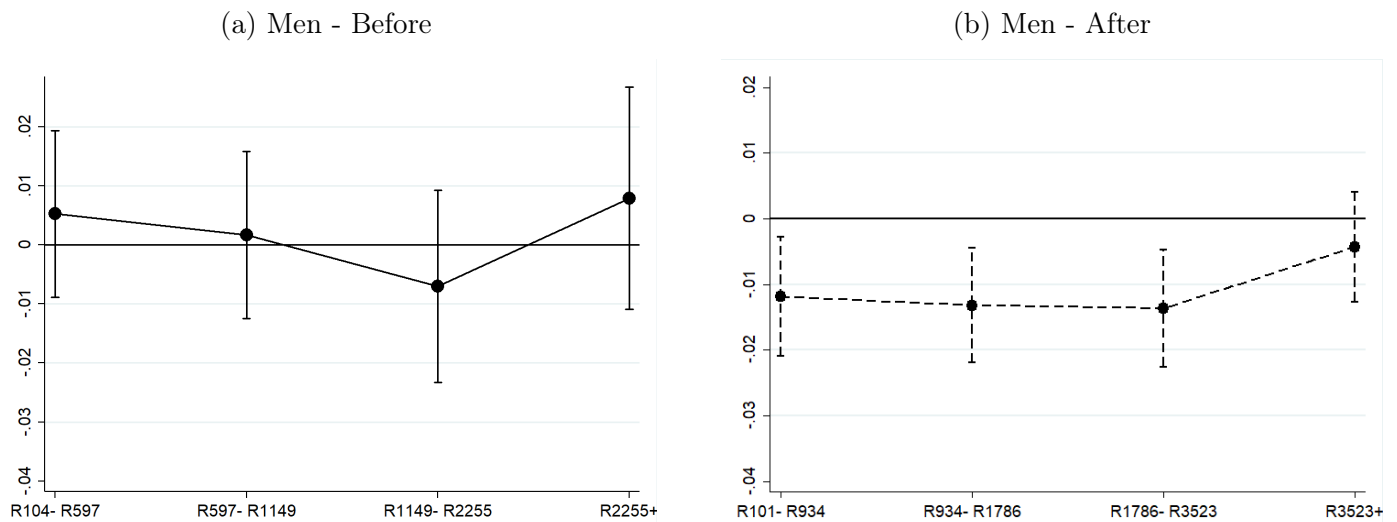
Importantly, the magnitude of the response is the same even for those workers whose

¹⁴Because of growth in real wages over time, the identification assumption that the magnitude of the discontinuity would have stayed the same over time *at each level of wages* is unlikely to hold. For more details see Appendix 3.C.

¹⁵The intuition is that the joint density of labor supply and wages is the product of two continuous densities: the conditional labor supply and the distribution of wages; if the latter is continuous, the discontinuity in the joint density identifies the discontinuity in the conditional labor supply.

full-time equivalent monthly salary, i.e. what they would make in a month given their hourly wage and 43 hours working week, is larger than the amount of the pension (R 1000). In other words, some informal workers are willing to give up more earnings than what they get with the pension. One interpretation of the stability of the effect with respect to hourly wage is that the pension might relieve a subsistence-level constraint. A given share of workers at each point of the informal wage distribution cannot afford not to work. Once this constraint is lifted, they stop working entirely. This would have potentially important implications in terms of welfare, as it suggests that any positive effect on utility might be greater than just the income effect.

Figure 3.5.2 – Effect on Probability to be **Informally** Employed by Quartile of **Informal** Hourly Wage, Quadratic Fit 50–70, RDD, Before and After, Men



Note: These graphs plot the coefficients of Equation 3.1 on the probability to be informally employed within each quartile of the informal hourly wage distribution before and after the OAP reform.

Quartiles are defined according to the distribution of informal hourly wages for the 55–59 years old population in each period. The x-axis is labelled with the bounds of the quartiles for the monthly salary equivalent at a 43 hours working week. The sample includes Black and Coloured men only.

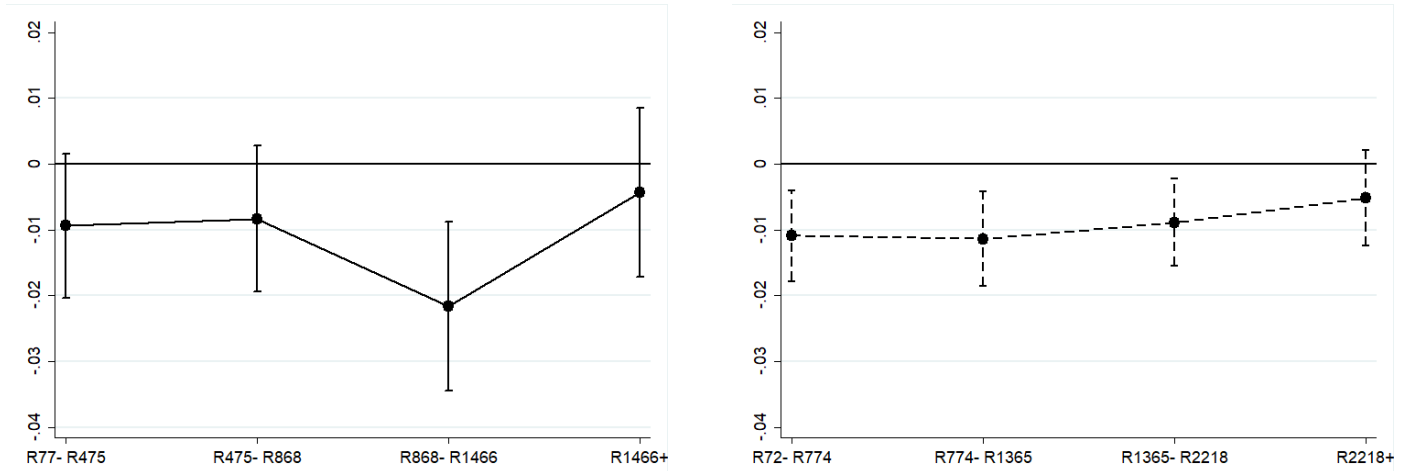
Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Moreover, we are interested in whether the differential response by formal/informal is the result of wages being higher in the formal sector. The question is whether formal workers, for the same wage, respond differently. In order to test this, we look at the effect of the reform on the probability to be formally employed at each quartile of the

Figure 3.5.3 – Effect on Probability to be **Informally** Employed by Quartile of **Informal** Hourly Wage, RDD, Before and After, Women

(a) Women - Before

(b) Women - After

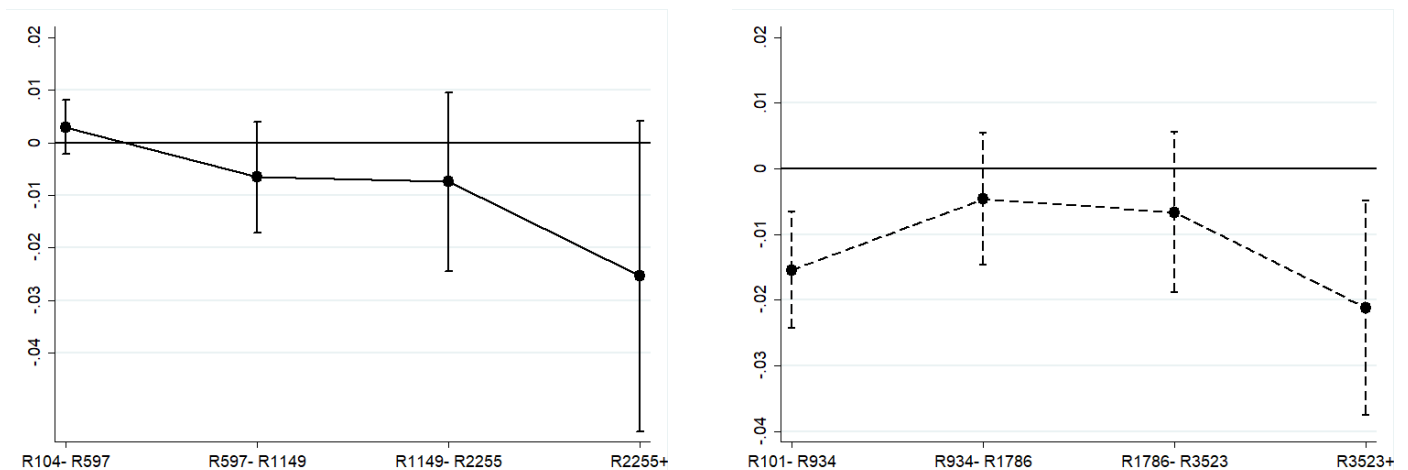


Note: These graphs plot the coefficients of Equation 3.1 on the probability to be informally employed within each quartile of the informal hourly wage distribution before and after the OAP reform. Quartiles are defined according to the distribution of informal hourly wages for the 55–59 years old population in each period. The x-axis is labelled with the bounds of the quartiles for the monthly salary equivalent at a 43 hours working week. The sample includes Black and Coloured women only.
Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Figure 3.5.4 – Effect on Probability to be **Formally** Employed by Quartile of **Informal** Hourly Wage, RDD, Before and After, Men

(a) Men - Before

(b) Men - After



Note: These graphs plot the coefficients of Equation 3.1 on the probability to be formally employed within each quartile of the informal hourly wage distribution before and after the OAP reform. Quartiles are defined according to the distribution of informal hourly wages for the 55–59 years old population in each period. The x-axis is labelled with the bounds of the quartiles for the monthly salary equivalent at a 43 hours working week. The sample includes Black and Coloured men only.
Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

informal wage distribution. Indicatively, the top quartile of the informal distribution roughly begins at the median of the formal wage distribution. The effects on formal employment by wage for men are plotted in Figure 3.5.4, before and after the reform. Interpretation here is complicated by the effects of private pension schemes with the same age threshold. However, these are concentrated in the top quartile of the distribution, which is where we observe a large drop in formal employment, of identical size, both before and after the reform.

With this caveat, we estimate a negative effect for men at the very bottom of the wage distribution, meaning those formal workers whose wage is within the first quartile of the informal wage distribution.¹⁶ Instead, workers formally employed in the 2nd and 3rd quartile of informal wages do not respond to the reform. The coefficient is slightly negative and insignificant, but identical in size to the response at the same quartile before the reform. The coefficient at the top quartile of informal wages (equivalent to formal workers above the median of formal wages) is large and negative, but constant across periods, and easily attributable to private pension schemes.

Importantly, the difference in the total response between formal and informal employment is coming from workers employed in 2nd and 3rd quartile of the informal wage distribution. At these level of wages, informal workers quit their jobs, while formal workers do not. This implies that the heterogeneous effect by sector is not the result of wages being higher in the formal sector, but of differential responses for the same level of wages. As mentioned before, this could be interpreted in two different ways. On the one hand, this could be the result of intrinsic characteristics of the jobs in question at those level of wages. The same worker, with the same characteristics, would react differently at the same wage in a formal or informal job. Alternatively, this can be interpreted as evidence that formal and informal workers are different across other dimensions (for example, in our simple framework, preferences for consumption), which in turn cause these differential responses.

¹⁶This group is only 10% of formal employment, but it is as large as the first quartile of informal workers, given the greater size of formal employment overall.

Other heterogeneity dimensions

The heterogeneity by levels of wage we observe in the previous section suggests to look at other dimensions of heterogeneity, to explore the potential explaining factors of the reform's impact.

We first look at what part of informal employment is driving our results. Indeed, informality as we have used so far collapses unregistered self-employment and non-formalized wage employment. The capacity to adapt one's hours may depend on whether they are self-employed or employees. At age 55 to 64, over the period we consider, informal wage earners represent 55% of all informally employed Black and Coloured men, as shown in Table 3.B.8; informally self-employed individuals make up the 45% remaining. We then run our difference-in-discontinuities estimations of the previous section on each category of informal employment. We show our results in Table 3.B.9. The relative magnitude of the coefficient are very much in line with the share of each category in total informal employment, despite the point estimate being imprecisely estimated for self-employment. Informal wage employment represent 61% of the total effect of the reform, while informal self-employment represents the 39% remaining. This seems to suggest that informal employment reacts homogeneously to the availability of the OAP.

The characteristics of an individual are likely to be correlated with his labor supply response to an income shock. This is notably the case of his place of residence, that can capture part of the characteristics of the labor demand that an individual faces. Unfortunately, there are breaks in the Statistics South Africa various labor force surveys' questionnaires' questions and formulations that do not allow us to observe, for instance, whether the individual lives in a rural or urban environment. Nonetheless, some geographical information is available throughout the survey years. In Table 3.B.10, we distinguish individuals according to whether they live in one of South Africa's 6 metropolitan areas or not¹⁷. These 6 metropolitan areas are home to 24% of the total population of

¹⁷These areas are (their seats in parentheses): Cape Town, Johannesburg, Tshwane (Pretoria), Ekurhuleni (Germiston), eThekweni (Durban), Nelson Mandela Bay (Port Elizabeth). We do not take into account Mangaung (Bloemfontein) and Buffalo (East London), as they only acquired the metropolitan

South Africa. The overall results on informal employment seem mostly driven by non-metropolitan areas. Barring differences in subsample sizes which could be driving the precision of the estimates, this difference between metropolitan and non-metropolitan areas could be driven by different factors. First, consumer prices are often much higher in denser and urbanized areas; this would make the relative importance of the same nominal transfer larger to rural individuals than to urban ones, all other things equal. Second, rural households may be more capable to complement the OAP with self-subsistence activities, which may be measured with difficulty in surveys. Finally, jobs in rural areas may have systematically different characteristics from jobs in urban areas; this is true of their level of (real or nominal) wages, but also of other features, such as their physical burden. All characteristics of jobs being correlated one with another, it is difficult to make a causal interpretation of these heterogeneous effects.

Finally, a crucial aspect of employment decision of the elderly are household dynamics. Unfortunately, the PALMS survey suffers from a sampling limitation that does not allow us to address directly this problem. Indeed, data on the respondent's spouse's characteristics, wages and employment status are only recorded when said spouse is residing with the respondents. Common residence of spouses is likely to be endogenous to the receipt by either of the spouses of the OAP.¹⁸ We nonetheless proceed to distinguish individuals by marital status, and run our empirical analysis on each subsample. We display our results in Table 3.B.11. These results seem to indicate that the bulk of the OAP's effect is driven by married individuals, although the effect for married and non-married individuals are not statistically distinguishable one from another with precision. This could indicate that the level of the OAP's transfer is not large enough for the eligibility to the OAP to allow a single recipient to give up informal work. Living in a couple typically lowers per-capita costs of living, and especially per-capita costs of subsistence, which could explain why

municipality status in 2011, and the data prior to this date does not allow to correctly classify individuals in these areas. Metropolitan areas are a type of government defined by the South African Constitution as to be used for conurbations, "centre[s] of economic activity", areas "for which integrated development planning is desirable", and areas with "strong interdependent social and economic linkages".

¹⁸We actually check that this is the case in the 2011 Census data.

the transfer may be sufficient to push married individuals out of informal work. It could also be that lower-paying informal employment is accepted or undertaken by married individuals to start with. Without additional exogenous variation to exploit, it is difficult to make a causal claim with regards to these various dimensions of heterogeneity.

3.5.3 Robustness Checks

Regression discontinuity designs lend themselves to a wide and well-documented array of robustness checks, which we can easily adapt to the difference-in-discontinuities framework. As mentioned before, “age heaping” is problematic when dealing with age as a forcing variable. In our empirical setting, this is an issue only if age heaping is more or less severe before or after the reform. Therefore, we look at the change in density around the threshold. In the spirit of a McCrary (2008) test, we run Equation 3.2 on the log number of individuals within each cell (age x year). We find no evidence of discontinuous change at the threshold, which suggests this is unlikely to be an issue in our estimations. A standard check in an RD design is also to examine the distribution of pre-determined observables around the threshold. In our modified version, we again focus on the changes in observables before and after the reform, which may indicate selection. For a set of covariates that includes education, race, province, household size and marital status, we find that they evolve smoothly around the threshold (Table 3.B.5).

Lastly, we check the sensitivity of our results to the selected window. In our main estimations, we have shown that the results are very similar with ± 10 window with a quadratic fit, and a ± 5 window with a linear fit (Tables 3.5.2 and 3.B.4). In Figure 3.B.9, we test the sensitivity of the estimates of Equation 3.2 with linear function as we gradually restrict the bandwidth size between ± 10 and ± 5 . For formal employment, the point estimate is negative when the window is large, but then converges to -1pp. as we restrict the window, which is equivalent to the estimate with a quadratic function on a larger window. On the contrary, the point estimate on informal employment remains stable, and roughly varies between -7pp. to -5pp.

Another way to employ our Diff-in-Disc estimator of Equation 3.2 is to exploit the change in discontinuity at 65, which is where the threshold was set for men before the reform. We show the results of this estimation in Table 3.B.6, with the note that coefficients should be interpreted with the opposite sign, as here we capture the effect of a negative discontinuity in the (change in the) share of people receiving the pension. Consistently, the results are very similar (and with opposite sign) of those presented before. The effect on employment is positive and significant, slightly larger but again mostly concentrated on informal employment. The coefficient on formal employment is slightly larger than before, but insignificant and imprecisely estimated.

3.6 Market-Level Effects

Analyzing the effects of a pension reform that concerns several hundreds of thousands of potential beneficiaries raises the question of its potential impact on labor markets in general. Addressing these potential spillovers helps understanding the trade-offs that a wide-scope public policy entails. Indeed, a common motivation for reforming pension schemes is to allow for the replacement of older generations of workers and to provide jobs for younger generations. In the case of South Africa, previous studies have focused their attention on within-household effects of receiving the pension, with mixed results and conclusions (Ardington, Case and Hosegood (2009), Abel (2013)). More generally, evidence on the question of substitution between old and young workers, which has received thorough empirical investigation in developed countries (Salem et al. (2010), Banks et al. (2010), Bovini and Paradisi (2019)), is largely absent in middle-income countries.

If all ages between the ages of 60 and 64 were treated equally, our estimate suggests that the reform drove 25,500 individuals out of the labor force; these are individuals who leave informal and low-paying formal jobs. In this section we address whether some of those jobs are picked up by people who are not directly concerned by the reform. In order to answer this question, we leverage heterogeneity in the effect of the pension by sector

of employment. This follows naturally the heterogeneous results on informal and formal employment rates, as we can expect sectors with more informal employment to be more affected.

However, a very important obstacle for this strategy to yield meaningful results is the limited statistical power provided by the natural experiment we exploit. South African employment is large relative to the direct employment effects of the pension. In the first quarter of 2008, 14.4 million people of all characteristics are employed in South Africa. The effect of the pension reform on the employment of non-beneficiaries only represents, by the largest estimate, 0.2% of the total labor force. More importantly, this also means that those potential effects are small with regards to sampling variability and to the natural temporal variability in the level of employment in the various sectors; in other words, this suggests that the natural experiment we study does not necessarily provide us with enough statistical power to uncover the relevant general equilibrium effects.

We address this difficulty by focusing our attention on the parts of the employment pool that are the closest potential substitutes to workers directly affected, i.e. males aged 55 to 59. The intuition behind this strategy is that people who are very similar in terms of observable characteristics to the treated population may be the ones most likely to be hired as substitutes for them once they exit the labor market.

With this in mind, we carry out the same analysis as before, distinguishing employment across 100 sectors by industry and occupations, and identify the ones where the employment effects are the more pronounced. Figure 3.6.1 shows that a few key sectors of the economy concentrate a large share of the total drop in employment. This heterogeneity allows us to classify the most heavily affected cells as “treated” and the others as “control” by splitting cells into groups according to the weighted quartiles of their point estimate. We define the first quartile (the sectors with the most negative coefficients) as “treated” and the fourth quartile as “control”.¹⁹ Here, identification relies on the assumption that employment in sectors that were differently affected by the reform of the

¹⁹Weighting the distribution of coefficients by the size of the employment of each sector guarantees that treatment and control groups are of equal size.

OAP scheme would have evolved in a similar way absent the reform, after the date it was effectively implemented.

Figure 3.6.1 – Heterogeneous Effects of the Pension Reform by Industry \times Occupation Cells



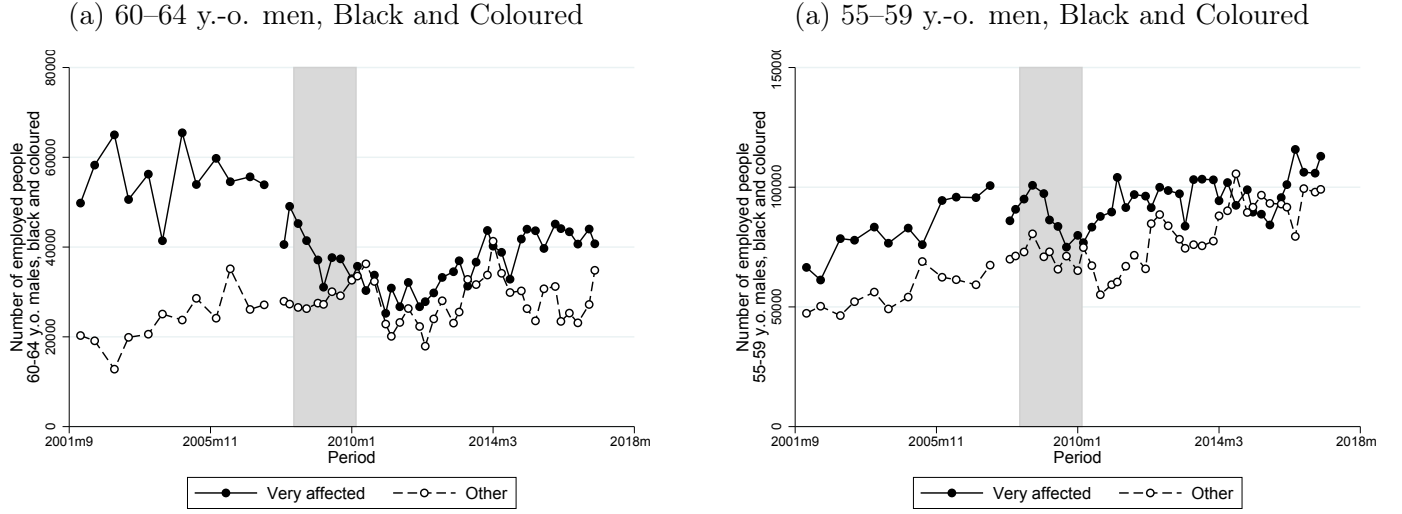
Note: This graph plots the point estimate of Equation 3.2 on the number of elderly workers employed in each industry \times occupation cell (black dots, left axis), and the share that the corresponding industry \times occupation cell represents in total elderly employment (grey bars, right axis). Cells are ordered from left to right by the magnitude of the effect of the pension on the number of employed workers.

Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

The shock in the aggregate labor supply that is induced by the pension reform is essentially a decrease in the supply curve on a given labor market. According to standard labor economic theory, this could translate into three different responses for individuals who are not directly affected by the shock, but can still be deemed as part of the same aggregate labor supply. The first is a change in the labor supply of people who are not working (extensive margin); the second is a change in the labor supply of individuals who are already working (intensive margin), and a third is a change in wages. Therefore, the outcomes of interest are the employment rate, the number of hours worked per employed person, and the wages, for the subgroups of the population we are interested in.

Figure 3.6.2 displays the result on employment rate. With regards to 60–64 Black and Coloured men, we find that on aggregate the treated cells do exhibit a sizable drop in employment, by about 20,000 workers from the third quarter of 2008 to the first quar-

Figure 3.6.2 – Effect of the Reform on the Employment of 60–64 and 55–59, Top vs. Bottom Quartile



Note: These graphs plot the total number of employed individuals in two groups of industry×occupation cells split by the value of the point estimates of the diff-in-disc equation 3.2. The solid line indicates industry×occupation in the first quartile, while the dashed line in the fourth quartile. The shaded area indicates the period of the reform. Panel (a) represents the evolution for 60–64 y.-o. Black and Coloured men. Panel (b) represents the evolution for 55–59 y.-o. Black and Coloured men.

Source: Authors' calculations on PALMS 3.2 (2002–2017).

ter of 2012; while the control is rather stable over the same period. The effect seems to partly disappear over time, as the gap between both groups of sectors widens after a few years. Importantly, the pre-reform evolution of trends in employment is also parallel. After the reform, the employment of the 55–59 year old does not seem to evolve differentially in treated and non-treated sector, which we take as evidence that the jobs left by pension recipients are not being picked up by these workers. Moreover, there is no visible adjustment neither in terms of hours worked nor wages (see Figure 3.B.10).

To confirm these results, we estimate a simple difference-in-difference model on each of these three outcomes. The estimating equation is:

$$Y_{c,t} = \kappa_c + \tau_t + \theta_1 T \times \{2010q2 < t < 2014\} + \theta_2 T \times \{2014 < t\} + \epsilon_{c,t} \quad (3.3)$$

where $Y_{c,t}$ is the outcome of interest in cell c at time t , κ_c and τ_t are cell and time-period fixed effects, T is an indicator variable for being a heavily affected cell, and $\epsilon_{c,t}$ is an

error term, clustered at the cell-level. As the graphical evidence indicates that the effect of the reform might not be constant, we allow the treatment effect θ to be flexible over time and split it in two components, a short- and a medium-term effect.

Results are displayed in Table 3.B.7. We recover the negative estimates on the treated groups, with a magnitude of about 22,000 Black and Coloured 60–64 y.-o. men, a result that is very compatible with the extrapolation of our diff-in-disc results.²⁰ This is mirrored by a large negative drop in the total number of hours worked per week, by about 930,000 hours in the treatment. Assuming no effect at the intensive margin for those still working, this would mean that the people who stop working as a result of the pension were working 45 hours per week, a number that is the mode of the distribution of hours per week in the sample. Weighting each cell by its total size in employment in the pre-reform period does not qualitatively affect these conclusions.

Estimates on the 55–59 y.-o. men do not show any sizable nor statistically significant effect of the reform in sectors where men aged 60–64 were more affected. The positive estimates in the unweighted case – which would be compatible with younger men picking up the jobs that the 60–64 are leaving – are very imprecise, and disappear when weighting each sector for its total employment size. As we are limited in our statistical power to disentangle whether the jobs left by the elderly were picked up by younger individuals, we can only reject a one-to-one substitution with the closest workers (55–59 males). Nonetheless, this indicates that, overall, the number of jobs freed up by the reform is unlikely to have any significant impact on the stock of jobs available to younger workers.

3.7 Conclusion

This paper uses a decrease in the age eligibility threshold for men in the public pension scheme of South Africa to study its effect on the employment of the elderly. We show that not properly accounting for other private schemes leads to significantly overestimate the

²⁰The small discrepancy could come from the fact that we are focusing here only on the two most extreme quartiles of sectors.

effects of the *Old Age Pension*. Despite no explicit requirement to retire when receiving the pension, we provide causal evidence that this reform triggered a large adjustment in old-age male employment at the extensive margin. This occurs because informal workers at all levels of informal wages quit their informal jobs, while formal workers respond only at very bottom of the wage distribution. Our results indirectly suggest that there might be welfare gains for recipients larger than the income effect, as it seems that the pension relieves some sort of subsistence-level constraint for people in informal employment.

These results contribute to the literature on informality in the labor market. The main debate in this literature has been whether workers choose informal employment, or take it due to the lack of better alternatives and as jobs of last resort. The findings of this paper support the second view, although within the context of old age workers in the South African labor market. For workers at age 60 or more, a transfer that is roughly equal to the median wage in the informal sector decreases informal employment by 30-40%; it also does not seem to cause any significant reallocation from formal to informal employment, suggesting that there are no workers for whom this change in incentives is large enough to induce any switching. This relaxes concerns about how the presence of a large informal sector might cause efficiency losses in means-tested programs such as public pensions.

Lastly, this paper aims to contribute to the design of pension systems in developing countries. From these results, one could derive two main policy implications, which may apply to different extents to countries other than South Africa: 1) while there are strong disincentive effects on employment when expanding a public, non-contributory pension scheme, these mostly impact informal jobs. We do not observe any significant crowding out of formal jobs, despite the non-negligible amount of the public transfer; 2) for countries with similar demographic and employment structures to South Africa, pension reforms of this kind are unlikely to free-up a significant number of jobs for the young.

3.A Conceptual framework

An individual i chooses how much to consume (C), how much to work ($l = T - L$) and in which sector to work ($k \in \{F, I\}$). Utility is given by:

$$u_i(C, L, k) = \alpha_i \ln(C) + (1 - \alpha_i) \ln(L) - \mu_i(k)$$

under the budget constraint

$$w_{i,k}T \geq C + w_{i,k}L$$

We assume for simplicity that $\mu_i(I) = 0$ and $\mu_i(F) = \mu_i$. Working in the formal sector implies a negative utility component that is not encountered when working in the informal sector. Prior to the reform, we assume that non-labor income is 0. This does not alter the qualitative results we derive but simplifies the notation. As a result of the reform, the non-labor income m is given to everyone in the informal sector I , and to people earning less than \bar{W} in the formal sector.

Prior to m being implemented, we observe people in the formal and in the informal sector. These are people for whom, respectively,

$$u_i(C^*(I), L^*(I)) < u_i(C^*(F), L^*(F)) \quad (\text{formal-sector workers}) \quad (3.4)$$

or

$$u_i(C^*(I), L^*(I)) > u_i(C^*(F), L^*(F)) \quad (\text{informal-sector workers}) \quad (3.5)$$

where $L^*(k)$, $C^*(k)$, $u(C^*(k), L^*(k))$, $k \in \{I, F\}$ refer respectively to the optimal levels of leisure and consumption, given that sector k has been chosen, and the corresponding

utility. Deriving $L^*(k)$ and $C^*(k)$ follows from first order conditions:

$$L^*(k) = (1 - \alpha_i)T$$

$$C^*(k) = \alpha_i w_{i,k} T$$

Conditions (3.4) and (3.5) thus translate into:

$$\begin{aligned} & \alpha_i \ln(\alpha_i) + (1 - \alpha_i) \ln(1 - \alpha_i) + \alpha_i \ln(w_{i,I}) + \ln(T) \\ & \leq \alpha_i \ln(\alpha_i) + (1 - \alpha_i) \ln(1 - \alpha_i) + \alpha_i \ln(w_{i,F}) + \ln(T) - \mu_i \end{aligned}$$

Selection into sectors is thus determined by:

$$\mu_i < \alpha_i \ln \left(\frac{w_{i,F}}{w_{i,I}} \right) \quad (\text{formal-sector workers}) \quad (3.6)$$

or

$$\mu_i > \alpha_i \ln \left(\frac{w_{i,F}}{w_{i,I}} \right) \quad (\text{informal-sector workers}) \quad (3.7)$$

In words, people choose to work in the formal sector when the relative monetary benefits of doing so (as opposed to work in the informal sector), weighted by their preference for consumption (the utility associated with this gap in expected earnings), is larger than the (utility) costs associated with it. The difference in their choice of sector is driven by (i) differences in $\frac{w_{i,F}}{w_{i,I}}$, (ii) differences in μ_i , or (iii) differences in α_i (of course, several mechanisms might be simultaneously at play). We'll examine the consequences of (i), (ii) and (iii) on the effect of the introduction of m .

Let's focus on the case where $w_{i,F} > w_{i,I}$, so that there is a potential incentive to chose the formal sector in the first place (absent any costs, everybody would chose the formal sector because now, $\alpha_i \ln(\frac{w_F}{w_I}) > 0$). If we don't assume that $w_{i,F} > w_{i,I}$, then trivially, the informal sector is more attractive than the formal sector on all accounts (expected

earnings, costs of entry), and adding non-labor income is not going to change sectoral choice (but is going to have a negative effect on labor supply).

We next detail the effects of introducing $m > 0$ on people's choice of sector. The effects are different according to the characteristics of people (who are defined by the vector $\{w_{i,F}, w_{i,I}, \alpha_i, \mu_i\}$):

- people who chose to work in the informal sector when $m = 0$ (A).
- people who chose to work in the formal sector when $m = 0$ and whose (actual) formal earnings are below the means-test (B).
- people who chose to work in the formal sector when $m = 0$ and whose (actual) formal earnings are above the means-test (C);

(A) Informal workers These workers satisfy condition (3.7)

$$\mu_A > \alpha_A \ln \left(\frac{w_{A,F}}{w_{A,I}} \right)$$

In the case where their pre-reform potential formal earnings are under the means-test, these workers face a new inequality,

$$\mu_A > \alpha_A \ln \left(\frac{w_{A,F}}{w_{A,I}} \right) - \ln \left(\frac{w_{A,F}}{w_{A,I}} \times \frac{w_{A,I}T + m}{w_{A,F}T + m} \right) \quad (3.8)$$

This inequality comes from the new optimal levels of leisure and consumption:

$$L^*(k, m) = (1 - \alpha_i) \left(T + \frac{m}{w_{i,k}} \right)$$

$$C^*(k, m) = \alpha_i (w_{i,k}T + m)$$

We want to compare $u(C^*(F, m), L^*(F, m), F)$ and $u(C^*(I, m), L^*(I, m), I)$, which translate into:

$$\begin{aligned}
& \alpha_i \ln(\alpha_i) + (1 - \alpha_i) \ln(1 - \alpha_i) + \alpha_i \ln(w_{i,I}) + \ln\left(T + \frac{m}{w_{i,I}}\right) \\
& > \alpha_i \ln(\alpha_i) + (1 - \alpha_i) \ln(1 - \alpha_i) + \alpha_i \ln(w_{i,F}) + \ln\left(T + \frac{m}{w_{i,F}}\right) - \mu_i
\end{aligned}$$

which yields

$$\mu_A > \alpha_A \ln\left(\frac{w_{A,F}}{w_{A,I}}\right) + \ln\left(\frac{T + \frac{m}{w_{A,I}}}{T + \frac{m}{w_{A,F}}}\right)$$

and after some rearranging, (3.8). However, trivially, $\alpha_A \ln\left(\frac{w_{A,F}}{w_{A,I}}\right) > \alpha_A \ln\left(\frac{w_{A,F}}{w_{A,I}}\right) - \ln\left(\frac{w_{A,F}}{w_{A,I}} \times \frac{w_{A,I}T+m}{w_{A,F}T+m}\right)$, because $\frac{w_{A,F}}{w_{A,I}} \times \frac{w_{A,I}T+m}{w_{A,F}T+m} > 1$, as $w_{A,F}w_{A,I}T + mw_{A,F} > w_{A,F}w_{A,I}T + mw_{A,I}$; so (3.7) implies (3.8), and informal workers will keep working in the informal sector. Note that the same can be said of informal workers for whom pre-reform potential earnings are above the means-test, since (3.8), for these workers, becomes:

$$\mu_A > \alpha_A \ln\left(\frac{w_{A,F}}{w_{A,I}}\right) - \ln\left(\frac{w_{A,F}}{w_{A,I}} \times \frac{w_{A,I}T+m}{w_{A,F}T}\right) \quad (3.8\text{bis})$$

which is also trivially implied by (3.7).

Labor supply will drop for those workers, as a consequence of the income effect, as leisure goes from

$$L(0) = (1 - \alpha_A)T$$

to

$$L(m) = (1 - \alpha_A)\left(T + \frac{m}{w_{A,I}}\right)$$

thus the labor supply drops from:

$$l(0) = T - L(0) = \alpha_A T$$

to

$$l(m) = T - L(m) = \alpha_A T - (1 - \alpha_A) \frac{m}{w_{A,I}}$$

So that:

$$\Delta_\alpha l(m) = \frac{l(m) - l(0)}{l(0)} = -\frac{1 - \alpha_A}{\alpha_A} \frac{m}{w_{A,I}} < 0$$

(B) Formal workers with $w_{B,F} l^*(F) < \bar{W}$ These workers satisfy (3.6)

$$\mu_B < \alpha_B \ln \left(\frac{w_{B,F}}{w_{B,I}} \right)$$

And face a new inequality

$$\mu_B < \alpha_B \ln \left(\frac{w_{B,F}}{w_{B,I}} \right) - \ln \left(\frac{w_{B,F}}{w_{B,I}} \times \frac{w_{B,I}T + m}{w_{B,F}T + m} \right) \quad (3.9)$$

This comes straightforwardly from the fact that, in the absence of the reform, workers locate on a segment of the (formal-labor related) budget constraint that will be shifted by the additional non-labor income. The relevant part of the formal-wage budget constraint (and the whole informal-wage budget constraint) thus both include a shift by m .

Labor supply will drop for those workers, as a consequence of a income effect, as leisure goes from

$$L(0) = (1 - \alpha_B)T$$

to

$$L_k(m) = (1 - \alpha_B) \left(T + \frac{m}{w_{B,k}} \right)$$

if condition (3.9) still holds, then these workers will stay in the formal sector and work less hours:

$$\Delta_m(l) = -\frac{1 - \alpha_B}{\alpha_B} \frac{m}{w_{B,F}} < 0 \quad (3.10)$$

if condition (3.9) does not hold, then these workers will switch to the informal sector and still work less hours:

$$\Delta_m(l) = -\frac{1 - \alpha_B}{\alpha_B} \frac{m}{w_{B,I}} < 0 \quad (3.11)$$

Note that whether the drop in labor supply for “stayers” (3.10) is larger than that of “switchers” (3.11) is not unequivocal. Indeed, stayers might differ from switchers because of a larger expected wage ratios, $\frac{w_{B,F}}{w_{B,I}}$, because of a larger α_B , or because of a smaller μ_B . All other things equal, if switching to informal work as a result of the pension m is due to a smaller α_B , then the drop in the labor supply of switchers is larger than that of stayers. On the contrary, if it is due to a smaller $\frac{w_{B,F}}{w_{B,I}}$, the drop in labor supply of switchers is likely to be smaller than that of stayers.

(C) Formal workers with $w_{C,F}l^*(F) > \bar{W}$ These workers also satisfy (3.6)

$$\mu_C < \alpha_C \ln \left(\frac{w_{C,F}}{w_{C,I}} \right)$$

However, the effect of the pension reform is somewhat more intricate. Indeed the means-tested non-labor income induces a discontinuity in the formal-wage budget constraint. Whether this makes workers prefer to locate under the means-test is determined by whether this discontinuity is large with regards to the utility of the pre-reform optimum, and whether the means-test is close to the pre-reform optimal formal labor supply. We call $l_{\bar{W}}$ the labor supply that satisfies: $w_{i,F}l_{\bar{W}} = \bar{W}$. If $u(\bar{W} + m, T - l_{\bar{W}}, F) > u_0^*(F)$, where $u_0^*(F)$ is the pre-reform formal-sector optimum, then workers, conditional on choos-

ing to work in the formal sector, will prefer to decrease their labor supply to meet the means-test. Note that this does not necessarily imply that the new formal optimum is a corner solution at the means-test: for some workers whose pre-reform optimum was near the means-test, the income-effect of m is likely to kick in and push them towards a new interior solution.

In the case the new formal optimum is a corner solution at the means-test, whether workers prefer this new formal-sector optimum to switching to informal sector is determined by:

$$u_m^*(I) \leq u(\bar{W} + m, T - l_{\bar{W}}, F) \quad (3.12)$$

where, similarly to above, $u_m^*(I)$ refers to the informal-sector optimum with the pension. On the case the new formal-sector optimum is an interior solution, this inequality becomes:

$$\mu_C \leq \alpha_C \ln \left(\frac{w_{C,F}}{w_{C,I}} \right) - \ln \left(\frac{w_{C,F}}{w_{C,I}} \times \frac{w_{C,I}T + m}{w_{C,F}T + m} \right) \quad (3.13)$$

Note that (3.13) is the same as (3.9). If on the other hand, conditional on choosing to work in the formal sector, workers keep with their pre-reform optimum, this means that the size of the pension is not enough to outweigh the amount of formal wage they would have to give up on to meet the means-test. In this case the formal sector optimum is left unchanged by the introduction of the pension. This is more likely when formal hourly wages are large. The condition to switch to the informal sector is given by:

$$\mu_C \geq \alpha_C \ln \left(\frac{w_{C,F}}{w_{C,I}} \right) - \ln \left(\frac{w_{C,F}}{w_{C,I}} \times \frac{w_{C,I}T + m}{w_{C,F}T} \right) \quad (3.14)$$

3.B Tables and Figures

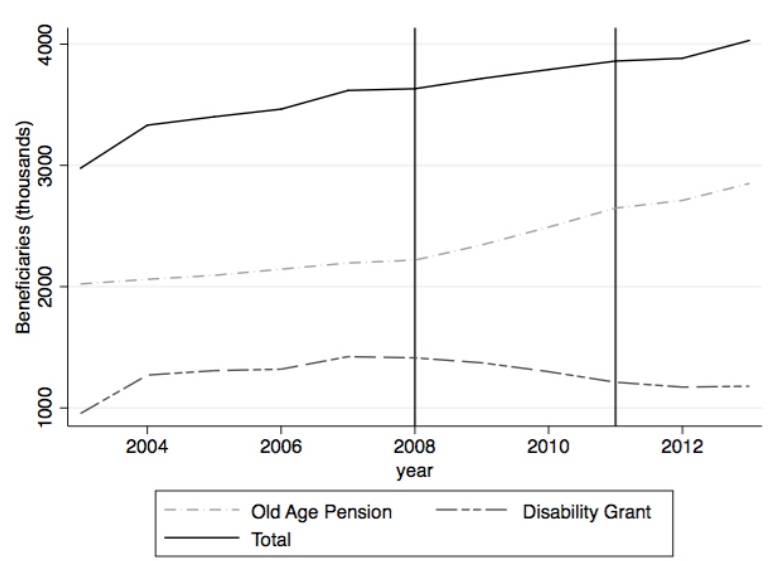
Table 3.B.1 – Evolution of the *Old Age Pension*, 1993–2010

Date	Age threshold		Amount	Amount (R '10)	Means-Test
	Men	Women			
1993	65	60	R 370	R 1008	
1994	65	60	R 390	R 977	R 4440
2000	65	60	R 540	R 902	
2003	65	60	R 700	R 955	R 16,920
2007	65	60	R 870	R 1069	
2008(Q3)	63	60	R 940	R 1049	R 26,928
2009(Q2)	61	60	R 1010	R 1051	R 27,552
2010(Q2)	60	60	R 1080	R 1080	R 31,296
2012	60	60	R 1200	R 1081	R 47,400
2013	60	60	R 1270	R 1081	R 50,340
2014	60	60	R 1350	R 1083	R 61,800
2015	60	60	R 1410	R 1082	R 64,680
2016	60	60	R 1510	R 1087	R 69,000

Note: The age threshold was different for men and women until it was equalized between 2008–10. Amount is presented in current Rand and 2010 Rand separately, CPI data is taken from OECD.stat.

Source: The main sources on OAP amounts are Eyal and Woolard (2011) and the South African government (<http://www.gov.za/services/social-benefits-retirement-and-old-age/old-age-pension>). Reform dates are from the National Budget Reviews (2007-2013). Information on the means-test for 2009 and 2010 is collected from US (2003-2015); Ranchhod (2006) for 2003; Case and Deaton (1998) for 1994; and from SASSA (2010, 2013, 2014, 2016) for all remaining years.

Figure 3.B.1 – Number of *Old Age Pension* and *Disability Grant* Beneficiaries, 2003–2013



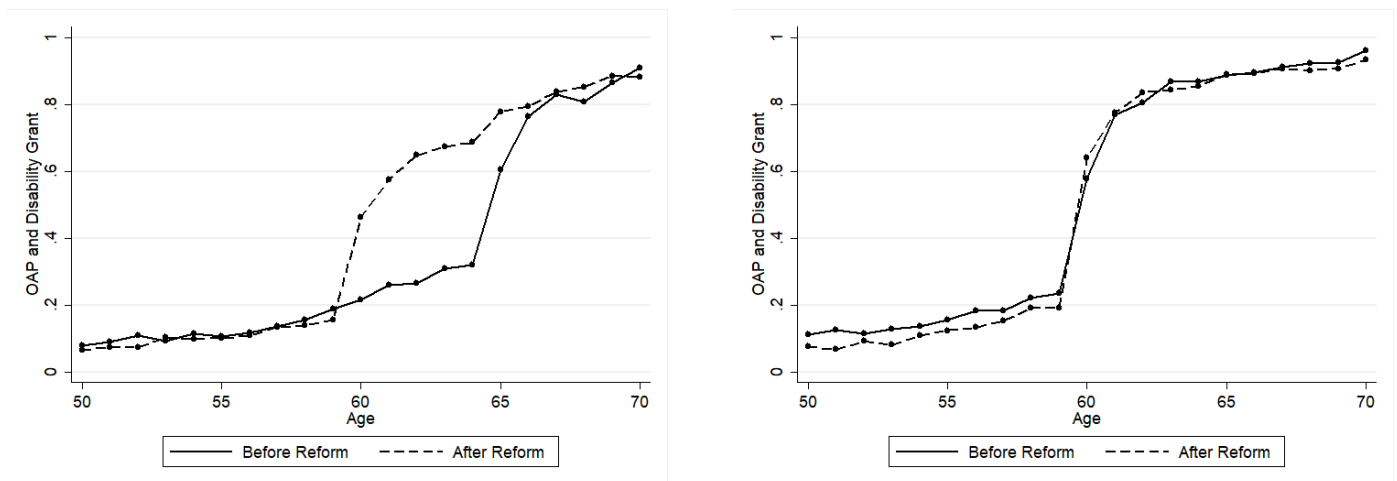
Note: This graph gives the number of beneficiaries of the *Old Age Pension* or of the *Disability Grant* between 2003 and 2013, and their sum (in thousands, 2000 on the graph equals 2 million). The vertical lines indicate the reform period for the OAP, where the age threshold for men was gradually lowered from 65 to 60.

Source: National Budget Reviews (2007–2013). These figures come from administrative data (SOCPEN). When there are small discrepancies for the same year, the latest available estimate is used.

Figure 3.B.2 – Share of People Receiving the *Old Age Pension* or *Disability Grant* by Age, **Before and After** Pension Reform, Black and Coloured Only

(a) Men

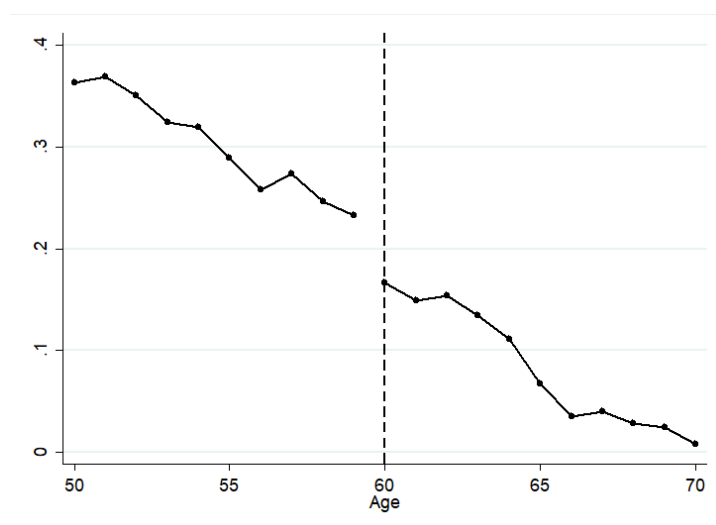
(b) Women



Note: These graphs plot the share of individuals receiving the *Old Age Pension* or the *Disability Grant* within each age bin, for men and women separately. The sample is restricted to the Black and Coloured population. The solid line is for the years before the reform (2002–2007), while the dotted line is for the years after the reform (2010–2015).

Source: Authors' calculations on GHS.

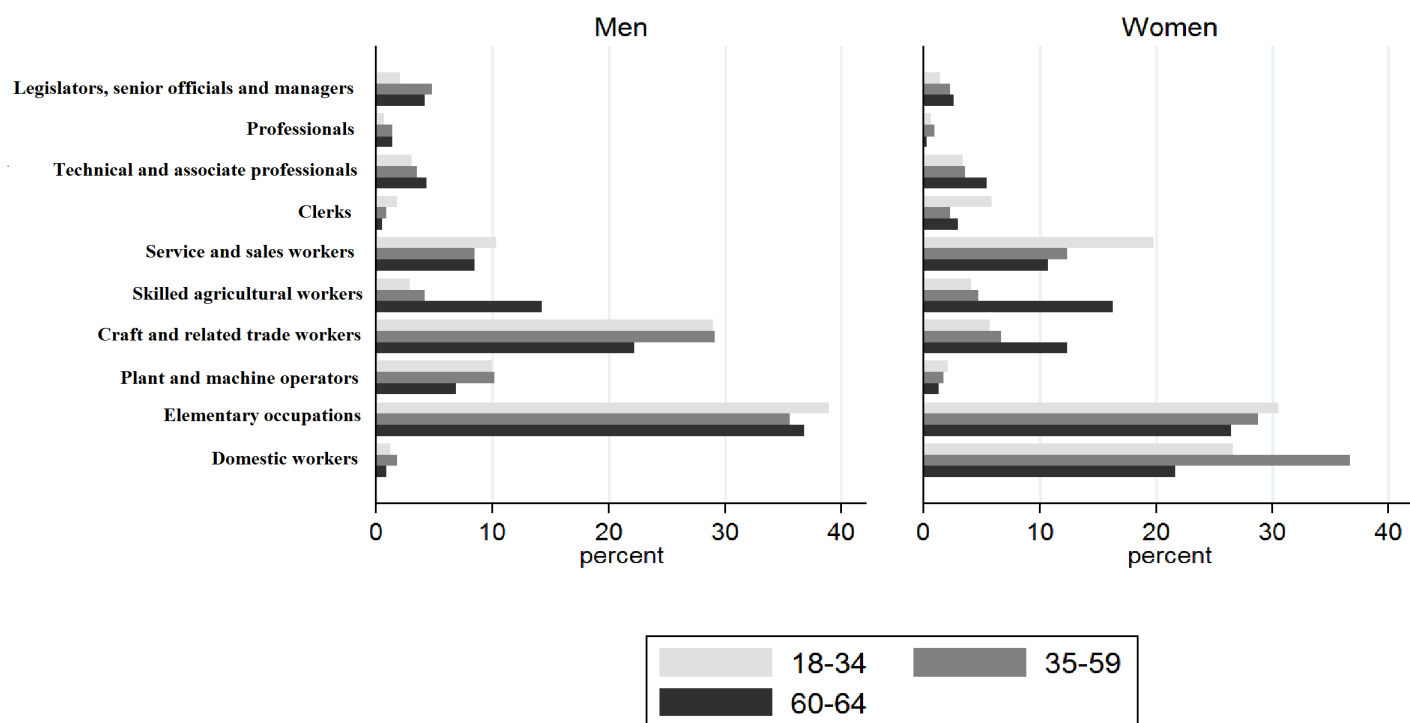
Figure 3.B.3 – Joint Probability to Be Employed and Contribute to a Private Pension, Before Reform



Note: This graph plots probability of being employed *and* contributing to a private pension scheme in the years between 2002 and 2007 for men. During the period, the age eligibility threshold for the *Old Age Pension* was set at age 65 for men.

Source: Authors' calculation on LFS (2002–2007).

Figure 3.B.4 – Informal Employment by Occupation and Age Groups, Men and Women, PALMS

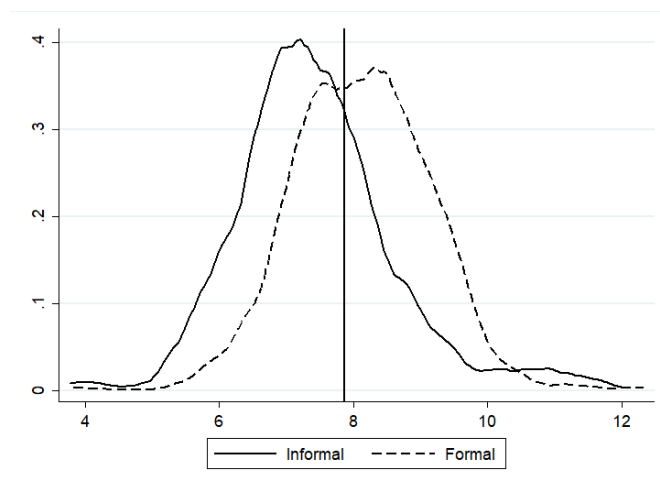


Note: These graphs plot the share of informal employment across occupational categories for different age groups, for men and women respectively. The age groups are 18–35 years old, 35–59, and 60–64.

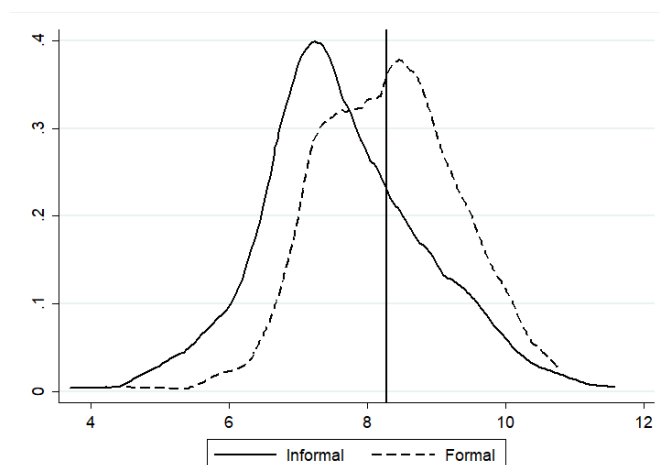
Source: Authors' calculations on PALMS 3.2 (2006–2008).

Figure 3.B.5 – Position of the Means-Test with Respect to Formal and Informal Monthly Earnings, 2010, 2012, 2014

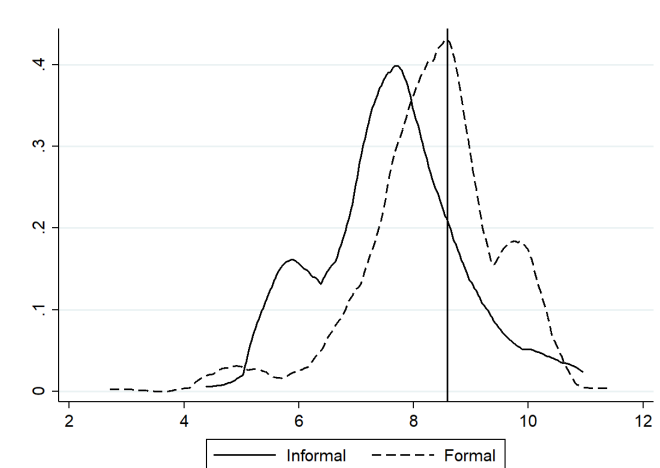
(a) 2010



(b) 2012



(c) 2014



Note: These graphs give the location of the means-test with respect to the distribution of formal and informal monthly earnings in three separate years 2010, 2012, and 2014. Informal monthly earnings are indicated by the solid line, while the dashed line is for formal monthly earnings. The solid vertical lines indicate the position of the means-test in the three years. Earnings and means-test are in nominal Rand amount (not adjusted for inflation).

Source: PALMS v. 3.2 (2010, 2012, 2014).

Table 3.B.2 – Characteristics by Labor Market Status, Men aged 60–64, Before Reform, 2002–2007

<i>Characteristics</i> (pop %)	Informal	Formal	Non- Employed
<i>Proportions</i>	0.13	0.15	0.72
<i>Socio-Demographics</i>			
Black (67.18%)	83.59	65.34	64.52
Married (80.11%)	82.32	87.70	78.09
Education (6.02yrs)	4.68	6.98	6.07
Household Size (4.66 ppl)	4.54	4.11	4.79
<i>Job Characteristics</i>			
Average Weekly Hours	45.7	47.3	
Part-time	15.69	3.01	
Median Monthly Salary (R'10)	1373	4662	
Median Hourly Wage (R'10)	7.3	22.9	
Median Tenure (yrs)	5	16	
Self-Employed	36.08	12.49	
Median Firm Size (ppl)	2–4	20–49	

This table gives average characteristics by labor market status, for men aged 60–64 between 2002 and 2007. On average over the period, 13% of men are informally employed, 15% are formally employed, and 72% do not work. Among those informally employed, 83.59% are Black. Education is measured in years of schooling, and household size in number of household members. Salary and wage information is in 2010 Rand, tenure is the number of years since the start of the current job. Self-employed is the share of the people running their own business. Firm size is a categorical variable for the number of co-workers.

Source: Authors' calculations on PALMS 3.2 (2002–2007).

Table 3.B.3 – Old Age Pension and Employment, RDD Results, Linear Fit 55–64, PALMS

	Before Reform			After Reform		
	(1) Employed	(2) Informal	(3) Formal	(4) Employed	(5) Informal	(6) Formal
a. Men						
$Age_{(60+)}$	-0.0573*** (0.0182)	-0.0007 (0.0140)	-0.0566*** (0.0157)	-0.1097*** (0.0107)	-0.0471*** (0.0077)	-0.0626*** (0.0100)
Mean Y at Age 59	0.41	0.18	0.23	0.46	0.15	0.31
Observations	22,405	22,405	22,405	46,764	46,764	46,764
R-squared	0.0295	0.0069	0.0279	0.0827	0.0176	0.0557
b. Women						
$Age_{(60+)}$	-0.0812*** (0.0124)	-0.0510*** (0.0104)	-0.0302*** (0.0084)	-0.0720*** (0.0083)	-0.0430*** (0.0061)	-0.0290*** (0.0068)
Mean Y at Age 59	0.25	0.15	0.09	0.31	0.14	0.17
Observations	33,026	33,026	33,026	68,977	68,977	68,977
R-squared	0.0609	0.0325	0.0277	0.0677	0.0247	0.0385

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table gives the results of Equation 3.1 with a **linear function**, for men (upper panel), and women (lower panel), on the age window 55–64. The sample is limited to Black and Coloured men and women. We only report the coefficient of interest, β_{RD} . The dependent variables are binary variables for: (1) employed, (2) informally employed, (3) formally employed, such that (1)=(2)+(3), and (4)=(5)+(6). *Mean Y at Age 59* refers to the value of the dependent variable at age 59 in the years after the reform. Robust standard errors in parentheses.

Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

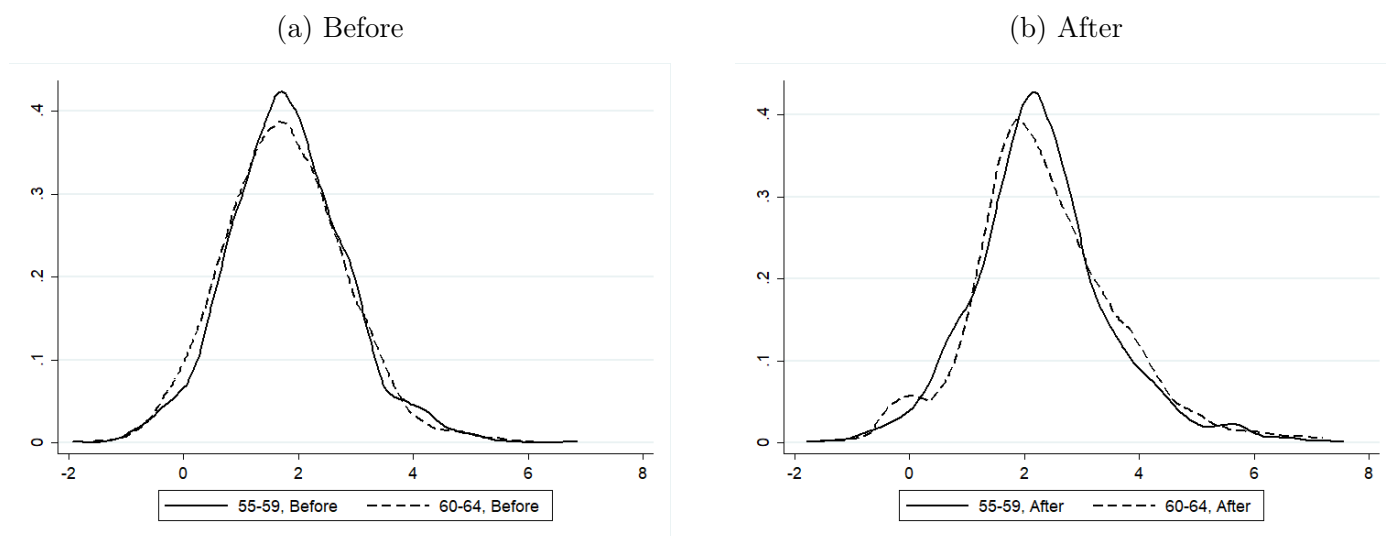
Table 3.B.4 – Old Age Pension and Employment, Linear Fit 55–64, Diff-in-Disc, PALMS

	Employed	Informal	Formal	
	(1)	(2)	Extensive (3)	Intensive (4)
a. Men				
$Post \times Age_{(60+)}$	-0.0539** (0.0214)	-0.0465** (0.0179)	-0.0074 (0.0216)	-0.1027 (1.0786)
Mean Y at Age 59	0.52	0.14	0.38	16.70
Observations	69,169	69,169	69,169	69,169
R-squared	0.0652	0.0177	0.0514	0.0444
b. Women				
$Post \times Age_{(60+)}$	0.0092 (0.0129)	0.0080 (0.0145)	0.0012 (0.0116)	0.1025 (0.5178)
Mean Y at Age 59	0.34	0.12	0.21	8.10
Observations	102,003	102,003	102,003	102,003
R-squared	0.0698	0.0276	0.0462	0.0400

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table gives the results of Equation 3.2 with a **linear function**, for men (upper panel) and women (lower panel) on the age window 55–64. The sample is limited to Black and Coloured men and women. We only report the coefficient of interest, β_{DiDRD} . The dependent variables are binary variables for: (1) employed, (2) informally employed, (3) formally employed, such that (1)=(2)+(3), and (4) hours in formal employment. *Mean Y at Age 59* refers to the value of the dependent variable at age 59 in the years after the reform. Robust standard errors clustered at the race-cohort group.

Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

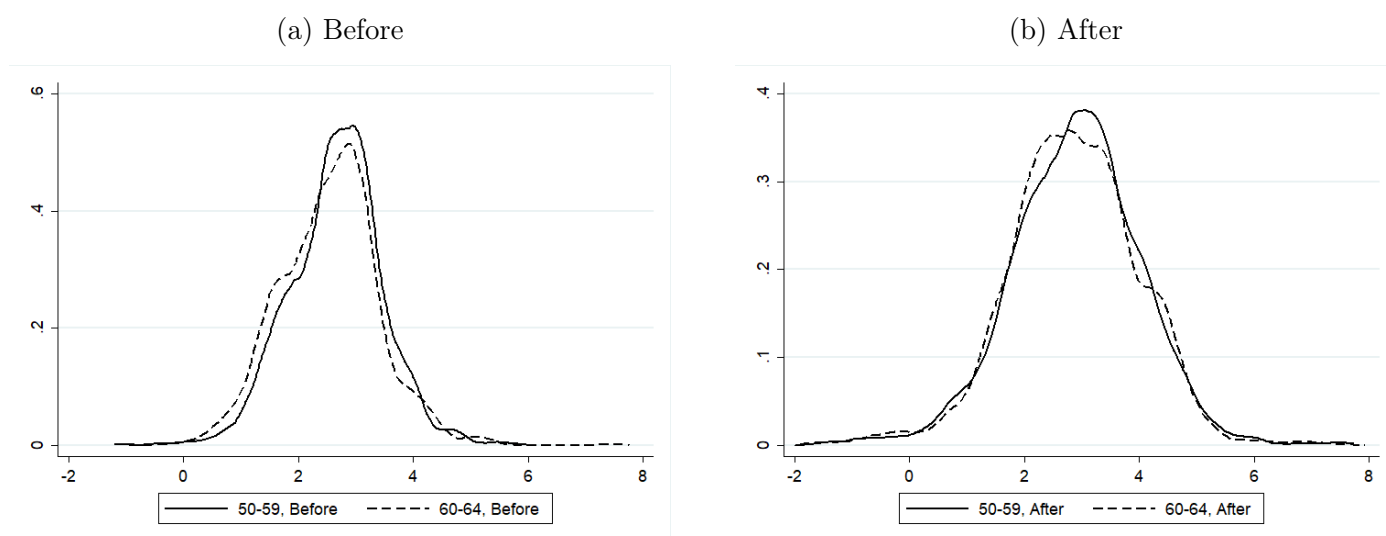
Figure 3.B.6 – Distribution of Informal Hourly Wages, 55–59 vs. 60–64, Before and After



Note: These graphs plot the distribution of log-hourly wage for informal workers by age group before the reform (2002–2007), panel (a), and after the reform (2010–2015), panel (b), adjusted for inflation. The sample includes Black and Coloured males only.

Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Figure 3.B.7 – Distribution of Formal Hourly Wage, 55–59 vs. 60–64, Before and After



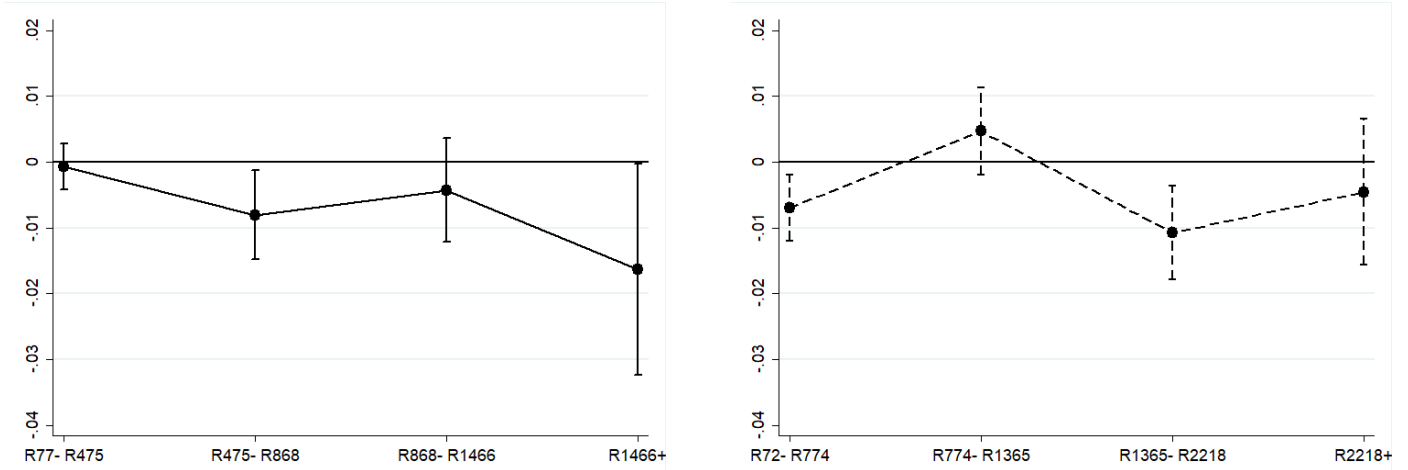
Note: These graphs plot the distribution of log-hourly wage for formal workers by age group before the reforms (2002–2007), panel (a), and after the reform (2010–2015), panel (b), adjusted for inflation. The sample includes Black and Coloured males only.

Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Figure 3.B.8 – Effect on Probability to be **Formally** Employed by Quartile of **Informal** Hourly Wage, RDD, Before and After, Women

(a) Women - Before

(b) Women - After



Note: These graphs plot the coefficients of Equation 3.1 on the probability to be formally employed within each quartile of the informal hourly wage distribution before and after the OAP reform. Quartiles are defined according the distribution of informal hourly wages for the 55-59 years old population in each period. The x-axis is labelled with the bounds of the quartiles for the monthly salary equivalent at a 43 hours working week. The sample includes Black and Coloured women only.

Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Table 3.B.5 – Density and Balancing Test, PALMS, Men Only

	Diff-in-Disc (1)
a. Density	
<i>Log of Individuals</i>	-0.1444 (0.1085)
<i>Observations</i>	240
b. Balancing Test	
<i>Y Variable</i>	
Black	0.0016 (0.0221)
White	-0.0307 (0.0204)
Married	0.0026 (0.0183)
Education	-0.1354 (0.2080)
Cape Province	0.0011 (0.0175)
Household Size	-0.0223 (0.1280)
<i>Observations</i>	166,599

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The sample is restricted to men. This table reports the coefficient β_{DiDRD} of Equation 3.2 with a quadratic function, on the age window 50 to 70. In the upper panel, observations are collapsed at age \times year cell level. The dependent variable is the log of individuals within each cell. In the lower panel, the variables aligned vertically are the dependent variables in the regression. *Education* is equal to the completed years of schooling. *Cape Province* is a binary variable equal to one for an individual residing in either the Western, Eastern, or Northern Cape provinces. Robust standard errors in parentheses.

Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

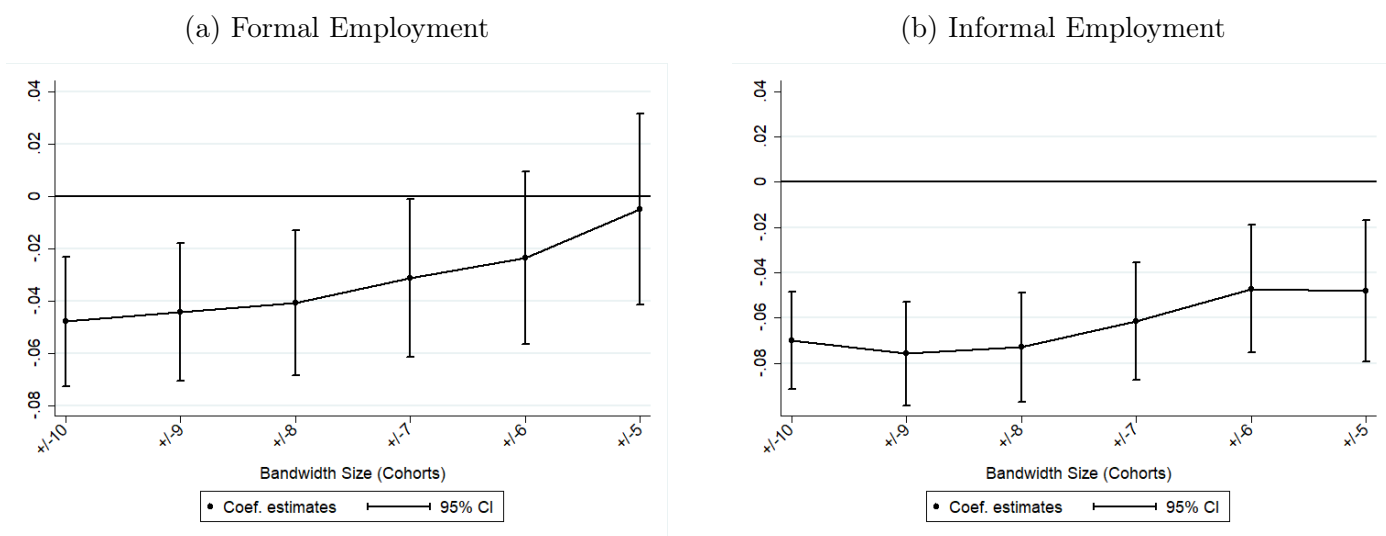
Table 3.B.6 – Old Age Pension and Employment, Linear Fit 60–70, Diff-in-Disc with Threshold at 65, PALMS

	Employed	Informal	Formal	
	(1)	(2)	Extensive (3)	Intensive (4)
a. Men				
$Post \times Age_{(65+)}$	0.0629** (0.0295)	0.0389*** (0.0126)	0.0240 (0.0252)	1.6033 (1.1366)
	66,903	66,903	66,903	66,903
R-squared	0.0764	0.0193	0.0929	0.0815
b. Women				
$Post \times Age_{(65+)}$	-0.0096 (0.0122)	-0.0097 (0.0080)	0.0002 (0.0097)	-0.0355 (0.4305)
Observations	99,982	99,982	99,982	99,982
R-squared	0.0532	0.0146	0.0613	0.0545

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table gives the results of Equation 3.2 with a linear function, for men (upper panel) and women (lower panel) on the age window 60–70. The threshold is set at 65, rather than 60. We only report the coefficient of interest, β_{DiDRD} . The dependent variables are binary variables for: (1) employed, (2) informally employed, (3) formally employed, such that (1)=(2)+(3), and (4) hours in formal employment. Robust standard errors clustered at the race-cohort group.

Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

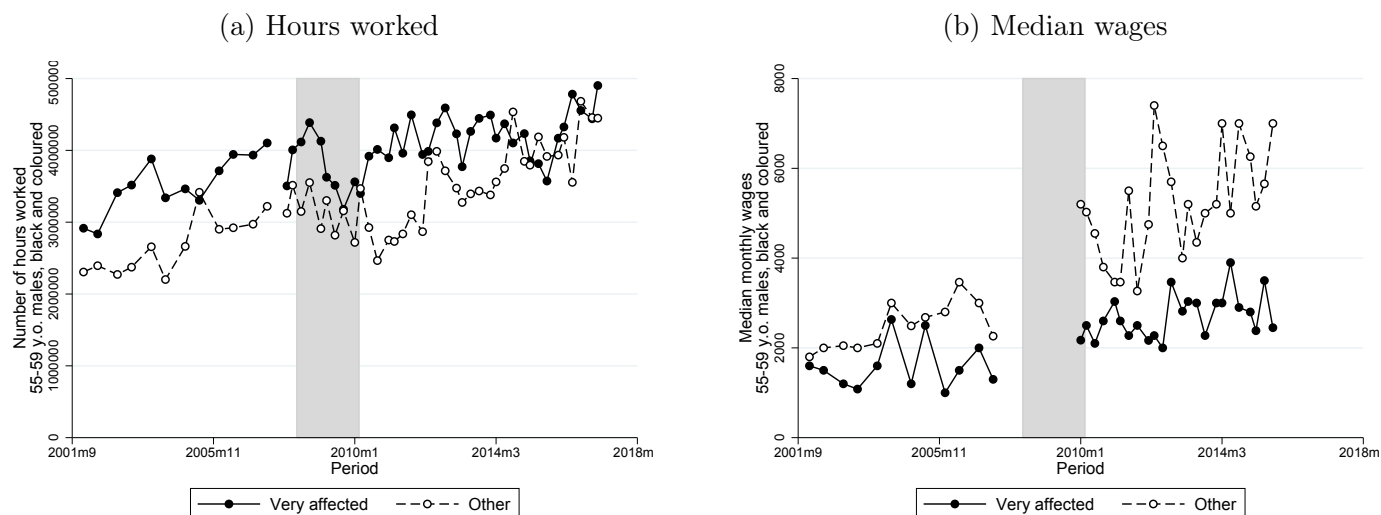
Figure 3.B.9 – Impact on Formal and Informal Employment, Bandwidth Sensitivity, Linear Function, PALMS



Note: These graphs plot the coefficients of Equation 3.2 with a linear function on formal and informal employment for different age windows, with 95% confidence intervals. The x-axis reports the number of age-values included in the estimation, where ± 10 equals the window from 50 to 70 years of age.

Source: Authors' calculations on PALMS (2002–2007 and 2010–2015).

Figure 3.B.10 – Effect of the Reform on Hours Worked and Wages of 55–59 y.o. Black and Coloured Men



Note: These graphs plot the total number of hours worked (Panel (a)) and the median wages (Panel (b)) of 55–59 y.o. Black and Coloured men in two groups of industry \times occupation cells split by the value of the point estimates of the diff-in-disc equation 3.2 in each cell. The solid line indicates industry \times occupation in the top quartile, while the dashed line in the bottom quartile. The shaded area indicates the period of the reform. Wage data is missing for the 2008–2010 period.

Source: Authors' calculations on PALMS 3.2 (2002–2017).

Table 3.B.7 – Labor Outcomes of 55–59 Men; Employment, Hours and Wages

	Unweighted			Weighted		
	60-64 Black & Col.	60-64 All	55-59 Black & Col.	60-64 Black & Col.	60-64 All	55-59 Black & Col.
Panel A: Nb of employed people						
Treatment \times (2010q4 \geq Year < 2014)	-2043.8** (990.4)	-2120.7* (1204.4)	630.9 (1130.5)	-3314.8** (1583.7)	-4067.8** (1856.4)	-1624.8 (1822.6)
Treatment \times (2014 \geq Year \geq 2017)	-1233.7 (957.7)	-1163.3 (1134.0)	319.9 (1150.1)	-2794.1* (1454.9)	-3461.0** (1678.3)	-2831.4 (1921.9)
<i>N</i>	1764	1764	1764	1722	1722	1722
Panel B: Nb of hours worked						
Treatment \times (2010q4 \geq Year < 2014)	-84524.6** (33885.4)	-93976.7** (43854.1)	39030.4 (42554.6)	-124636.7** (51283.5)	-166383.4** (63566.8)	-16145.1 (64835.9)
Treatment \times (2014 \geq Year \geq 2017)	-46549.3 (31997.0)	-47047.1 (40414.5)	21958.8 (41289.6)	-98312.1** (43469.8)	-128169.1** (55193.4)	-79453.2 (65113.8)
<i>N</i>	1764	1764	1764	1722	1722	1722
Panel C: Median monthly wage						
Treatment \times (2010q4 \geq Year < 2014)	-277.9 (1288.0)	-1308.2 (981.6)	-1839.3*** (446.4)	-199.0 (2079.9)	-1744.2** (796.0)	-1634.8*** (442.3)
Treatment \times (2014 \geq Year \geq 2017)	3490.7 (3326.8)	-208.5 (1064.7)	-1677.2*** (506.5)	5719.4 (5281.1)	-922.3 (848.5)	-1542.1*** (466.1)
<i>N</i>	568	568	587	568	568	587

Stars indicate the statistical significance of the coefficients. * : $p < 0.1$, ** : $p < 0.05$, *** : $p < 0.01$.
Each column corresponds to the estimation of Equation 3.3 relative to the population indicated in each column.
Treatment status is defined by the magnitude of the industry \times occupation-specific labor supply response of the 60–64 y-o. Black and Coloured men.
The weights in columns (4) to (6) correspond to the size of the industry \times occupation cells in terms of employment of non-treated groups in 2007.
Standard errors are clustered at the industry \times occupation level.

Table 3.B.8 – Informal employment rate decompositions

	Informal	Informal wage earners	Informal Self-employment
All population aged 15+			
Employment rate	9.5	6.2	3.3
Nb. obs	2573257	2573257	2573257
Male, Black and Coloured, aged 55-64			
Employment rate	11.5	6.4	5.2
Nb. obs	84037	84037	84037

Note: *** : $p < 0.01$ ** : $p < 0.05$ * : $p < 0.1$. Employment rates here are defined as the number of individuals in each category (informally employed, informally self-employed, informal wage-earners), over the corresponding total population (all working age population, or 55-64-year-old Black and Coloured Men.

Source: Authors' calculations on PALMS 3.2 (2002-2007 and 2010-2015)

Table 3.B.9 – Diff-in-disc (Quadratic Fit, 50-70): Heterogeneity by type of labor

	Informal	Informal	
		Wage earners	Self-employed
	(1)	(2)	(3)
$Post \times Age_{(60+)}$	-0.0511** (0.0195)	-0.0314** (0.0138)	-0.0197 (0.0127)
Mean Y at Age 59	0.16	0.09	0.07
Observations	140064	140064	140064
R-squared	0.0264	0.0205	0.0099

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table gives the results of Equation 3.2 with a **quadratic function**, for men (upper panel) and women (lower panel) on the age window 50–70. The sample is limited to Black and Coloured men and women. We only report the coefficient of interest, β_{DiDRD} . The dependent variables are binary variables for: (1) employed, (2) informally employed, (3) formally employed, such that (1)=(2)+(3), and (4) hours in formal employment. *Mean Y at Age 59* refers to the value of the dependent variable at age 59 in the years after the reform. Robust standard errors clustered by race-cohort group.

Source: Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Table 3.B.10 – Diff-in-disc (Quadratic Fit, 50-70): Heterogeneity by type of area

	Employed	Informal	Formal	
	(1)	(2)	Extensive	Intensive
	(1)	(2)	(3)	(4)
a. Men				
(i) Non-metropolitan areas				
$Post \times Age_{(60+)}$	-0.110*** (0.0370)	-0.0761*** (0.0213)	-0.0338 (0.0333)	-1.343 (1.641)
Mean Y at Age 59	0.42	0.16	0.26	11.55
Observations	93232	93232	93232	93232
R-squared	0.1463	0.0288	0.0955	0.0854
(ii) Metropolitan areas				
$Post \times Age_{(60+)}$	-0.0393 (0.0677)	-0.0328 (0.0426)	-0.00648 (0.0559)	-0.335 (2.655)
Mean Y at Age 59	0.52	0.13	0.38	17.03
Observations	28713	28713	28713	28713
R-squared	0.1293	0.0250	0.0879	0.0795
b. Women				
(i) Non-metropolitan areas				
$Post \times Age_{(60+)}$	0.0289 (0.0201)	-0.00421 (0.0183)	0.0331* (0.0175)	1.441** (0.713)
Mean Y at Age 59	0.27	0.14	0.13	5.10
Observations	142150	142150	142150	142150
R-squared	0.1192	0.0438	0.0676	0.0596
(ii) Metropolitan areas				
$Post \times Age_{(60+)}$	0.0201 (0.0716)	-0.00212 (0.0342)	0.0223 (0.0542)	0.649 (2.474)
Mean Y at Age 59	0.36	0.15	0.21	8.60
Observations	37333	37333	37333	37333
R-squared	0.1401	0.0520	0.0746	0.0699

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table gives the results of Equation 3.2 with a **quadratic function**, for men (upper panel) and women (lower panel) on the age window 50–70. The sample is limited to Black and Coloured men and women. We only report the coefficient of interest, β_{DiDRD} . The dependent variables are binary variables for: (1) employed, (2) informally employed, (3) formally employed, such that (1)=(2)+(3), and (4) hours in formal employment. *Mean Y at Age 59* refers to the value of the dependent variable at age 59 in the years after the reform. Robust standard errors clustered by race-cohort group. *Source:* Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

Table 3.B.11 – Diff-in-disc (Quadratic Fit, 50-70): Heterogeneity by marital status

	Employed	Informal	Formal	
	(1)	(2)	Extensive (3)	Intensive (4)
a. Men				
(i) Non-married				
$Post \times Age_{(60+)}$	-0.0185 (0.0590)	-0.0442 (0.0403)	0.0257 (0.0385)	1.544 (1.738)
Mean Y at Age 59	0.31	0.15	0.16	6.93
Observations	39504	39504	39504	39504
R-squared	0.0827	0.0326	0.0467	0.0418
(ii) Married				
$Post \times Age_{(60+)}$	-0.0766** (0.0313)	-0.0553*** (0.0199)	-0.0214 (0.0325)	-0.980 (1.689)
Mean Y at Age 59	0.50	0.16	0.33	14.97
Observations	100560	100560	100560	100560
R-squared	0.1626	0.0255	0.1191	0.1044
b. Women				
(i) Non-married				
$Post \times Age_{(60+)}$	0.0338 (0.0209)	0.0140 (0.0213)	0.0198 (0.0230)	0.862 (0.947)
Mean Y at Age 59	0.32	0.16	0.16	6.17
Observations	117689	117689	117689	117689
R-squared	0.1554	0.0600	0.0842	0.0742
(ii) Married				
$Post \times Age_{(60+)}$	0.00151 (0.0225)	0.00340 (0.0191)	-0.00189 (0.0168)	-0.174 (0.675)
Mean Y at Age 59	0.25	0.12	0.13	4.93
Observations	87745	87745	87745	87745
R-squared	0.0941	0.0345	0.0640	0.0569

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This table gives the results of Equation 3.2 with a **quadratic function**, for men (upper panel) and women (lower panel) on the age window 50–70. The sample is limited to Black and Coloured men and women. We only report the coefficient of interest, β_{DiDRD} . The dependent variables are binary variables for: (1) employed, (2) informally employed, (3) formally employed, such that (1)=(2)+(3), and (4) hours in formal employment. *Mean Y at Age 59* refers to the value of the dependent variable at age 59 in the years after the reform. Robust standard errors clustered by race-cohort group. *Source:* Authors' calculations on PALMS 3.2 (2002–2007 and 2010–2015).

3.C Heterogeneity by Hourly-Wage Levels

We detail in this Appendix the issues with carrying out a difference-in-discontinuities estimation procedure on employment dummies for different levels of wages, and why and under which assumptions a simpler regression discontinuity design is more appropriate and easier to implement.

We call Y_{01} , Y_{00} , Y_{11} and Y_{10} the potential outcomes of the individual when he receives the treatment in $T = 0$, when he does not (still in $T = 0$), and when he receives the treatment in $T = 1$, and when he does not. Let us define the treatment as being eligible to the pension. D is a dummy variable for receiving the treatment, T is a time variable equal to 1 if the period is post-reform, and 0 if it is pre-reform, and X represents the age of the individual: $D = \mathbb{1}\{X \geq 60 \cap T = 1\}$. $\tau = E[Y_{11} - Y_{10}|X = 60]$ measures the effect that we are interested in, which is the local effect of the pension at 60, for people who are 60 after the reform is implemented. We do not observe Y_{01} , Y_{00} , Y_{11} and Y_{10} for all X and T , but only:

$$Y = (1 - T)(1 - D)Y_{00} + (1 - T)DY_{01} + DTY_{11} + (1 - D)TY_{10}$$

The classical RDD estimand is defined as:

$$\tau_{RDD} = E[Y|T = 1, D = 1, X = 60] - \lim_{x \rightarrow 60^-} E[Y|T = 1, D = 0, X = x]$$

which, under the continuity assumption (\star) that $\lim_{x \rightarrow 60^-} E[Y|T = 1, D = 0, X = x] = E[Y|T = 1, D = 0, X = 60]$, identifies τ :

$$\begin{aligned} \tau_{RDD} &= E[Y|T = 1, D = 1, X = 60] - E[Y|T = 1, D = 0, X = 60] \\ &= E[Y_{11}|T = 1, D = 1, X = 60] - E[Y_{10}|T = 1, D = 0, X = 60] \\ &= E[Y_{11} - Y_{10}|T = 1, D = 1, X = 60] \end{aligned}$$

$$= \tau$$

A potential problem with the RDD estimand – and justification for using the difference-in-discontinuities – lies in the fact that (\star) might not be verified: there could be a discontinuity at the threshold in the absence of the pension reform, i.e. in the absence of a change in D . However, at the price of making two assumptions, we can leverage the data from the pre-reform period, which yields information on this discontinuity. These assumptions $(\star\star)$ and $(\star\star\star)$ write down as:

$$\begin{aligned}
(\star\star) \quad & E[Y_{10}|T = 1, D = 0, X = 60] - \lim_{x \rightarrow 60^-} E[Y_{10}|T = 1, D = 0, X = x] \\
= \quad & E[Y_{00}|T = 0, D = 0, X = 60] - \lim_{x \rightarrow 60^-} E[Y_{00}|T = 0, D = 0, X = x] \\
(\star\star\star) \quad & \lim_{x \rightarrow 60^-} E[Y_{11} - Y_{10}|T = 1, X = x] \\
= \quad & \lim_{x \rightarrow 60^+} E[Y_{11} - Y_{10}|T = 1, X = x]
\end{aligned}$$

(provided those limits exist.) Assumption $(\star\star)$ means that the discontinuity in the absence of treatment is constant over time: absent the reform, the same discontinuity, if any, would be observed in $T = 1$ as is observed in $T = 0$; it is similar to the continuity assumption (\star) . Assumption $(\star\star\star)$ expresses the fact that the discontinuity for the treated and control group are equal.

And this leads to the now well-known difference-in-discontinuities strategy:

$$\begin{aligned}
\tau_{\text{DiDisc}} = & E[Y|T = 1, D = 1, X = 60] - \lim_{x \rightarrow 60^-} E[Y|T = 1, D = 0, X = x] \\
& - (E[Y|T = 0, D = 0, X = 60] - \lim_{x \rightarrow 60^-} E[Y|T = 0, D = 0, X = x])
\end{aligned}$$

as documented for instance in (Grembi, Nannicini and Troiano, 2016).

Suppose now that we wish to distinguish the employment response by the levels of hourly wages: $\tau_{\bar{w}} = E[Y_{11} - Y_{10}|T = 1, X = 60, w = \bar{w}]$. We do not observe potential wages but only realized ones. In other words, rather than observing the conditional labor

supply at certain levels of hourly wages, we observe the joint distribution of wages and labor supply. Naively running the difference-in-discontinuities on a dummy equal to 1 if an individual is employed at the level of wage \bar{w} and 0 otherwise is equivalent to taking the following estimand for $\tau_{\bar{w}}$:

$$\begin{aligned}
\tau_{\text{naive}} &= E[Y, w_{T=1} = \bar{w} | T = 1, D = 1, X = 60] \\
&\quad - \lim_{x \rightarrow 60^-} E[Y, w_{T=1} = \bar{w} | T = 1, D = 0, X = x] \\
&\quad - \left(E[Y, w_{T=0} = \bar{w} | T = 0, D = 0, X = 60] \right. \\
&\quad \left. - \lim_{x \rightarrow 60^-} E[Y, w_{T=0} = \bar{w} | T = 0, D = 0, X = x] \right) \\
&= P(Y, w_{T=1} = \bar{w} | T = 1, D = 1, X = 60) \\
&\quad - \lim_{x \rightarrow 60^-} P(Y, w_{T=1} = \bar{w} | T = 1, D = 0, X = x) \\
&\quad - P(Y, w_{T=0} = \bar{w} | T = 0, D = 0, X = 60) \\
&\quad + \lim_{x \rightarrow 60^-} P(Y, w_{T=0} = \bar{w} | T = 0, D = 0, X = x) \\
&= P(Y | T = 1, X = 60, w_{T=1} = \bar{w}) P(w_{T=1} = \bar{w} | T = 1, X = 60) \\
&\quad - \lim_{x \rightarrow 60^-} P(Y | T = 1, X = x, w_{T=1} = \bar{w}) P(w_{T=1} = \bar{w} | T = 1, X = x) \\
&\quad - P(Y | T = 0, X = 60, w_{T=0} = \bar{w}) P(w_{T=0} = \bar{w} | T = 0, X = 60) \\
&\quad + \lim_{x \rightarrow 60^-} P(Y | T = 0, X = x, w_{T=0} = \bar{w}) P(w_{T=0} = \bar{w} | T = 0, X = x)
\end{aligned}$$

Let's assume that $\lim_{x \rightarrow 60^-} P(w_{T=0} = \bar{w} | T = 0, X = x) = P(w_{T=0} = \bar{w} | T = 0, X = 60)$ on the one hand, and $\lim_{x \rightarrow 60^-} P(w_{T=1} = \bar{w} | T = 1, X = x) = P(w_{T=1} = \bar{w} | T = 1, X = 60)$ on the other, so that wage distributions in each time period are continuous at the threshold. Then:

$$\tau_{\text{naive}} = (P(Y | T = 1, X = 60, w_{T=1} = \bar{w}) - \lim_{x \rightarrow 60^-} P(Y | T = 1, X = x, w_{T=1} = \bar{w}))$$

$$\begin{aligned}
& \times P(w_{T=1} = \bar{w} | T = 1, X = 60) \\
& - (P(Y | T = 0, X = 60, w_{T=0} = \bar{w}) - \lim_{x \rightarrow 60^-} P(Y | T = 0, X = x, w_{T=0} = \bar{w})) \\
& \times P(w_{T=0} = \bar{w} | T = 0, X = 60) \\
= & \left[P(Y | T = 1, X = 60, w_{T=1} = \bar{w}) - \lim_{x \rightarrow 60^-} P(Y | T = 1, X = x, w_{T=1} = \bar{w}) \right. \\
& \left. - (P(Y | T = 0, X = 60, w_{T=0} = \bar{w}) - \lim_{x \rightarrow 60^-} P(Y | T = 0, X = x, w_{T=0} = \bar{w})) \right] \\
& \times P(w_{T=1} = \bar{w} | T = 1, X = 60) \\
& + \left[P(Y | T = 0, X = 60, w_{T=0} = \bar{w}) - \lim_{x \rightarrow 60^-} P(Y | T = 0, X = x, w_{T=0} = \bar{w}) \right] \\
& \times (P(w_{T=1} = \bar{w} | T = 1, X = 60) - P(w_{T=0} = \bar{w} | T = 0, X = 60)) \\
= & \tau_{\bar{w}} \times P(w_{T=1} = \bar{w} | T = 1, X = 60) \\
& + \left[P(Y | T = 0, X = 60, w_{T=0} = \bar{w}) - \lim_{x \rightarrow 60^-} P(Y | T = 0, X = x, w_{T=0} = \bar{w}) \right] \\
& \times (P(w_{T=1} = \bar{w} | T = 1, X = 60) - P(w_{T=0} = \bar{w} | T = 0, X = 60))
\end{aligned}$$

As can be seen in the expression above, the issue with this strategy is that the naive estimand is not equal to $\tau_{\bar{w}}$, nor to $\tau_{\bar{w}}P(w_{T=1} = \bar{w})$, which is arguably an object of interest, as it is equal to $\tau_{\bar{w}}$, weighted by the density of the corresponding level of wages²¹. The pre-reform discontinuity enters as a second term, weighted by the difference in densities of the level of wage of interest. Unless we can make the very strong assumption that the distribution of wages is stable over time, this term is different from 0 and biases our estimates of $\tau_{\bar{w}}$.

However, the existence of a pre-reform discontinuity is something we can check. Therefore, under the assumption the discontinuity in the absence of the reform, if any, is time-invariant, whether the difference-in-discontinuity strategy is required to estimate τ can be decided upon by looking at the pre-reform discontinuity. Provided that we can assume that there is no discontinuity in the absence of the reform, the regression discontinuity

²¹In any case, the latter density is observed, and can thus be estimated.

framework is enough to estimate the aggregate treatment effect. This is helpful, as using the joint distribution of employment and wages in a simpler regression discontinuity framework is less problematic than in the difference-in-discontinuity framework. Indeed, it writes down as:

$$\begin{aligned}
\tau_{RDD, \bar{w}} &= E[Y, w_{T=1} = \bar{w} | T = 1, X = 60] - \lim_{x \rightarrow 60^-} E[Y, w_{T=1} = \bar{w} | T = 1, X = x] \\
&= P(Y, w_{T=1} = \bar{w} | T = 1, X = 60) - \lim_{x \rightarrow 60^-} P(Y, w_{T=1} = \bar{w} | T = 1, X = x) \\
&= P(Y | T = 1, X = 60, w_{T=1} = \bar{w}) P(w_{T=1} = \bar{w} | T = 1, X = 60) \\
&\quad - \lim_{x \rightarrow 60^-} P(Y | T = 1, X = x, w_{T=1} = \bar{w}) P(w_{T=1} = \bar{w} | T = 1, X = x)
\end{aligned}$$

Assuming that $\lim_{x \rightarrow 60^-} P(w_{T=1} = \bar{w} | T = 1, X = x) = P(w_{T=1} = \bar{w} | T = 1, X = 60)$, then:

$$\begin{aligned}
&= \left(P(Y | T = 1, X = 60, w_{T=1} = \bar{w}) - \lim_{x \rightarrow 60^-} P(Y | T = 1, X = x, w_{T=1} = \bar{w}) \right) \\
&\quad \times P(w_{T=1} = \bar{w} | T = 1, X = 60) \\
&= \tau_{\bar{w}} \times P(w_{T=1} = \bar{w} | T = 1, X = 60)
\end{aligned}$$

Continuity of the potential wage distribution at the threshold achieves identification of $\tau_{\bar{w}}$.

Finally, note that in the case the difference-in-discontinuity is warranted (that is, when a pre-period discontinuity is observed), then both the RDD and naive difference-in-discontinuity strategies on the joint distribution of labor supply and wages will fail to yield a consistent estimate of the conditional treatment effect.

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RÉSUMÉ

Cette thèse rassemble trois projets de recherches indépendants en économie appliquée du développement. Le premier étudie le comportement des secrétaires locaux du Parti Communiste chinois, et la performance économique de leurs préfectures. Il montre que le système politique chinois fournit à ses cadres politiques des incitations à encourager une forte croissance du PIB dans la perspective d'obtenir une promotion ; à travers des investissements dans les infrastructures et l'immobilier, ce qui suscite des expropriations de terres rurales. Le deuxième s'intéresse aux conséquences du choc économique mondial de 2008 sur les migrations internes en Chine, et montre que la stabilité des taux agrégés de migration cache une grande hétérogénéité de réactions selon les caractéristiques des individus. Le troisième évalue l'effet de la réforme du minimum vieillesse sud-africain entre 2008 et 2010 et montre qu'elle a eu un effet sur l'offre de travail des seniors concentré sur l'emploi informel.

MOTS CLÉS

Chine, Afrique du Sud, bureaucratie, migrations internes, retraites

ABSTRACT

This thesis is a collection of three independent research project in applied development economics. The first chapter studies the behavior of local Chinese Communist Party secretaries between 1996 and 2014, and the economic performance of their prefectures. It shows that the Chinese political system incentivizes local bureaucrats to generate high growth, and that this translates into higher real estate investment and higher expropriation rates of rural land. The second chapter looks at the consequences of the 2008 global financial crisis on the internal migration patterns in China. It shows that the apparent stability of aggregate migration rates hides substantial heterogeneity across individuals of different characteristics. The third chapter evaluates the impact of the non-contributory Old Age Pension scheme in South Africa. It finds that its substantial effect on the labor supply of the elderly is concentrated on informal employment.

KEYWORDS

China, South Africa, bureaucrat incentives, internal migration, pensions

